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# The American Historical Review

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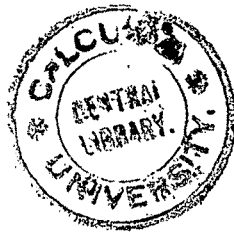
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# The American Historical Review

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THE AMERICAN HISTORICAL ASSOCIATION



*Cover Illustration.* Detail of the Thomas Kendel gravestone, ca. 1678, Wakefield, Massachusetts. Photograph from Allan I. Ludwig, *Graven Images: New England Stonecarving and Its Symbols, 1650-1815* (Middletown, Conn.: Wesleyan University Press, 1966). (See David E. Stannard, "Death and Dying in Puritan New England," pp. 1305-30.)

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# The American Historical Review

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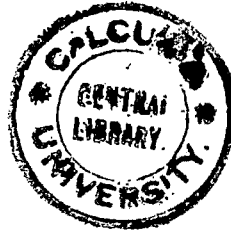
# The American Historical Review

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## The Encounter of Two Societies: Western Conquerors and Byzantines in the Peloponnesus after the Fourth Crusade

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DAVID JACOBY

THE SECOND HALF of the eleventh century witnessed the turning of the tide in the history of the eastern Mediterranean. The Norman conquest of Byzantine Italy and Muslim Sicily and the growing activity of Western merchants in the Byzantine Empire, the Muslim Levant, and Egypt provide two facets of the same phenomenon: together they may be considered as the initial thrust of a rejuvenated West, announcing its imminent military, economic, and demographic expansion eastward. The Crusades were part and parcel of this general phenomenon of Western or Latin expansion, yet they have made a particular imprint in its framework. They brought to Byzantine and Muslim territories the establishment of permanent Latin rule imposed by conquest, resting upon a Latin elite, and reinforced by immigration from the West. The initial phase of conquest is tied to the First Crusade, which enabled the creation of the Kingdom of Jerusalem and the Latin states of Tripoli, Antioch, and Edessa; Byzantine Cyprus was captured by Richard I of England almost by accident in 1191. A second wave of conquest came in the wake of the Fourth Crusade, when Western knights, the Venetian state, and several adventurers acting in their own behalf imposed their rule on Byzantine territories. Finally, Catalans conquered the duchy of Athens in 1311, and Genoese the island of Chios in 1346.

As a result of conquest relations between the Latins and the population of these areas underwent a major change. Commercial activity of Western merchants in Byzantium or Muslim countries in earlier years had called for purely economic and social intercourse with local inhabitants. Temporary or even permanent residence had no bearing on their position as aliens, a status that was further emphasized by their enjoyment of commercial and judicial privileges. But conquest, whether gradual or abrupt,

This is a thoroughly revised version of a paper read in Washington, D.C. on December 28, 1969, at the annual meeting of the American Historical Association. I wish to express my thanks to Professor Bryce Lyon, of Brown University, and Professor Charles M. Brand, of Bryn Mawr College, as well as to other commentators, who have prompted me to clarify or stress certain aspects of the problems I dealt with.

created a problem of a completely different nature: the relationship between an elite of conquerors, their descendants, and the Latins who joined them, on the one hand, and the indigenous population on the other, had to be defined and a pattern of permanent coexistence between the two groups devised. Feudalism transplanted from the West was imposed by a feudal nobility upon areas wrested from the Muslims. In former Byzantine territories the nature of the ruling class of conquerors was more diversified. Norman Italy, Cyprus, and the Peloponnesus witnessed the establishment of feudal rule, while elsewhere a nonfeudal elite wielded authority and held the reins of government. Such was the case in areas subjected to the direct dominion of Venice, especially Crete, as well as in the Catalan duchy of Athens and Genoese Chios. In each instance the very nature of the ruling class as well as the structure of the local society determined to a large extent the character of their encounter, which evidently generated changes in both groups. An investigation of this complex phenomenon in Frankish Morea might well serve as a case study.

ON APRIL 13, 1204, a combined force of Crusaders and Venetians captured Constantinople; the so-called Fourth Crusade had come to an end.<sup>1</sup> The disintegration of the Byzantine Empire, already initiated toward the end of the twelfth century, was accelerated, and within a year large portions of its territory were occupied by Latin conquerors. One of these areas, the Peloponnesus, or the Morea, was invaded in November 1204 by Western knights who gradually extended their rule over the whole peninsula, with the exception of Coron and Modon, which remained under Venetian rule. Although major changes occurred in its boundaries, the Frankish principality of the Morea remained in existence for more than two centuries, eventually disappearing in 1432.<sup>2</sup> The length of Latin rule and the relative wealth of evidence relating to this territory enable us to describe in a fairly accurate way the impact of feudalism on the indigenous population, especially on the Greek upper class, and the changes wrought by this encounter within the Frankish knightly class.

Before dealing with the evolution of society in Frankish Morea, it is,

<sup>1</sup> On the Fourth Crusade, see Edgar H. McNeal and Robert L. Wolff, "The Fourth Crusade," in Kenneth M. Setton, ed., *A History of the Crusades*, 2 (Philadelphia, 1962): 153-85; Antonio Carile, "Partitio Terrarum Imperii Romanie," *Studi Veneziani*, 7 (1965): 125-305; Charles M. Brand, *Byzantium Confronts the West, 1180-1204* (Cambridge, Mass., 1968), 232-69; and Donald E. Queller and S. J. Stratton, "A Century of Controversy on the Fourth Crusade," *Studies in Medieval and Renaissance History*, 6 (1969): 233-77.

<sup>2</sup> The most recent general surveys of Frankish rule in the Morea are by Jean Longnon, *L'empire latin de Constantinople et la principauté de Morée* (Paris, 1949), and by Antoine Bon, *La Morée franque. Recherches historiques, topographiques et archéologiques sur la principauté d'Achaïe (1205-1430)* (Paris, 1969). Unfortunately, many studies on specific subjects published in recent years have not been taken into account by the last author. William of Champlitte assumed the title of prince in 1205. Longnon, *L'empire latin*, 74.

however, imperative to dwell at some length upon the social structure of Byzantium. Despite basic common features, its social structure lacked uniformity: ethnic, economic, and geopolitical factors account for wide diversity within the Empire,<sup>3</sup> and therefore special emphasis should be placed, in our context, on its western provinces. Many important problems relating to the evolution of Byzantine society remain unsolved or open to controversy. This is partly so because evidence is not always available and also because scholars have traditionally focused their attention on fiscal and institutional history. On the other hand, the distinctive features of feudalism as it evolved in the areas of the West from which the Latin conquerors came are better known. Undoubtedly, striking differences existed between Byzantine and feudal societies. Nevertheless, their encounter in Frankish Morea is marked by adaptation, compromise, and integration, a process whose character, stages, and limitations require definition.

TWO OF THE MOST important features of Byzantium, especially when compared with feudal society in the West, are the continuous existence of the state and the nature of Byzantine law. These two closely connected factors exerted a powerful influence on the molding of Byzantine society and made their imprint upon the Byzantine mind. Byzantium inherited from Rome its bureaucratic centralization and an all-embracing administration as well as the basic principles of the political and legal tradition of the Greco-Roman world. These fundamental characteristics were upheld, although time and again the authority of the emperor was challenged and the Empire as a whole submitted to severe strains and stresses. Such was the case shortly before the Fourth Crusade.

The abstract concept of *politeia*, or *res publica*, implied the existence of public law and public taxation. The emperor, who personified the state and was the depository of political and judicial authority, was also the source of all grace and responsible for the implementation of the law.<sup>4</sup> On occasion he granted privileges to individuals, ecclesiastical institutions, or, collectively, to the inhabitants of a city or a territory. These privileges were mostly of a fiscal nature, such as exemption from

<sup>3</sup> See, for example, Speros Vryonis, Jr., "Byzantium: The Social Basis of Decline in the Eleventh Century," *Greek, Roman and Byzantine Studies*, 2 (1959): 161-62.

<sup>4</sup> See Franz Dölger, "Die Kaiserurkunde der Byzantiner als Ausdruck ihrer politischen Anschauungen," *Historische Zeitschrift*, 159 (1938-39): 229-50, reproduced in Dölger, *Byzanz und die europäische Staatenwelt* (Ettal, 1953), 9-33; Louis Bréhier, *Le monde byzantin*, 2, *Les institutions de l'empire byzantin* (Paris, 1949), 1-16, 63-65, 173, 218-19; Herbert Hunger, *Prooimion, Elemente der byzantinischen Kaiseridee in den Arengen der Urkunden* (Vienna, 1964), especially 84-154; Agostino Pertusi, "I principi fondamentali della concezione del potere a Bisanzio," *Bullettino dell'Istituto storico italiano per il medio evo e Archivio Muratoriano*, 80 (1968): 1-23; and Joachim Scharf, "Ius divinum. Aspekte und Perspektiven einer byzantinischen Zweigewaltentheorie," Peter Wirth, ed., *Polychronion, Festschrift Franz Dölger zum 75. Geburtstag* (Heidelberg, 1966), 462-79.

taxes due to the state or the right to collect taxes within the boundaries of an estate and retain them, in part or entirely, instead of passing them on to the imperial fisc. Even so, taxes retained their public character. When access to an estate was forbidden to imperial officers, its possessor or holder was evidently to replace those officers in the exercise of certain functions. Such a grant did not, however, entail the development of private jurisdiction; at best it is comparable to the immunity granted by rulers in the Merovingian and early Carolingian West. In the Empire the concession of administrative, fiscal, or limited judicial powers or exemptions did not amount to a definitive alienation of state prerogatives. The emperor never surrendered his supreme jurisdiction over the inhabitants of the Empire. The limited character both in time and scope of the grants, clearly expressed in the charters delivered, is further illustrated by their renewal at the request of the beneficiaries. The latter were always considered to be imperial agents, empowered by delegation to exert the authority of the state.<sup>5</sup> Nevertheless, when the control of the central government weakened, great landowners in the provinces took hold of the administrative machinery and usurped imperial powers. Such was especially the case in the last years of the twelfth century, shortly before the Frankish conquest.

Among the various grants awarded by the emperors, the *pronoia* has enjoyed particular attention. Literally "provision," it consisted of a concession of state revenues to an individual who collected them directly; to this effect, the emperor transferred to the recipient peasants and the imperial land they cultivated. The *pronoia* originated under the reign of the Comnenoi, toward the end of the eleventh or, more likely, in the early twelfth century, and became more widespread under Manuel I, who ascended the imperial throne in 1143. According to George Ostrogorsky, it was the counterpart of the Western fief; granted on a conditional basis in return for military service, it enabled the recruitment of mounted knights and became the basis of the Byzantine military system. Moreover, it strengthened the landed wealth and power of greater or lesser feudal lords at the expense of the state. The transfer of imperial authority over peasants to the recipients of *pronoiai* had important social

<sup>5</sup> On the nature of the grants, see George Ostrogorsky ("Pour l'histoire de l'immunité à Byzance," *Byzantion*, 28 [1958]: 165-97, 235-46), who has, however, overstressed their judicial aspects. A more balanced view is to be found in Anastasio I. Mouratides, "The Byzantine Immunity System" (Ph.D. dissertation, McGill University, 1965), i-xvi, 80-94, 166-223. See also Nicolas Oikonomides, "The Donation of Castles in the Last Quarter of the 11th Century," *Polychronion*, 413-17; Hélène Glykatzi-Ahrweiler, "La concession des droits incorporels; donations conditionnelles," *Actes du XII<sup>e</sup> Congrès international des Études byzantines* (Belgrade, 1964), 2: 103-14, and "Charisticariat et autres formes d'attribution de fondations pieuses aux X<sup>e</sup>-XI<sup>e</sup> siècles," *Recueil des travaux de l'Institut d'Études byzantines* (Belgrade); *Zbornik Radova*, 10 (1967): 1-5; see also Paul Lemerle, "Un aspect du rôle des monastères à Byzance: les monastères donnés à des laïcs, les charisticaires," *Académie des Inscriptions et Belles-Lettres, Comptes-rendus* (Jan.-Mar. 1967), 10.

implications and was a further symptom of the feudalization of Byzantium before 1204.<sup>6</sup> Considering all these factors, it is obvious that the *pronoia* is particularly relevant to our subject—a study of the interaction of Byzantine society with Western feudalism. The question is made the more pressing because twelfth-century sources relating to the *pronoia* are rather scanty, and Ostrogorsky has supplemented them by later evidence drawn from Frankish Morea, in order to clarify both the nature and diffusion of this institution.<sup>7</sup> This procedure casts heavy doubts, however, on the identification of the *pronoia* with the fief in the early thirteenth century.

The later evidence adduced by Ostrogorsky is to be found in the Greek version of the *Chronicle of Morea*, which was composed for a Greek audience in the Peloponnesus in the second half of the fourteenth century, most likely between 1341 and 1388, at least 137 years after the Frankish conquest. This chronicle provides ample information about holders of *pronoiai*, who fought on horseback and were followed by their dependents. *Pronoia* appears in it as identical to fief, and the Greek *archon*, as the counterpart of the Western knight; yet these equations should not be taken at face value. One cannot infer from this late-written source that the meanings of its terms were the same as they had been in the early thirteenth century, nor can one prove thereby the identity of institutions, such as *pronoia* and fief, at the time of the Frankish conquest. This is especially so because the Greek version derives from a French chronicle written for the Frankish feudatories and describing the life of the Moreot princes, barons, and their vassals, as well as their feats of arms, military obligations, mores, way of life, and outlook.<sup>8</sup> The numerous *pronoiai* mentioned in the Greek version are therefore mostly fiefs, and their holders were Frankish feudatories, not members of the Greek upper class.

A careful analysis of the feudal vocabulary used in the Greek version and of several articles of the *Assizes of Romania*, a legal treatise compiled in the Morea between 1333 and 1346, points to two important qual-

<sup>6</sup> See George Ostrogorsky, *Pour l'histoire de la féodalité byzantine* (Brussels, 1954), especially 9–61 for the period under consideration here; see also Ostrogorsky's *History of the Byzantine State* (New Brunswick, 1969), 330–31, 371–72, 392, 425. Answering criticism, the author has recently restated his basic views while slightly correcting them, in "Die Pronoia unter den Komnenen," *Recueil des travaux de l'Institut d'Études byzantines-Zbornik Radova*, 12 (1970): 41–54; see also his "Observations on the Aristocracy in Byzantium," *Dumbarton Oaks Papers*, 25 (1971): 11–12, 14, 17–18.

<sup>7</sup> As emphasized in Ostrogorsky, *Pour l'histoire*, 55, and *History*, 371 n. 2.

<sup>8</sup> On the Greek version, see David Jacoby, "Quelques considérations sur les versions de la *Chronique de Morée*," *Journal des Savants* (July–Sept. 1968), 150–59, 188. An English translation of this version has been published by Harold E. Lurier, *Crusaders as Conquerors: The Chronicle of Morea* (New York, 1964), but see the numerous corrections by Peter Topping, in a review published in *Speculum*, 40 (1965): 737–42. On the French version, see Jacoby, "Quelques considérations," 134–50, 181–87.

ifications.<sup>9</sup> In the first place, the Greek upper class in Frankish Morea did not hold any *pronoia*, in spite of the use of this term by the Greek version to describe their landed estates; second, the term is not used in a legal or technical way and stems anyhow from a late period. The *pronoia* gradually evolved in Byzantine territory into a hereditary tenure, and its military nature became more pronounced under the Palaeologoi, in the second half of the thirteenth century. Because it then resembled the fief the Greeks of the Morea were obviously prompted to use the term *pronoia* to describe the Western institution, and its adoption for this purpose should therefore be placed at that time or perhaps even in the early fourteenth century.<sup>10</sup>

In conclusion, neither backward projection nor generalization on the basis of the Greek version of the *Chronicle of Morea*, as propounded by Ostrogorsky, are warranted. Evidence on the hereditary *pronoia* appearing at a late period cannot be adduced to describe the institution at the time of the Frankish conquest. The same holds true of the portrayal of the mounted knights and their dependents by the Greek version: as Franks are described in this source, the latter provides no clue as to the true nature of the military service performed by the Greek holders of *pronoiai* prior to 1204.<sup>11</sup> Moreover, as the Greek version offers no conclusive evidence about the existence of the *pronoia* in the Peloponnesus at that date, one of the main arguments in favor of its wide diffusion in the whole of the Byzantine Empire has to be dismissed.<sup>12</sup>

Even though only meager evidence remains after the Greek version of the *Chronicle of Morea* has been rejected, it still appears that the *pronoia* of the twelfth century was basically different from the Western fief in several respects. It was granted only for the lifetime of the recipient and could be neither alienated nor enfeoffed, subinfeudation being non-existent in Byzantium. *Pronoiai* were not always conceded in return for military service, and there is no indication whatsoever that their holders constituted either an important factor in the Byzantine army or a military class.<sup>13</sup> Finally, it should be stressed that the concession of a

<sup>9</sup> For the dating of the *Assizes of Romania*, see my book *La féodalité en Grèce médiévale. Les "Assises de Romanie": sources, application et diffusion* (Paris, 1971), 75-82. The text of this treatise has been edited by Georges Recoura, *Les Assises de Romanie* (Bibliothèque de l'École des Hautes Etudes, fasc. 258) (Paris, 1930) (hereafter *Assises*). An English translation has been provided by Peter W. Topping, *Feudal Institutions as revealed in the Assizes of Romania, the Law Code of Frankish Greece* (Philadelphia, 1949), 15-99.

<sup>10</sup> Criticism of Ostrogorsky is to be found in my study "Les archontes grecs et la féodalité en Morée franque," *Travaux et Mémoires du Centre de recherche d'histoire et civilisation byzantines*, 2 (1967): 421-65. Ostrogorsky has obviously misread my article. See "Die Pronoia," 52-53. I have never claimed that the *pronoia* as an institution had been introduced in the Peloponnesus when it was under Frankish rule, but that such was the case with the *term* used to describe it.

<sup>11</sup> Contrary to the assertion of Ostrogorsky, *History*, 371 n. 2.

<sup>12</sup> In favor of a wide diffusion, see Ostrogorsky, *Pour l'histoire*, 55; cf. Jacoby, "Les archontes," 479-81, to the contrary.

<sup>13</sup> Contrary to Ostrogorsky ("Die Pronoia," 42-43), who has recently emphasized again the



*pronoia* did not entail a grant of public authority or jurisdiction over the peasants tilling its land.<sup>14</sup> The nature of political power and the distribution of authority in Byzantium before 1204 do not warrant the assumption that a process of feudalization was under way in the Empire, nor did the *pronoia* necessarily play a significant part in undermining its strength.<sup>15</sup>

Considerations about the social structure of Byzantium provide additional evidence that fundamental differences existed between twelfth-century Byzantium and feudal society in the native countries of the conquerors. Byzantine law and society inherited the clear-cut distinction between slaves and free men. Legally, all free men, whether relatives of the emperor, imperial officials, members of the Senate, or "commoners," were the *douloi*, or servants, of the ruler<sup>16</sup> and were governed by the same law system regardless of their social status or their relationship to him. On the whole, it was the aim of the emperor to ensure that all his subjects should enjoy the same treatment in law courts,<sup>17</sup> but in practice, judicial discrimination could not always be prevented: judges were often impressed by the social standing of the parties appearing before them, and the exertion of social and economic pressure or the use of bribery were evils that beset Byzantium as they did other medieval societies.<sup>18</sup> Still, as the nature of imperial grants shows, preferential treatment in law courts was not based on a legal concept of differentiation. Whether conceded to landlords, dignitaries, officials, or soldiers, titles and priv-

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exclusively military character of this institution, and to "Observations," 11 and 14, where the author implies the existence of contingents of holders of *pronoia*, for which no evidence can be adduced. I shall return elsewhere to this subject. On the composition of the Byzantine army in the late eleventh and twelfth centuries, see mainly Paul Lemerle, "Recherches sur le régime agraire à Byzance: la terre militaire à l'époque des Comnènes," *Cahiers de civilisation médiévale*, 2 (1959): 265-81; see also Hélène Ahrweiler (*Byzance et la mer* [Paris, 1966], 205-22, 397-407), who relies, however, too heavily on Ostrogorsky in relation to the diffusion of the *pronoia*.

<sup>14</sup> See above, n. 5. The presumed analogy of the *pronoia* with the Western fief has already been discarded by Miloš Mladenović, in "Zur Frage der Pronoia und des Feudalismus im byzantinischen Reiche," *Südost-Forschungen*, 15 (1956): 123-35, 137-39.

<sup>15</sup> Ostrogorsky ("Observations," 8-9) waters down the concept of feudalism by stating that its expression is "the presence of the seigneurial estate peopled by dependent peasants," which might apply to many countries and societies, even in antiquity. A penetrating analysis is offered by Claude Cahen. ("Réflexions sur l'usage du mot de 'féodalité'. A propos d'un livre récent," *Journal of the Economic and Social History of the Orient*, 3 [1960]: 4-20), who emphasizes the importance of the exercise of public authority and prerogatives by private persons as an essential ingredient of feudalism.

<sup>16</sup> Jean Verpeaux, "Les oikeioi. Notes d'histoire institutionnelle et sociale," *Revue des études byzantines*, 23 (1965): 96-97.

<sup>17</sup> See Bréhier, *Le monde byzantin*, 2: 173, 219. The same principle was restated at a later date, in 1334, by Emperor Andronicus III. Paul Lemerle, "Le juge général des Grecs et la réforme judiciaire d'Andronic III," *Mémorial Louis Petit, Mélanges d'archéologie et d'histoire byzantines* (Bucharest, 1948), 299-300.

<sup>18</sup> In case of abuse one could seek redress from the emperor. A suggestive study on the discrimination against the poor in contemporary American society is provided by Jerome E. Carlin, Jan Howard, and Sheldon L. Messinger, "Civil Justice and the Poor: Issues for Sociological Research," *Law and Society Review*, 1 (1966): 9-90, also published separately (New York, 1967).

ileges were always bestowed on an individual basis and never collectively upon a specific group enjoying the same social standing.<sup>19</sup> They provided no exemption from regular judicial procedure for individuals, nor did they give rise to a class endowed with a particular legal status or governed by a particular set of laws.

Legal equality in Byzantium should not be mistaken for social or economic equality, nor did it necessarily afford equal chances of promotion in society.<sup>20</sup> High-ranking birth was often emphasized; the "well born" or children of "well-born parents" were favored at the outset<sup>21</sup> and upstarts utterly despised.<sup>22</sup> Definitions of class boundaries remained vague, however. Terms such as "powerful" (*dynatoi*) or "poor" (*aporoï*, *ptochoi*) implied mainly a social and, additionally, an economic standing, not necessarily identical.<sup>23</sup> Yet high-ranking birth or differences in social status, economic status, power, or prestige never assumed a legal character and were never sanctioned or perpetuated by law.<sup>24</sup> This fundamental characteristic of Byzantine society stands out in sharp contrast to feudal society as it evolved in the native countries of the conquerors: in the feudal West of the late twelfth century, social classes were synonymous with legal classes, and status had become hereditary. To be sure, aristocracy existed in Byzantium as a social class, and certain families belonging to this group played a dominant role in the evolution of the Empire,<sup>25</sup> but no hereditary nobility enjoying a particular legal status is to be found.<sup>26</sup> Moreover, the absence of clearly defined legal classes<sup>27</sup> or a rigid system of stratification enabled a constant trend of upward mobility and inclusion within the elite of the Empire. To some extent Byzantine society

<sup>19</sup> The individual character of the privileges should be stressed in our context. See Jacoby, "Les archontes," 466-67, especially nn. 231, 236.

<sup>20</sup> This is especially the case for the *paroikoi*, who by the late twelfth century formed the major part of the Byzantine peasantry; although legally free, they were subjected to severe personal restrictions. I shall deal elsewhere with this subject.

<sup>21</sup> See Romilly J. H. Jenkins, "Social Life in the Byzantine Empire," *Cambridge Medieval History*, vol. 2, pt. 2, ed. Joan M. Hussey (Cambridge, 1967), 99-100, and below, n. 25; see also Ostrogorsky, "Observations," 4-5.

<sup>22</sup> See, for example, Brand, *Byzantium Confronts the West*, 143.

<sup>23</sup> See especially Paul Lemerle, "Esquisse pour une histoire agraire de Byzance: les sources et les problèmes," *Revue historique*, 219 (1958): 268-80.

<sup>24</sup> Evidence to this effect is found in Monemvasia, in the southern Peloponnesus. See Jacoby, "Les archontes," 466 n. 231.

<sup>25</sup> See, for example, Vryonis, "Byzantium," 161-64; see also Nicolas Svoronos, "Société et organisation intérieure dans l'empire byzantin au XI<sup>e</sup> siècle: les principaux problèmes," *Thirteenth International Congress of Byzantine Studies* (Oxford, 1966), Main Papers, 12: 1-7.

<sup>26</sup> See Rodolphe Guiland, "La noblesse de race à Byzance," *Byzantinoslavica*, 9 (1947-48): 307-14; Guiland, "La transmission héréditaire des titres nobiliaires à Byzance," *Kodaiigaku (Palaeologia)*, 8 (1959): 137-43; and Guiland, "La noblesse byzantine. Remarques," *Revue des études byzantines*, 24 (1966): 40-42, 49-52. The first two studies have been reprinted in Rodolphe Guiland, *Recherches sur les institutions byzantines*, 1 (Berlin, 1967), respectively 15-22 and 65-72. See also the important remarks by Franz Dölger, in his review of Philip P. Argenti, *Libro d'Oro de la noblesse de Chio* (Oxford, 1955), published in *Byzantinische Zeitschrift*, 49 (1956): 126-27.

<sup>27</sup> With the exception of slaves and *paroikoi*. See above, n. 20.

was an "open" society and remained so toward the end of the twelfth century. One can find many instances in which *homines novi*, even of lowly origin, gained access to the upper class;<sup>28</sup> several of them were to ascend the imperial throne. Inclusion within the elite of the Empire was not necessarily attained in the wake of dynastic changes. Education and intellectual achievement were highly valued in Byzantium;<sup>29</sup> so were administrative efficiency and military valor. The imperial bureaucracy, the army, and serving powerful men<sup>30</sup> provided avenues for social promotion and consequently afforded chances to acquire land and power, sometimes on a large scale.<sup>31</sup> The progressive development of a class of rich townspeople, beginning toward the end of the tenth century, added a new factor of mobility. From about the middle of the eleventh century, emperors appointed several of them to the Senate and to high imperial offices.<sup>32</sup>

Powerful men, dignitaries, and great landowners occasionally had armed retainers; great landowners also exerted some authority over the free peasants tilling their lands, especially when these landowners were vested with certain fiscal or administrative powers granted to them by the emperor. Personal bonds of dependence, however, always retained their private nature and were never recognized by law or sanctioned by custom.<sup>33</sup> Vassalage as it was known in the West was alien to the Byzantine political structure and to Byzantine thought. Obviously, contact with Latins, even before the First Crusade, had acquainted Byzantium with feudal institutions, although the encounter did not prepare the ground for their acceptance in the Empire or change the relationship of the emperor with his subjects. Significantly, vassalage appeared in Byzantium only when the emperor established such a relationship with Latins, which

<sup>28</sup> Such as Philokales, formerly a peasant, mentioned in a novel of Emperor Basil II dated 996. See Lemerle, "Esquisse," 277-78. An English translation of this text is provided by Charles M. Brand, *Icon and Minaret: Sources on Byzantine and Islamic Civilization* (Englewood Cliffs, N. J., 1969), 91-97, and see especially 93. For the twelfth century, see the example mentioned in Brand, *Byzantium Confronts the West*, 143. Many eunuchs were of lowly origin; on their importance, see Rodolphe Guiland, "Les eunuques dans l'empire byzantin," in *Recherches*, 1: 165-97. See also below, n. 30. Ostrogorsky ("Observations," 4-5, 9, 29) insists upon the existence of a hereditary aristocracy, but the evidence adduced throughout his study is not convincing and, besides, points to upward mobility into the upper class. A thorough study of this phenomenon is still wanting.

<sup>29</sup> On which see Jenkins, "Social Life," 80; Ostrogorsky, "Observations," 29-30; and Paul Lemerle, *Le premier humanisme byzantin* (Paris, 1971), 255-56.

<sup>30</sup> See especially Hans-Georg Beck, "Byzantinisches Gefolgschaftswesen," in *Bayerische Akademie der Wissenschaften, Philosophisch-historische Klasse, Sitzungsberichte, Jahrgang 1965* (Munich, 1965), 5: 1-31; Verpeaux ("Les oikeioi," 89-99), who also deals with the period following the Fourth Crusade; and Ostrogorsky, "Observations," 12-16.

<sup>31</sup> The successful career of an intellectual of lowly origin is described by Nicoletta Dujé, "Un haut fonctionnaire byzantin du XI<sup>e</sup> siècle: Basile Malésès," *Revue des études byzantines*, 30 (1972): 167-78.

<sup>32</sup> See Svoronos, "Société et organisation intérieure," 8-10.

<sup>33</sup> See above, n. 30. Ostrogorsky ("Observations," 14-15) agrees to this fact, but nevertheless arrives at the surprising conclusion that dependents occasionally were not directly subject to the emperor.

explains the exclusive use for this purpose of Western feudal terminology, transliterated or translated into Greek.<sup>34</sup>

Great landowners played a dominant role in the provinces of the Empire. Their estates—a source of wealth, prestige, and power—assured them of leadership on a regional or local scale and of support by their dependents, whether these were inhabitants of small cities or small landholders.<sup>35</sup> Yet many great landowners strove to enhance their social status and strengthen their influence by acquiring administrative or military functions within the imperial machinery of government. Honorary titles, which assured a standing in the imperial hierarchy, were eagerly sought and, if not granted, bought from the emperor.<sup>36</sup> It is therefore not surprising that the same word, *archon*, applied to great provincial landowners as well as to imperial officials, the latter vested with administrative or military authority. One occasionally would make a distinction between the rich landlord, or *ktematikos archon*, and the official in charge of civilian administration or the army officer, known respectively as *thematikos* and *tagmatikos archon*.<sup>37</sup> Although many *archontes* derived their social status from rural estates, those of the Western provinces of Byzantium seem on the whole to have resided in cities.<sup>38</sup> As some of these cities served as administrative and military centers, it is not surprising that they afforded a powerful asset when *archontes* took advantage of the weakness and eventual collapse of imperial authority on the eve of the Latin conquest. Several such cases in the Peloponnesus are also illustrative of the dual position of many *archontes* as great landowners and imperial officials, a position fully exploited to foster political ambitions.

Among these *archontes* Leo Sgouros achieved the most spectacular career. In Nauplia he inherited the powerful standing of his father, who had been the virtual ruler of the city. He appears to have been an imperial officer in 1198 and seems to have borne an official title in 1203.

<sup>34</sup> See Jacoby, "Les archontes," 440, 476, and Verpeaux, "Les oikeioi," 93-94. Bohemund of Antioch uses Western terminology to express the relationship of the princes of Lesser Armenia to Emperor Alexius I. Anne Comnène, *Alexiade*, ed. Bernard Leib (Paris, 1945), 3: 134 (ll. 1-4).

<sup>35</sup> For the eastern provinces, see above, n. 25. In the Peloponnesus this is apparent in Monemvasia and in the case of Sgouros, on whom see below.

<sup>36</sup> For the tenth to twelfth centuries, see Paul Lemerle, "Roga et rente d'état aux X<sup>e</sup>-XI<sup>e</sup> siècles," *Revue des études byzantines*, 25 (1967): 77-100; Armin Hohlweg, *Beiträge zur Verwaltungsgeschichte des oströmischen Reiches unter den Komnenen* (Munich, 1965), 34-40.

<sup>37</sup> See Jacoby, "Les archontes," 465-66, and below, on Sgouros and Chamaretos; see also Hélène Glykatzī-Ahrweiler, "Recherches sur l'administration de l'empire byzantin aux IX<sup>e</sup>-XI<sup>e</sup> siècles," *Bulletin de correspondance hellénique*, 84 (1960): 83 n. 4.

<sup>38</sup> Nicolas G. Svoronos, "Recherches sur le cadastre byzantin et la fiscalité aux XI<sup>e</sup> et XII<sup>e</sup> siècles: le cadastre de Thèbes," *Bulletin de correspondance hellénique*, 83 (1959): 67-77, 141-44; Henri de Valenciennes, *Histoire de l'empereur Henri de Constantinople*, ed. Jean Longnon (Paris, 1948), par. 672, mentions the welcome of *papas*, *alcontes*, and other inhabitants of Thebes to the Latin emperor Henry in 1209. On the *kastrenoi* of Athens toward the end of the twelfth century and the *archontes* of the Peloponnesus, see Jacoby, "Les archontes," 427-28, 466-67, and below.

From Nauplia he extended his power in 1202 by seizing Argos and Corinth and allied himself with pirates of Aegina and Salamis in order to subdue Athens, but to no effect. He successfully assaulted Thebes and advanced in Thessaly in 1204-05, his power finally collapsing when he faced the Frankish invaders. Another *archon*, Leo Chamaretos, who bore the official title of *proedros* of Lacedaemonia, in 1205 managed briefly to impose his authority on the area surrounding Sparta.<sup>39</sup> At about the same time an *archon* of the region of Modon, in the southwestern Peloponnesus—his identity and exact standing cannot be ascertained—<sup>40</sup> allied himself with Geoffrey of Villehardouin, one of the leaders of the Franks, hoping to conquer the peninsula with him. The position of the Slav *archontes* was somewhat different. Several groups of Slav population who settled in Byzantine territory from the late sixth to the early ninth century preserved their particular features and their tribal structure—and this was especially the case in the Peloponnesus. The imperial government included them within the framework of its military organization and, in return for military service, granted them fiscal privileges. Moreover, the emperor recognized the authority and strengthened the traditional status of their chiefs by conferring on them imperial titles, and it is therefore not surprising that they were considered *archontes*.<sup>41</sup>

As leaders of the local population at the time of the Frankish conquest, the *archontes* of the Peloponnesus headed the struggle against the invaders. Some *archontes* perished, others fled;<sup>42</sup> most of them came to terms with the Latin invaders or were eventually compelled to accept their rule. A major change was to take place in their status.

THE FRANKISH ARMY that conquered the Peloponnesus included knights from widely scattered areas in the West, extending from Flanders to Provence, though most of them came from Champagne and Burgundy.<sup>43</sup> Despite differences in their backgrounds they brought with them institutions, traditions, attitudes, and values common to feudal society in the

<sup>39</sup> See Jacoby, "Les archontes," 466-67; Brand, *Byzantium Confronts the West*, 152-54, 244-45; Antoine Bon, *Le Péloponnèse byzantin jusqu'en 1204* (Paris, 1951), 123, 172-73, 204-05, nos. 67 and 68.

<sup>40</sup> Geoffroy de Villehardouin, *La conquête de Constantinople*, ed. Edmond Faral (Paris, 1938-39), 2: 134-36, pars. 325-26. Karl Hopf (*Geschichte Griechenlands vom Beginn des Mittelalters bis auf unsere Zeit*, *Ersch-Gruber Encyklopädie*, 85 [Leipzig, 1867-68], 212 b) identified him with a member of the Kantakouzenos family, but this is without foundation. See Donald M. Nicol, *The Byzantine Family of Kantakouzenos (Cantacuzenus), ca. 1100-1460, A Genealogical and Prosopographical Study* (Dumbarton Oaks Studies, 11) (Washington, 1968), 7 n. 15.

<sup>41</sup> This is obvious in the case of the Melings living in Laconia. See Jacoby, "Les archontes," 465-66, also 443-44; in addition see Hélène Ahrweiler, "Le sébaste, chef de groupes ethniques," *Polychronion*, 34-38, especially 36-37.

<sup>42</sup> Such a case is mentioned in a letter by Michael Choniates. Spyridon Lampros, *Michael Akommatou tou Choniatou ta sozomena* (Athens, 1879-80), 2: 276-80 (in Greek).

<sup>43</sup> Jean Longnon, "Problèmes de l'histoire de la principauté de Morée," *Journal des Savants* (Apr.-June 1946), 86-87.

northern part of the Capetian kingdom and neighboring areas, such as the county of Burgundy, toward the end of the twelfth century.<sup>44</sup>

Feudal society in these territories was by then strictly stratified, basic social status being synonymous with legal status and, most important, transmitted by heredity. Each class was governed by a particular legal system. Since the eleventh century there were, to be sure, constant streams of mobility within the various social classes, which lacked both social and economic homogeneity. But, whether vertical or horizontal, this mobility had no bearing on the legal status of an individual. Non-nobles serving on a permanent basis in the administration or as horsemen in the armies of kings, dukes, counts, and barons thereby could enhance their social and economic position and enjoy special prestige, although they were prevented by a legal barrier from joining the ranks of the nobility. Neither did the holding of a feudal tenement by *ministeriales* or serjeants, attested to in the twelfth century, entail any change in their personal status.<sup>45</sup> Social promotion involving the crossing of class boundaries was by then essentially restricted to the lower strata of society.<sup>46</sup> Servile peasants became free by moving to new lands or by settling in towns; townspeople acquired personal freedom in different ways. The change in their social and legal status implied a legal procedure or some formal act to this effect, such as the grant of an individual, or collective, charter by a feudal lord or, in the case of a peasant who had settled in a town, testimony as to the length of his stay there. Legal procedure of this nature was especially imperative in order to gain promotion to the upper class of feudal society. The development of class-consciousness within the ranks of the feudatories, illustrated by the ceremony of dubbing to knighthood, made access to the latter status most difficult, although not impossible. By the late eleventh century nobility was already considered a matter of blood, and by 1200 chivalry had become an order, with its specific ritual, morals, and obligations.<sup>47</sup> Nevertheless, one could still acquire noble status by being knighted. It is true

<sup>44</sup> On this matter and on the discussion that follows, see especially Marc Bloch, *La société féodale* (Paris, 1939-40), whose views have been corrected on several points; Georges Duby, *La société aux XI<sup>e</sup> et XII<sup>e</sup> siècles dans la région mâconnaise* (Paris, 1953), and, by the same author, "Une enquête à poursuivre: la noblesse dans la France médiévale," *Revue historique*, 226 (1961): 1-22; Jean Richard, *Les ducs de Bourgogne et la formation du duché du XI<sup>e</sup> au XIV<sup>e</sup> siècle* (Dijon, 1954); and Robert Boutruche, *Seigneurie et féodalité*, 2: *L'Apogée (XI<sup>e</sup>-XIII<sup>e</sup> siècles)* (Paris, 1970). The county of Burgundy was included in the Empire, but family ties and constant contacts account for an evolution of feudalism close to that of the duchy of Burgundy. See Richard, *Les ducs de Bourgogne*, and Jean-Yves Mariotte, *Le comté de Bourgogne sous les Hohenstaufen, 1156-1208* (Paris, 1963).

<sup>45</sup> See Duby, *La société*, 241, 381-96, 436-37; Boutruche, *Seigneurie et féodalité*, 2: 285-87.

<sup>46</sup> On the other hand, downward mobility is frequently attested for members of the nobility. See Duby, *La société*, 241-43, 411-18, and Edouard Perroy, "Social Mobility among the French Noblesse in the Later Middle Ages," *Past and Present*, no. 21 (Apr. 1962): 25-38.

<sup>47</sup> See the full discussion of the problem by Duby, *La société*, 230-45, 411-18, 630-33; see also Duby, "Les origines de la chevalerie," in *Ordinamenti militari in Occidente nell'alto medioevo* (Settimane di studio del Centro italiano di studi sull' Alto Medioevo, XV, 2) (Spoleto, 1968), 739-61.

that the privilege of granting knighthood tended to become restricted to a few and that the number of beneficiaries remained on the whole small. But this restriction belongs to the first half of the thirteenth century and grows out of the sharp economic rise of certain burgesses as well as the constant progress of royal power in Capetian France, both of which had been initiated in the preceding century.<sup>48</sup>

The impersonal concept of state remained alien to the mind of feudal society. Personal bonds, of a private nature, provided the backbone of social and political hierarchy; they were sanctioned by custom, governed by strict rules, and had legal force. The organization of society was reflected in institutions and practices such as homage, fealty, and fief. Knights formed the dominant class of society, but wide differences in standing, power, and wealth can be perceived within their ranks. Judicial and legislative authority as well as the right of taxation were vested in private hands and exercised by the upper ranks of feudal lords, although, as a result of centralization, the kings of France and the various counts and dukes also exercised public authority in these matters. Most knights lived on their own rural estates or in the court of a rural lord. Warriors by vocation, they strictly refrained from any direct involvement in economic activity, whether in agriculture or in commerce. Although this attitude was an integral part of their social ethos, it did not prevent them from occasionally pursuing a conscious economic policy implemented by their servants or subjects. In the twelfth century knights in the areas from which the conquerors came normally enjoyed exemption from regular taxes or payments of a rent in money; commutation of military service, relief, aids, and general levies were not, of course, considered as such.<sup>49</sup> The very style of life of the knights reflected their values and mentality. Feudal vocabulary itself faithfully expressed their sense of social superiority and their attitude toward the inferior classes. As the occasional performance of homage by non-nobles suggests, the ethos and concepts of the nobility pervaded the whole of society.<sup>50</sup>

IT COULD HARDLY be expected that feudalism, with its particular social structure, political organization, and institutions, would be transplanted from the West to the Peloponnesus without undergoing any changes. While retaining its essential characteristics, it had to adapt to new cir-

<sup>48</sup> See Duby, *La société*, 522-23, 579-81, 623, 629, 634-36, and Boutruche, *Seigneurie et féodalité*, 2: 284-89; see also Perroy, "Social Mobility," 25-38, but the evidence adduced from the Forez does not necessarily warrant generalization.

<sup>49</sup> Some small fiefs were, however, liable to periodic payments and services instead of military service, but this was rather exceptional. See Boutruche, *Seigneurie et féodalité*, 2: 202-03.

<sup>50</sup> On this last point, see Georges Duby, "La féodalité? Une mentalité médiévale," *Annales: Économies, Sociétés, Civilisations*, 13 (1958): 765-71; Duby, "The Diffusion of Cultural Patterns in Feudal Society," *Past and Present*, no. 39 (Apr. 1968), 3-10; see also Kenneth-John Hollyman, *Le développement du vocabulaire féodal en France pendant le haut moyen âge* (Geneva, 1957); Boutruche, *Seigneurie et féodalité*, 2: 164-65, 170-72, 287; and Jacoby, "Les archontes," 463.

cumstances, to make compromises and adjustments. Gradations of power and rank within the conquering host were inevitably reflected in Morea. In addition the Franks had to take into account a virtually permanent state of war, first during the period of conquest, and again, from 1262 on, when Byzantium sought to expand her rule in the Peloponnesus after regaining a foothold in the peninsula.<sup>51</sup> Finally, the existing structure of Byzantine society was to exert an influence on the class of Frankish knights.

In their native countries the knights who conquered the Morea had with few exceptions belonged to the intermediate and lower ranks of the nobility. One discovers among them a small group of leaders adorned with special prestige. They in turn were followed by knights and serjeants, many of whom had already been their vassals and dependents in the West, while others had joined them in the course of the Crusade or the conquest of the Peloponnesus. The initial gap between leaders and vassals persisted and even widened when the conquerors settled in the Morea. For lack of land only a small number of baronies was created, most of them consisting of four to eight fiefs.<sup>52</sup> Enfeoffment of knights and mounted serjeants was necessarily restricted by the prince and the barons, a relatively large demesne being essential for the preservation of their political, social, and economic ascendancy. Grants of land did not exceed one-third of their fiefs,<sup>53</sup> a rule imposed on petty feudatories to ensure the performance of the military service to which they were liable.<sup>54</sup> It is therefore not surprising that many knights held only one fief or part of a fief and mounted serjeants half a fief or even less;<sup>55</sup> grants were occasionally restricted to the lifetime of the recipient

<sup>51</sup> This situation accounts for many of the rules governing the fief and military service in the Morea. See Jacoby, "Les archontes," 434-35, 447-51, 459-60, and "La féodalité," 54-55; also below, about the *archontes*. Yet relations between Byzantium and Frankish Morea were peaceful for long periods. Jacoby, "Un régime de coseigneurie gréco-franque en Morée: les 'casaux de parçon,'" *Mélanges d'Archéologie et d'Histoire publiés par l'École Française de Rome*, 75 (1963): 111-25.

<sup>52</sup> For all this, see my study "La Morée franque. Bilan et perspectives de recherches," to appear in *Le Moyen Âge*.

<sup>53</sup> As illustrated by the grant by Prince William II of eight fiefs, out of twenty-four belonging to the barony of Akova, to Marguerite of Passavant. See the French version of the *Chronicle of Morea*, ed. Jean Longnon, *Livre de la conquête de la principauté de l'Amorée, Chronique de Morée (1204-1305)* (Paris, 1911) (hereafter *French chronicle*), pars. 525-27, 531.

<sup>54</sup> *Assises* (see above, n. 9, in fine), arts. 30, 46, 72; see also art. 107 and Jacoby, "Les archontes," 450.

<sup>55</sup> After enumerating the baronies, the Greek version of the *Chronicle of Morea* states: "The knights who had one fief each and also the serjeants who were enfeoffed I do not name because of the amount of writing it would require." *The Chronicle of Morea, To chronikon tou Moreos*, ed. John Schmitt (London, 1904) (hereafter *Greek chronicle*), v. 1965-67. For parts of fiefs, see above and previous note; also *Assises*, arts. 57, 89, and the examples provided below. Mounted serjeants as a rule held a serjeanty, a feudal tenement providing an income worth half that of a fief liable to a knight's service. Jacoby, "Les archontes," 449. Serjeants enfeoffed with part of a serjeanty are mentioned in the Aragonese version of the *Chronicle of Morea*, ed. Alfred Morel-Fatio, *Libro de los fechos et conquistas del principado de la Morea* (Geneva, 1883) (hereafter *Aragonese chronicle*), pars. 133, 139.



or of the donor.<sup>56</sup> The existence of money fiefs and of household knights further emphasizes the strong position of the prince and the barons on the one hand and on the other, the precarious standing of most members of the knightly class and of the serjeants.<sup>57</sup>

All these factors account for the particular structure of the feudal hierarchy in Frankish Morea. It had only three ranks below the prince, who headed the pyramid. Next to him came his direct vassals, whether lieges or feudatories of simple homage; among the lieges the barons of the principality enjoyed a special position as his tenants-in-chief and were referred to as "peers of the prince."<sup>58</sup> In turn, all the lieges of this first rank could have vassals of their own, and such was also the case with lieges of the latter rank.<sup>59</sup> The rite of homage performed was directly related to social and legal status; all noble feudatories did liege homage whereas non-noble serjeants did plain homage, a difference which accounts for the fact that the latter could have no vassals of their own.<sup>60</sup> Differences in rights and prerogatives between barons, other lieges, and feudatories of simple homage are illustrated in the field of jurisdiction.<sup>61</sup> Like the prince, barons had rights of low and high justice, the latter including criminal jurisdiction, whereas the other lieges enjoyed only the exercise of low justice.<sup>62</sup> As for the feudatories of simple homage, they did not participate in formal court gatherings convened by their lords, had no court of their own, and their judicial competence was restricted to civil cases of their dependent peasants, the *villani*, or vil-

<sup>56</sup> *Assises*, art. 98; Jacoby, "Les archontes," 451, 459.

<sup>57</sup> On money fiefs, see Jacoby, *La féodalité*, 135-36, 186, 224; household knights are mentioned below.

<sup>58</sup> On the barons as peers of the prince, see *Assises*, art. 43 (first part); as lieges of the prince, see *Assises*, arts. 2, 48, where the translation should read: "the liegemen (i.e., laymen) and prelates who have a barony"; see also arts. 22, 49. The most recent treatment of liege homage is by Boutruche, *Seigneurie et féodalité*, 2: 162-70.

<sup>59</sup> *Assises*, art. 12, on feudatories of a baron of a "chavalier"; this last term is synonymous with liege, as illustrated by the enumerations in art. 14 ("baron, cavalier o feudatario"), art. 2 ("baroni et homeni ligii . . . et li altri afeudadi"), and arts. 49 ("baroni et altri cavalieri e feudatarii ligii"), 207, 216, barons and lieges have their own lieges; see also art. 28, where the first "feudatario" (l. 2) is a liege, and art. 216. In art. 94 "baron" is synonymous with liege of the prince.

<sup>60</sup> "Que todos los varones et los cavalleros et los nobles escuderos fiziessen omenage de ligious, et los otros escuderos et nobles Griegos fizieron omenage de plano." *Aragonese chronicle*, par. 137. The noble squires mentioned in the first category were of noble descent, but had not yet been dubbed, as we shall also see below. The non-noble squires were serjeants, to whom the "noble Greeks" were assimilated. This process also will be examined below. The rite of liege homage is described in *Assises*, arts. 3, 68. The feudatories of simple homage had no court of their own and therefore no vassals. Lieges were entitled to hold additional fiefs by simple homage (*Assises*, arts. 34, 68) or several fiefs from different liege lords (arts. 46, 63, 99).

<sup>61</sup> See the classification in *Assises*, art. 1 (where *fedeli* is identical to *afeudadi* in art. 2); see also arts. 2 (cited above, n. 59), and 143 ("de la corte de algun baron, de legio o de feudatario"). On judicial prerogatives, see *Assises*, arts. 42, 43, 48, 49, 72, 94, 143, 151, 162, 186, 203, 207.

<sup>62</sup> In the feudal West high justice included criminal cases involving the death penalty or mutilation as well as important civil cases relating to the legal status of persons and property. Low justice was exercised in criminal as well as civil cases of minor importance, concerning rents and dues for land, for example.

lains.<sup>63</sup> It is apparent, then, that social differentiation within the Frankish elite was most pronounced. The power of the barons, their extensive prerogatives, which included the right to erect castles,<sup>64</sup> their style of life, in the midst of their vassals and dependents, all these singled them out as an exclusive group.<sup>65</sup> The cleavage between the vassals of simple homage, the lowest stratum of the feudal hierarchy, and other feudatories was no less marked. Although feudatories, they were not members of the knightly class, a fact that proved to be of particular importance to the evolution of Moreot society and that accounts partly for the attitude adopted by the Franks toward the Greek upper class.<sup>66</sup>

Despite vast differences within their ranks, the Frankish knights displayed common values and attitudes, which were shared by the Western knights who joined them in the Morea in the second half of the thirteenth and in the fourteenth centuries.<sup>67</sup> They were imbued with a powerful class-consciousness that had matured in the native countries of the conquerors and was further enhanced by the successful conquest and conditions prevailing in the Morea. Their numerical inferiority in the midst of a large Greek population was especially obvious in isolated castles and rural mansions; nevertheless, they manifested no fear of being submerged. Confidence in their own survival as a ruling class was inspired by a genuine sense of social and military superiority, most pronounced in the baronial group. This sense of superiority is reflected in the matrimonial policy pursued by the knights: many of them married daughters of noble families in France and subsequently brought their wives to the Morea; others wedded Latin women from various areas of the West or the Morea, while intermarriage with Greeks is seldom attested.<sup>68</sup> Their relative seclusion and particular style of life afforded additional guarantees of social continuity. Most of them lived in the repaired or enlarged acropolis of a city, an isolated mountain-castle or a fortified rural mansion, apart from Greek society and its economic activity, but this did not prevent them from residing occasionally in the houses they held in cities.<sup>69</sup> Festive

<sup>63</sup> See especially *Assises*, arts. 72, 42. At the end of art. 72 it is stated that "a liege man has a court while a man of simple homage cannot have one." See also Jacoby, "Les archontes," 468-69.

<sup>64</sup> *Assises*, art. 94.

<sup>65</sup> In 1304 the powerful Nicholas III of Saint-Omer insisted that Vincent de Marais, vassal and counselor of Prince Philip of Savoy, should leave the princely court gathering, for "he was not worthy nor a man [of sufficient standing]" to take part in judicial proceedings involving a daughter of Prince William II and a baron. *French chronicle*, pars. 954-72, especially par. 961. See also Jacoby, *La féodalité*, 46-47.

<sup>66</sup> This attitude will be examined below.

<sup>67</sup> On later arrivals, see Jacoby, *La féodalité*, 82-83, 85-86, and Bon, *La Morée*, 168-69, 238-40.

<sup>68</sup> *Crònica de Ramon Muntaner*, ed. E. B. (Barcelona, 1927-51), chs. 244, 261, in fine; *Assises*, art. 111 ("the prince or another lord cannot deny permission to his vassal to leave the principality . . . if he wishes to contract a marriage," obviously in the West); see also Jacoby, *La féodalité*, 41-42. Intermarriage with Greeks is attested, and for the princes of the Morea was obviously prompted by political considerations.

<sup>69</sup> A most suggestive description of building activity is to be found in *French chronicle*. "Li baron dou pays et li autre gentil homme si comencèrent a faire fortresses et habitacions, qu'y chastel, qu'y maisons sur sa terre, et changier leurs sournoms et prendre les noms des fortresses

meetings, tournaments, such as the one held in 1304 in Corinth,<sup>70</sup> the historical literature and romances in French read aloud at court gatherings and circulated in the Morea,<sup>71</sup> the frescoes painted on the walls of the castle of Saint-Omer in Thebes, "depicting how the Franks conquered Syria," or in the mansion of the archbishop of Patras, "representing the story of the destruction of Troy,"<sup>72</sup> all these afford a glimpse of the life, values, and outlook of the knightly class of the Morea.

THE MOST OBVIOUS RESULT of the conquest was the abolition of Byzantine imperial rule and administration as well as the replacement of the independent local *archontes* by new masters. The imposition of a group of foreign conquerors on the indigenous population heralded the introduction of a new system of government, with corresponding institutions and a change in the structure of society, both in conformity with feudal concepts and principles. The new social and political regime is clearly reflected in the realm of jurisdiction and taxation, the foremost expressions of political authority; previously vested in the emperor and usurped for a few years by the local *archontes*, they now became the exclusive prerogative of feudal lords. Moreover, the whole structure of society underwent a metamorphosis. It was now divided into two distinct groups: on the one hand, the Latin conquerors and Western immigrants who joined them; on the other, the indigenous Greeks and Slavs. Although religious differences did not constitute a primary factor in the relations between the two groups, they nevertheless became a criterion of basic social status and provided a convenient means of social identification. Latins or Franks, by definition free,<sup>73</sup> were those who recognized the sole authority of Rome in religious and ecclesiastical matters. Although formally subjected to the pope in these fields,<sup>74</sup> the Greeks re-

qu'il faisoient" (pars. 218-19). On the architectural features of these Frankish buildings, see Bon, *La Morée*, 601-84, especially 680; on the social aspect, see Ernst Kirsten, "Die byzantinische Stadt," *Berichte zum XI. Internationalen Byzantinisten-Kongress* (Munich, 1958), 33-34, 39-40. On Frankish feudatories living in cities, see below.

<sup>70</sup> See respectively *Crònica de Ramon Muntaner*, ch. 244, and *French chronicle*, pars. 1015-24; see also Longnon, *L'empire latin*, 192.

<sup>71</sup> The original version of the *Chronicle of Morea* was written in French between 1292 and 1320 and belonged to Bartolomeo II Ghisi, one of the most powerful barons in the Morea, until it was destroyed in 1331 or 1332; a shorter version with interpolations, also in French, was finished between 1341 and 1346; finally, a copy of this version was shipped from the Morea to the West in 1397. See Jacoby, "Quelques considérations," 134-50, 181-89. The inventory of the books left on his death in 1281 by Leonardo da Veroli, chancellor of the principality and trusted counselor of Prince William II, mentions fourteen romances. See Longnon, *L'empire latin*, 213. They were obviously in French, which was also used for the *Assizes of Romania*. See Jacoby, *La féodalité*, 82-88.

<sup>72</sup> The fresco in Thebes is mentioned in *Greek chronicle*, v. 8071-92; see also *French chronicle*, pars. 553-54. It belongs to the period of Nicholas II of Saint-Omer, lord of the castle from 1258 to 1289; see also Longnon, *L'empire latin*, 216. On the fresco of Patras, see Bon, *La Morée*, 452, 597, and n. 4; it is mentioned toward the end of the fourteenth century but cannot be dated.

<sup>73</sup> In opposition to *Grecus*, *Francus* was synonymous with Latin, but it also meant "freeman," hence the verb *affrancare*, to free from the bonds of slavery or servitude.

<sup>74</sup> See Bon, *La Morée*, 89-102, on the Church in the Morea.

mained on the whole faithful to their own Church;<sup>75</sup> with few exceptions, they were considered unfree by the conquerors.

The conquest of the Peloponnesus by the Franks proved to be more difficult than expected, was slow to proceed, and came to a successful end only in 1248. The difficulty of the enterprise and the smallness of their army induced the Frankish leaders to come to terms with the local population whenever possible and to conciliate it by promises formulated in written privileges delivered to the *archontes* or local leaders. The Franks guaranteed the maintenance of the Greek Church and the Greek clergy as well as the implementation of Byzantine law. In the words of the Greek version of the *Chronicle of Morea* they promised "that from now on, no Frank will force us to change our faith for the faith of the Franks, nor our customs and the law of the Romans."<sup>76</sup> Other promises dealt with the integrity of landholding, the preservation of individual privileges, and the retention of the Byzantine fiscal system.<sup>77</sup> Yet absolute continuity was not to be expected. As the conquest proceeded, estates of the Byzantine state, absentee landowners, Greek ecclesiastical institutions, and local *archontes* were confiscated, partitioned, and granted as fiefs to Frankish feudatories.<sup>78</sup> The use of Byzantine law ceased completely in the public sphere and was severely restricted in private matters.<sup>79</sup> The transfer of jurisdiction and taxation into private hands—that is, to the feudal lords—implied the introduction of a new concept of society.

The imposition of a political superstructure imported from the West and lacking any roots in the Peloponnesus arrested the natural evolution of Greek society, which was henceforth subjected to the influence of the conquerors, and even its internal structure was altered. The approach of Western knights to the Greek population depended to a very large extent upon their own political and institutional traditions, on concepts, values, and outlook that had evolved in the feudal society of the West. The Franks conceived society as strongly stratified and based upon a strict hierarchy of personal, private links. Depicting Byzantine society,

<sup>75</sup> A most convincing testimony about the attitude of the Greeks is provided by Marino Sanudo Torsello (*Istoria del Regno di Romania*, in Charles Hopf, *Chroniques gréco-romanes inédites ou peu connues* [Berlin, 1873]), who refers to Cyprus, Crete, Negroponte, Rhodes, other islands, and the Morea. "Benchè detti lochi siano sottoposti al dominio de Franchi e obbidienti alla Chiesa Romana, non dimeno quasi tutto il popolo è greco e inclina a quella setta, e il cuor loro è volto alle cose greche, e quando potessero mostrarlo liberamente, lo farianno" (p. 143). Although the *Istoria* was written between 1328 and 1333, more than a century after the conquest, this description no doubt reflects a permanent situation. Sanudo expressed the same opinion in a letter written on April 10, 1330, published by Friedrich Kunstmann, "Studien über Marino Sanudo den Aelteren, mit einen Anhang seiner ungedruckten Briefe," *Königliche bayerische Akademie der Wissenschaften, Historische Klasse, Abhandlungen*, (Munich, 1855), 7: 777. There is, however, evidence that some Greeks joined the Roman Church in the thirteenth or fourteenth century. See below. The Slavs were in the same category as the Greeks.

<sup>76</sup> *Greek chronicle*, v. 2093-95.

<sup>77</sup> See Jacoby, "Les archontes," 430-31.

<sup>78</sup> *Ibid.*, 423, 426-27, 441-42.

<sup>79</sup> See Jacoby, *La féodalité*, 34-38.

Western chroniclers of the early thirteenth century often speak of "fealty and homage" performed by Greeks to their emperor, implying thereby the existence in the Empire of a political and social structure similar to that of the feudal West.<sup>80</sup> Their description reflects a complete lack of understanding on the part of the Latins for societies other than their own. It is therefore not surprising that Frankish knights viewed the Greek society of the Morea as basically divided into two classes: on the one hand, the *archontes*, considered as free, on the other, the *villani*, or villains, subjected to the authority and jurisdiction of their lords and enjoying only severely limited personal freedom and legal capacity.<sup>81</sup> Free Greeks who did not belong to the class of *archontes* are seldom mentioned; numerically few, they represented a marginal element within the Greek society of Frankish Morea.<sup>82</sup>

The Franks not only perceived Greek society as divided into two classes but also considered that each of these, again in conformity with Frankish concepts, should be governed by its own set of laws. Characteristically the Franks translated social and economic realities into legal terms, and what had been, under Byzantine rule, a relatively "open" Greek society became a highly stratified system of sociolegal classes, the social status of the Greeks becoming hereditary. Crossing the basic lines of divisions became extremely rare and difficult and presumed a legal procedure to achieve it. Neither wealth nor education were of any weight, and upward mobility was solely dependent upon the favor of the Frankish princes and barons.

In spite of the cleavage between Latins and Greeks, the *archontes* gradually achieved some measure of integration within the ranks of the Moreot feudatories, the first step having already been taken at the time of the conquest. The agreements between the conquerors and *archontes* contained, among other clauses, provisions for homage and an oath of fealty performed by the *archontes* to the first leader of the conquerors, William of Champlitte, and, later, to his successors, the princes of Morea. As vassals the *archontes* were included in the class of feudatories owing simple homage, the lowest stratum in the feudal hierarchy, together with the Frankish mounted serjeants who were not of noble descent.<sup>83</sup> The first stage of integration therefore had a limited scope. It applied to the *archontes* only on a personal level and did not imply any grant of fiefs; it had a solely legal character but did not result in social integration within the nobility. Nor did it involve any change for the *archontes*

<sup>80</sup> See Jacoby, "Les archontes," 439-40.

<sup>81</sup> *Assises*, arts. 178, 194; art. 198 speaks of the *Griego villan*, to be distinguished from the Greek, who is an *archon*.

<sup>82</sup> For the time being, see David Jacoby, "Un aspect de la fiscalité vénitienne dans le Péloponnèse aux XIV<sup>e</sup> et XV<sup>e</sup> siècles: le 'zovaticum,'" *Travaux et Mémoires*, 1 (1965): 419 and n. 81.

<sup>83</sup> See above, n. 60. On *archontes* who achieved a higher status, see below.

in the status of their hereditary estates, the transmission and alienation of which were governed by Byzantine law, as before the conquest.<sup>84</sup>

The integration of the *archontes* within the ranks of the feudal hierarchy gradually proceeded beyond the legal and personal aspects already described, and a new stage was reached by the middle of the thirteenth century. That political as well as military considerations account for this process is perfectly illustrated by Moreot chronicles and Angevin documents. In 1248, after Monemvasia in the southern Peloponnesus had surrendered to him, Prince William II granted fiefs to three *archontes*, members of the three most influential families of this city, who thereby enlarged their landed wealth.<sup>85</sup> Grants of this nature also took place toward the end of 1261 or in the first months of the following year. When the Latin emperor of Constantinople, Baldwin II, traveled to the West after his capital had been recaptured by Byzantine forces, he stayed a few months in the Morea. Some of his vassals, among them Greek *archontes*, settled in the principality and were enfeoffed by Prince William II.<sup>86</sup> Prior to his death in 1278 the prince endowed the grandfather of John Sideros with feudal land.<sup>87</sup>

About the same time a new factor of integration appears. During the war waged in 1262 by Geoffroy de Briel, baron of Karytaina, against Byzantine forces in the southern Peloponnesus, this Frankish lord, using a stratagem suggested to him by the Greek warriors in his service, managed to vanquish the enemy. After achieving victory Geoffroy "gave to his Greeks many fine presents in land and other kind, and from the noblest amongst them he made knights." According to the story there was an intimate relationship between the Frankish baron and Greek members of his household: "he had nourished and brought them up," and they were his vassals,<sup>88</sup> obviously by simple homage, as was customary.<sup>89</sup> He endowed them with feudal tenements, which was not unusual by that time. Special attention, however, must be given to his attitude toward the "noblest amongst" the Greeks, who were no doubt of higher social status and probably belonged to the class of *archontes*. They were dubbed by their lord and, as a result, became knights in the same way as

<sup>84</sup> *The Assizes of Romania* devote four articles to the status of the *archontes* and their estates, arts. 71, 138, 178, 194. See also Jacoby, "Les archontes," 451-63.

<sup>85</sup> *Greek chronicle*, v. 2955; Jacoby, "Les archontes," 470.

<sup>86</sup> *French chronicle*, par. 87. For the approximate date of the enfeoffments, see Jacoby, "Quelques considérations," 165 and nn. 150-51.

<sup>87</sup> See below, n. 98.

<sup>88</sup> *Aragonese chronicle*, pars. 311-31, especially 313, 319, 331 ("et de los mas nobles fizo cavaleros"). The dating by the editor of this version ("1264") is erroneous and should be corrected by taking into account the sequence of events following the return of Prince William II to the Morea (see above, n. 86), as related by *Aragonese chronicle*, pars. 308-12; the episode took place before the arrival of a second Byzantine army to the Peloponnesus in 1263, on which see Bon, *La Morée*, 129.

<sup>89</sup> See above, p. 887 and n. 60.

the Franks.<sup>90</sup> The ceremony of dubbing sanctioned their firm and definitive integration within the ranks of the feudal nobility; once granted to a Greek, the status of knight obviously became hereditary. On the basis of fourteenth-century sources it seems that Greek knights henceforth bore the title *dominus*, or its French or Italian equivalents,<sup>91</sup> and rose to the rank of lieges by performing the appropriate rite prior to the investiture of their new fiefs. Added to vassalage and fiefholding, already fused, the knighthood granted by Geoffroy de Briel to some of his Greeks completed the process of social integration within the feudal nobility. And although no such evidence is available for vassals of the princes in the thirteenth century, one may assume that several of them enjoyed the same privilege.

It should be stressed that this integration was not achieved by all *archontes*, nor was it restricted to members of their class. In the fourteenth century two different groups of Greeks achieved integration in varying degrees. The largest group consisted, to be sure, of Greek feudatories, mostly *archontes*, who had performed simple homage; although feudatories, they did not become members of the knightly class. Significantly, they appear together with Frankish squires or mounted serjeants, who had not been dubbed, in the host assembled in 1304 by Nicholas of Saint-Omer to wage war against the Greeks of Epirus.<sup>92</sup> Besides *archontes* one finds in 1354 other Greeks among the feudatories of simple homage, such as the son of a *papas*, or Greek priest, and a *magister*, both undoubtedly of lowly origin and generally considered unfree by the Franks.<sup>93</sup> This clearly emphasizes that social promotion involving the crossing of class boundaries was entirely dependent, in Frankish Morea, upon the favor displayed by Frankish barons, princes, or representatives of princes. The integration of Greeks, mostly *archontes*, within the lowest stratum of the feudal hierarchy, initiated by the middle of the thirteenth century, seems to have gained considerable impetus thereafter. This phenomenon was no doubt concomitant with a decline in the number of Frankish feudatories and the growing need for military forces and administrative personnel.<sup>94</sup>

<sup>90</sup> See Jacoby, "Les archontes," 470-71.

<sup>91</sup> Jean Longnon and Peter Topping, *Documents sur le régime des terres dans la principauté de Morée au XIV<sup>e</sup> siècle* (Paris, 1969), respectively 21, l. 11 (in a document dated 1336); 128, ll. 5-6 (of 1354 or later): "per messere Johanni Misito," who was a liege (see below, p. 895 and n. 101), "et altri nobili de lo paese"; "per dominum Stephanum Cutrillum, militem" (on whom see below).

<sup>92</sup> *French chronicle*, par. 885. Among eighty-nine cavalrymen, there were "xiiij chevaliers adoubés, et li autres estoient escuier et gentil homme dou pays et tramontains"; "gentil homme dou pays" refers to Greek *archontes*. See also Jacoby, "Les archontes," 471. For later evidence on such *archontes*, see below.

<sup>93</sup> "Magister Manollus Vorkas" and "Theodorus Papa Stamatopulus." Longnon and Topping, *Documents*, 101, ll. 38-39. When "papa" appears between the first name and the name of the father, it should read "pape" and is related to the latter; see, e.g., "Ebreto Pape Leonis," "Xena" (a woman), "Pape Chimacopulo." *Ibid.*, 49, l. 3; 60, l. 41.

<sup>94</sup> For this process, see below. That most Greek feudatories were indeed *archontes* is illustrated

A second, smaller group of Greek feudatories consisted of those who were granted knighthood by barons, princes, or representatives of the latter. These feudatories as a rule held the rank of lieges,<sup>95</sup> thereby achieving integration within the feudal nobility. Besides *archontes*, one also finds among them the son of a *papas*, liable to six months a year of mounted service, according to a source dated 1354.<sup>96</sup> His social ascent is particularly impressive and does not seem to have been an isolated case. Although it is impossible to know how many of the Greek knights were not *archontes*, the number must have remained small. As the status of Greek knights was hereditary, it is not surprising to find some families active throughout the fourteenth century, but only a few attained prominent positions in the feudal elite and played a dominant part in the history of Frankish Morea. In spite of their power, landed wealth, and prestige in the principality none of them gained access to the baronial group. Only Franks and a few Italians became barons, and none of their families intermarried with Greek lieges.

The position of Greek feudatories as members of the social and political elite of the principality is well documented. Among "the ten best men of the country" of Chalandritsa in 1316, one finds two Latin lieges and three Greeks, presumably feudatories of simple homage.<sup>97</sup> John Sideros, scion of a family of Greek feudatories, was the grandson of a man who had received feudal lands from Prince William II prior to 1278. When a group of barons decided in 1341 to hand over the principality to Byzantine rule, Sideros, perhaps because of his knowledge of the Greek language, was chosen as one of the two emissaries who submitted the plan to Emperor John VI Kantakouzenos.<sup>98</sup> George and Basil Galentini, sons of a Greek knight, along with the two sons of the powerful baron Centurione I Zaccaria and other "noblemen" in 1374 asked the bailiff Francesco de San Severino to grant them knighthood, while the lists of feudatories who guaranteed the agreements concluded with Venice in 1387 and 1396 on behalf of the principality include several Greek lieges.<sup>99</sup>

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by the *Assizes of Romania*, arts. 71, 138, 178, 194; this treatise refers only to them when discussing the position of Greeks in the context of the feudal elite.

<sup>95</sup> See the list of Latin and Greek feudatories in Longnon and Topping, *Documents*, 73, ll. 1-12. The first two are lieges bearing the titles "meser" and "dominus" (on which see above, p. 893 and n. 91), whereas the others are feudatories of simple homage. The correspondence between liege homage and knighthood has already been emphasized above.

<sup>96</sup> "Dominus Theoderus Papa Chyriacopulos, tenetur servire sex mensibus cum serviciis stivoriis." *Ibid.*, 101, ll. 33-34. On the *stivoria*, see Jacoby, *La féodalité*, 135-37.

<sup>97</sup> Johan Janopolo, Raynalt Jalomati, and Mamallera Vazilaqui are Greeks. *Aragonese chronicle*, sec. 590. The two Latin lieges are entitled "micer," on which see above, nn. 91, 95.

<sup>98</sup> By 1341, sixty-three years had elapsed since the death of William II. It does not seem likely that the father of John Sideros was the beneficiary of the grant, as suggested by Hopf (*Geschichte*, 1: 433 a) and Bon (*La Morée*, 208, where "1302" should be replaced by "1304," and 242); see also *ibid.*, 212-13.

<sup>99</sup> *Aragonese chronicle*, pars. 718-21; see also Jacoby, "Les archontes," 474.



A most spectacular rise was achieved by two families, whose members took advantage of their position as high-ranking officers in the princely administration. The knight Stephen Cutrullus, or Koutroules, was in 1336 *protovestiarius* of the principality and as such handed out fiefs on behalf of the prince, controlled their content, and sold the products of the princely demesne. His son Nicholas, active since 1354 at least, was one of the most powerful feudatories in 1379, while John, son of Nicholas, was still a squire (*scudarius*) in 1375, having not yet been dubbed.<sup>100</sup> John Misito was granted several fiefs between 1313 and 1316, which were confirmed in 1324 by Prince Jean de Gravina when Misito was captain of Kalamata on the prince's behalf. John Misito was succeeded three years later in his fiefs by his son Nicholas, who died in 1344 and appears among the most influential men of the principality at that time.<sup>101</sup> The third member of this dynasty of Greek lieges was John II, one of the mightiest landlords of the Morea in 1377, who died before 1391.<sup>102</sup>

The knight's fief, or *feudum nobile*, of the Morea provided as a rule a revenue of 1,000 hyperpers and the standard fief of serjeanty half this income; feudatories holding such fiefs were liable to mounted service during the whole year.<sup>103</sup> Whether vassals of princes or of Frankish lords, most Greek feudatories were endowed with small fiefs, for which they had to perform only a small amount of military service. A servant of Catherine of Valois, mother of Prince Philip of Taranto, received together with his wife, prior to 1338, a fief supplying a revenue of twelve gold ounces or 189 hyperpers, with an obligation to provide the corresponding service, two months a year.<sup>104</sup> In 1354 one of the Greek feudatories of simple homage in Krestena was liable for one and a half months of mounted service, another for three months, and a third for a whole year; Nicholas Cutrullus, a knight, owed six months.<sup>105</sup> In several other cases, the small feudal tenements held by Greeks were obviously parts of

<sup>100</sup> Longnon and Topping, *Documents*, 21, ll. 11-12; and 45, l. 8, mention Stephen in 1336 and 1337; 73, l. 3; 150, l. 10; and 198, l. 6, refer to Nicholas, respectively in 1354, 1361, and 1379; see also Jacoby, "Les archontes," 472 and nn. 261, 265, as well as 473-74. On the functions of the *protovestiarius*, see Longnon, *L'empire latin*, 198, 205. The unpublished document mentioned in Jacoby, "Les archontes," 474 n. 271 (in fine), refers to John in 1375 and not in 1405, the year it was drafted. On other Greek squires, see *Aragonese chronicle*, pars. 718-21, and Jacoby, "Les archontes," 474.

<sup>101</sup> See Hopf, *Geschichte*, 1: 423 b, 435 b; Bon, *La Morée*, 204-05, 213-14, 242, 427 (where "Florent de Hainaut" should be replaced by "Louis de Bourgogne").

<sup>102</sup> Longnon and Topping, *Documents*, 127, l. 28-30; 149, l. 22-150, l. 9 (as liege of Prince Robert of Taranto); 198, l. 16; 214, l. 22. See also Bon, *La Morée*, 242, 252, 253 n. 1, 275, and Jacoby, "Les archontes," 473.

<sup>103</sup> See Jacoby, "Les archontes," 449-51, and *Aragonese chronicle*, par. 139; see also above, n. 55.

<sup>104</sup> Longnon and Topping, *Documents*, 58, ll. 8-10; comment in Jacoby, "Les archontes," 472-73. On the rate of exchange of the ounce, see Longnon and Topping, *Documents*, 21 n. 8; on the ratio of the tournois to the sterling and hyperper (1:4:80), see Jacoby, "Un régime de coseigneurie," 122 n. 7.

<sup>105</sup> Longnon and Topping, *Documents*, 73, ll. 6, 9-11. For other small fiefs of Greek feudatories, see *ibid.*, 95, ll. 18-22; 101, ll. 33-36; on Nicholas Cutrullus, see also above.

larger grants comprising scattered plots of land.<sup>106</sup> The fragmentary state of our documentation does not enable a definitive assessment of the income provided by the fiefs of Greek feudatories, especially of the lieges, who were entitled to hold more than one fief and to have holdings from several lords.<sup>107</sup> It seems obvious nevertheless that many of the fiefs held by Greek feudatories were insufficient to supply a livelihood to their holders. One may therefore safely assume that several of the latter had additional sources of income, primarily hereditary estates.

This was to raise many intricate problems, as hereditary estates held by Greeks were governed by Byzantine law. Compared with fiefs they enjoyed preferential status; they were what medieval Western jurists would have called allodial lands. They could be divided among heirs into equal parts, in true Byzantine fashion, whereas the fief could not be partitioned. Many of these estates owed no military service at all, and in cases where such service was to be performed, it could be evaded; indeed, at times it was difficult, if not impossible, to assess the amount of that service, because of the continuous process of partition.<sup>108</sup> Moreover, it was to be expected that, in some cases at least, Greeks holding fiefs would try to assimilate them to their hereditary estates, in order to escape the obligations and restrictions implied in the status of their feudal land. One can, then, easily understand the stress laid upon a clear-cut distinction between these two categories of landholding, a distinction that is once again to be found in the *Assizes of Romania*. It is not surprising that among the Frankish feudatories, especially of the lower rank, to whom most of the *archontes* and Greek feudatories were assimilated, a certain antagonism to the *archontes* should have built up in connection with the holding of nonfeudal land. These Frankish feudatories urged the imposition of military service on all land held in the principality by members of the feudatory class, whether Franks or Greeks, regardless of the legal regime by which this land was governed. The compiler of the *Assizes of Romania* expresses his support for this trend, but there is no evidence that it gained the upper hand. There can be no doubt, however, that some tension prevailed between Greek and Latin feu-

<sup>106</sup> See, for example, documents drafted in 1337 and 1338 that reflect a situation prior to these dates. *Ibid.*, 43, l. 27-44, l. 4; 45, ll. 10-22; 62, ll. 12-31, which should be compared with the parallel data provided by a document of 1354, in *ibid.*, 113, l. 22-114, l. 20.

<sup>107</sup> See *Assizes*, arts. 34, 46, 63, 68, 99.

<sup>108</sup> *Greek chronicle*, v. 1644-48, relates that it had been agreed upon at the time of the conquest that *archontes* of certain areas would perform military service according to the size of their estates. See Jacoby, "Les archontes," 430-31. Among the vassals of Niccolò Acciaiuoli, one Greek was liable to five months of mounted service "pro suo de redetago," i.e., *hereditagio*, or hereditary estate, while another owed six months of a serjeant's service: "tenetur de uno scutiferu mesibus [sic!] sex per anno redetago suo"; at Glyky, Goti Murmuru owed only one month of unspecified service for the same reason. Longnon and Topping, *Documents*, 77, ll. 9-12; 83, l. 5. Goti Murmuru, or Mourmoures, was probably a member of the well-known family of officials, on which see *ibid.*, 298, s.v. Murmuru. On the problems relating to the hereditary estates of the *archontes* and other Greek feudatories, see Jacoby, "Les archontes," 451-58; see also 430-31, 442-45.

datories in the third and fourth decades of the fourteenth century, the period in which the *Assizes* were compiled.<sup>109</sup>

In spite of this tension the integration of Greek feudatories within the feudal class of Morea proceeded even further than I have indicated. The most striking expression of this phenomenon is to be found in the second half of the fourteenth century; it is embodied in the Greek version of the *Chronicle of Morea*. The original version of this chronicle, no longer extant, was, as we have seen, written in French approximately between 1292 and 1320. In the true fashion of the *chansons de geste* it gave a glowing account of the epic of conquest, linking events in the Morea during the early thirteenth century with the First Crusade and the conquest of Jerusalem. The chronicle glorified the deeds of the conquerors and their descendants, emphasizing their military superiority and high morality. The author displayed an intense interest in judicial procedure as well as a thorough dislike and contempt for the Greeks. The original *Chronicle of Morea* was a typical product of the knightly class of the Morea; it was clearly intended for the members of this class and was to be read aloud when they assembled.<sup>110</sup>

The Greek version of the *Chronicle of Morea* was composed at a later date on the basis of a shorter French version and various oral traditions. Evidence seems to indicate that its author was a Greek feudatory, a member of the court of Erard III, lord of Arkadia in the southern Peloponnesus.<sup>111</sup> Being a Greek he felt the need to emphasize his hatred against the enemies of Frankish Morea, but significantly refrained from attacking the Greeks of the principality. The anti-Greek tendency of the French version of the *Chronicle*, heightened to a considerable extent in the Greek version,<sup>112</sup> is directed exclusively against the Greeks of Byzantium and of the despotate of Epirus: "Who will put faith in them, believe their oath, since they do not respect God nor love their ruler? They do not love each other except with guile."<sup>113</sup> From his attacks against the Orthodox Greeks, whom he accuses of being schismatics,<sup>114</sup> one may infer that the author accepted the subjection of the Greek Church to the pope, as established in the Morea at the time of the conquest, and therefore, in the religious and ecclesiastical sphere, clearly distinguished the Greeks of the principality from other Greeks.

It is apparent, then, that the author of the Greek version espoused entirely the cause and ideals of the conquerors, considering himself a Frank in the same way as did the feudatories of Western descent. When he

<sup>109</sup> For the evidence, see Jacoby, *ibid.*, 455-56, 458-59.

<sup>110</sup> For its dating and characteristics, see Jacoby, "Quelques considérations," 181-87; see also 134-35, 136-38.

<sup>111</sup> See *ibid.*, 140, 155, and especially 157.

<sup>112</sup> See especially the additions due to the author of the *Greek chronicle*, v. 724-30, 754-841, 1260-62, 3932-37, 3940, 3974-75.

<sup>113</sup> *Ibid.*, v. 728-30.

<sup>114</sup> *Ibid.*, v. 798-800.

refers to the Latins camping outside Constantinople in 1203, he speaks of "our people,"<sup>115</sup> which implies, on his part, full identification with the interests, values, and attitudes, even with the past of the Frankish feudatories. To be sure, the author did not express only personal views, nor were his views put forward solely in order to impress them upon other Greeks. The Greek version of the *Chronicle of Morea* was undoubtedly intended for Greek-speaking feudatories who identified themselves with the Franks in the same way the author did. The political verse in which it was written suited the taste of a Greek public; the language, a Greek strongly tainted by French and Italian influences and saturated with feudal terminology, reflects the Greek dialect spoken in the Morea in the fourteenth century.<sup>116</sup> At the same time that the Greek version of the *Chronicle of Morea* illustrates the attitudes and outlook of at least a section of the Greek *archontes* and feudatories, it also measures the extent to which they had achieved integration within the class of feudatories and points to their growing class-consciousness.

Some Greeks even went a step further. In 1245 Manuel Mourmouras, whose family was to play an important role in the administration of Frankish Morea,<sup>117</sup> built a church at his own expense in the area of Kranidion, in Argolis; its dedicatory inscription, besides mentioning this Greek landlord, seems to indicate that the latter had joined the Roman Church.<sup>118</sup> Similarly, a Greek epitaph found at Kalamata in the southern Peloponnesus and dated 1354, according to the Christian era used by the Franks but without any Byzantine dating, may have been ordered by a Greek who embraced the Roman faith.<sup>119</sup> Such cases, prompted by the desire to achieve full assimilation to the Franks, must have remained quite exceptional, most Greeks remaining faithful to their own Church. Indeed Latins started to adhere to the Greek rite, a convincing testimony to the weakness of the Latin clergy, which was bitterly denounced by Pope John XXII in 1322.<sup>120</sup>

Although achieved on a legal, social, and at times even religious level, the integration of Greeks was not complete. To be sure, many Greeks, especially feudatories, were bilingual. The very existence of a Greek version of the *Chronicle of Morea* is nevertheless significant, as it emphasizes that a gap persisted between Franks and Greeks in the cultural sphere. The same holds true in the religious field, although some

<sup>115</sup> *Greek chronicle*, v. 636.

<sup>116</sup> See Jacoby, "Quelques considérations," 157-58, 187.

<sup>117</sup> See above, n. 108, in fine.

<sup>118</sup> See Georgios Soteriou, "He Hagia Trias tou Kranidiou," *Epeteris tes hetaireas byzantinon spoudon*, 3 (1926): 193-205 (in Greek), and Bon, *La Morée*, 115.

<sup>119</sup> See Nikos A. Bees, "Christianikai epigraphai Messenias," *Dellion tes historikes kai ethnologikes hetaireas tes Hellados*, 6 (1904): 375 (in Greek), and Bon, *La Morée*, 591 n. 3, in fine.

<sup>120</sup> Caesar Baronius, *Annales ecclesiastici*, 24 (Bar-le-Duc, 1872): 187-88, anno 1322, par. 48 (Oct. 1, 1322); the same letter also mentions Greeks attending services of the Latin Church. On the Greeks, see also above, n. 75.

Greeks had joined the Roman Church. Relying on an earlier, presumably oral, tradition,<sup>121</sup> the author of the Greek version relates that the conquerors had promised that "no Frank will force us to change our faith for the faith of the Franks"<sup>122</sup>—the use of "us" for those who belong to the Greek Church in itself being highly significant. Many Greek feudatories resided in urban centers, while Frankish knights lived mostly in castles; intermarriage between the two groups is seldom attested and must have been rare in any event. All these factors evidently prevented social fusion of the two groups, a fact illustrated by a most revealing remark in the *Greek Chronicle*. While following the French version, which speaks of the arrogance of Gautier V de Brienne, duke of Athens, the author of the Greek version points out that this "is a trait of the Franks."<sup>123</sup> In spite of his strong identification with the Frankish feudatories, he viewed himself as part of a different social group, and such presumably was the feeling of most *archontes* and Greek feudatories.

CONSIDERING THE ATTITUDES and values common to the Frankish knights at the time of the conquest, the integration of Greeks within the class of feudatories seems at first glance surprising, but the conjunction of several factors explains how this phenomenon was brought about: the class-consciousness and self-confidence of the Frankish knights; their approach to the *archontes*; their practical needs; and, finally, the eagerness of the Greeks to achieve integration. The pattern of integration can be safely reconstructed.

Although imbued with a powerful class-consciousness, the Frankish feudal elite was willing to a certain extent to loosen its rigid system of social and legal stratification, so as to allow for the entry of Greeks within the lower rank of the feudatories. The Greek *archontes* could easily be viewed by the Frankish conquerors as Greek counterparts. Their traditional standing in Greek society; their leadership, combined with the exercise of a certain measure of authority; their style of life, different to some extent from that of other Greeks—all these seemed familiar to the Frankish knights. Besides, many *archontes* had undoubtedly been granted fiscal privileges by the emperors prior to the conquest. In the West, as we have seen, exemption from regular taxation on the whole distinguished the knightly class from other classes of society.

Therefore, when political, administrative, and military needs made it imperative Frankish princes and barons encouraged the integration of Greeks into the class of feudatories. The first stage was restricted to a

<sup>121</sup> On oral traditions incorporated in the Greek version, see Jacoby, "Quelques considérations," 153, 155.

<sup>122</sup> *Greek chronicle*, v. 2093-94.

<sup>123</sup> Compare *French chronicle*, par. 500, and *Greek chronicle*, v. 7263-7300, especially 7290, loosely translated here.

personal and legal aspect; on the whole, it was a convenient device, in conformity with the attitudes and outlook of the conquerors, to ensure the loyalty of the leaders of the local Greek and Slav population as well as the fruitful cooperation of Greek *archontes* with the conquerors in the administration of the principality. Even in the early years of Frankish rule, prior to 1210, several *archontes* assisted Frankish leaders as they carved out fiefs in the conquered areas:<sup>124</sup> the use of Byzantine land lists in this procedure depended upon the knowledge of the Greek language and of the intricate Byzantine fiscal terminology. As late as the fourteenth century fiscal documents were still drafted in Greek,<sup>125</sup> which partly accounts for the importance of the Greeks among the administrative personnel of the principality.<sup>126</sup> Integration grew in scope and intensity when the military position of Frankish Morea worsened in the sixties of the thirteenth century. Greeks, who started to display interest in the process, were obviously prompted more by social considerations than by material concerns such as the acquisition of fiefs, which were mostly small. Especially if he was dubbed, the Greek feudatory gained access to a new, superior status in society, adorned with special prestige; in relation to other Greeks, he greatly enhanced his social position. The pressure on the part of Greeks to receive knighthood must have gradually increased in the fourteenth century, and, correspondingly, so did the willingness of the princes and barons to grant it.<sup>127</sup> In view of the precarious political and military position of the Franks and the constant pressure exerted by Byzantium, the full cooperation of the Greeks was essential. Integration in the long run implied acceptance of the standards, values, and attitudes of the Frankish knights. The eagerness to achieve complete identification with this class is convincingly illustrated by the Greek version of the *Chronicle of Morea*.

It should be stressed that integration within the ranks of feudatories and, more specifically, within the knightly class was not restricted to Greeks and should be viewed in a wider context. The same general considerations that prompted the Frankish feudal elite to display a favorable attitude toward the *archontes* proved beneficial to Slav leaders<sup>128</sup>

<sup>124</sup> See *French chronicle*, pars. 107, 120, and *Greek chronicle*, v. 1641-50, 1830-35. On the circumstances, see Jacoby, "Les archontes," 441-42, and *La féodalité*, 54, 223-25.

<sup>125</sup> "In quodam practico in greca scriptura scripto, facto per dictum Johannem Murmurum" in 1337. Longnon and Topping, *Documents*, 52, ll. 14-15. "E o facti li inventarii in greco," in 1361. *Ibid.*, 147, l. 9.

<sup>126</sup> See above on the families Murmurus, Cutrullus, and Misito; in 1297 Vassilopoulos appears as *protovestiarius*, on whose functions see above, n. 100. Nikolakos of Patras was governor of the castle of St. George of Skorta in 1319 or 1320. *French chronicle*, par. 829, and p. 404, the latter confirmed by *Aragonese chronicle*, par. 647; see also below, n. 143. Many minor officials were Greeks.

<sup>127</sup> Which explains the growing number of Greek knights in fourteenth-century documents. See Bon, *La Morée*, 238-39, 260 n. 1, 266 n. 3. The same trend is perceptible in regard to Latins; Egidius de Leonessa, a *physician*, is granted a vineyard "in feudum et ligiam" in 1397. Text in Ernst Gerland, *Neue Quellen zur Geschichte des lateinischen Erzbistums Patras* (Leipzig, 1903), 185-86, doc. no. 7, and see Jacoby, *La féodalité*, 179-81.

<sup>128</sup> *Greek chronicle*, v. 3008-31, provides details about the submission of the Melings and the homage of their leaders to William II.

and even to Turks. When in 1263 Turkish mercenaries remained in Frankish Morea, William II ordered them to be baptized, and thereafter the prince made two of their leaders knights, endowed them with fiefs, and gave them wives—at least one of whom was a Frankish noblewoman, presumably of low rank.<sup>129</sup> Pressing—and well attested—military needs at this period and the establishment of Turkish troops in the principality account for these steps.<sup>130</sup> Far more important was the trend, beginning in the second half of the thirteenth century, toward the integration of Italian non-nobles, several of whom served in the princely administration. Leonardo da Veroli, for example, chancellor of the principality during the reign of William II, married the daughter of a Frankish baron.<sup>131</sup> When princes granted knighthood and fiefs to Italian merchants and bankers they did so primarily for economic reasons. Pietrono da Siena, a resident of Glarentsa, the most important harbor of Frankish Morea, was a liege of the prince since the sixties of the thirteenth century. Other Sienese and Florentines, among them members of the Acciaiuoli family, were endowed with the same status and fiefs in the late thirteenth and in the first half of the fourteenth centuries, presumably in return for the financial help they had extended to the princes; they in turn were joined by several Genoese and Venetians.<sup>132</sup>

The entrance of growing numbers of Greeks and Italians into the ranks of feudatories, especially of the knights, inevitably affected the nature of Frankish society, and of the knightly class in particular. Many Greek and presumably all Italian feudatories of non-noble descent with few exceptions<sup>133</sup> resided in urban centers, the Italians concentrating in the maritime cities of the Morea.<sup>134</sup> The extension of Byzantine domination in the Peloponnesus, initiated in 1262, gradually reduced the rural areas under the rule of the principality and prompted newly arrived Italians to join their brethren in the cities.<sup>135</sup> Greek lieges, several of them active in the administration of the principality, mingled with Frankish knights. At the same time Italian merchants and bankers combined their economic activity with a newly acquired knightly status, violating thereby the code of chivalry so cherished by Frankish knights. No con-

<sup>129</sup> *French chronicle*, par. 397; *Greek chronicle*, v. 5730-38; *Aragonese chronicle*, par. 363.

<sup>130</sup> On the area in which they settled, see Bon, *La Morée*, 337-38.

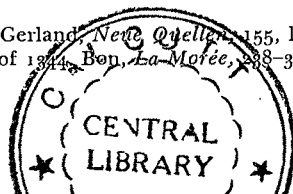
<sup>131</sup> On Leonardo, see Bon, *La Morée*, 127, 128, 149, 156, 160, and above, n. 71.

<sup>132</sup> On Italians in the Morea, see Jacoby, *La féodalité*, 84-85; see also 227, 234; the Venetians endowed with fiefs were not always resident in the Morea.

<sup>133</sup> Such as those who became barons, the Ghisi and Zaccaria.

<sup>134</sup> It should be emphasized that Frankish knights and barons, although resident in rural areas, sometimes had houses within the cities. Lise des Quartiers had one in Glarentsa prior to 1337. Longnon and Topping, *Documents*, 48, l. 29; see also Bon, *La Morée*, 323-24. In 1375 Erard III Mavros, lord of Arkadia, had a house in Venetian Modon. See Jacoby, "Quelques considérations," 149 n. 68. A princely residence existed in Andravida, considered the capital of Frankish Morea, as well as in other cities. See Bon, *La Morée*, 677; some of the vassals attached to his court may have settled in them permanently.

<sup>135</sup> Examples for 1353, 1414, and 1429 are found in Gerland, *Neue Quellen*, 155, ll. 11-15; 199, ll. 4-6; 210, ll. 16-18. Italian knights appear in a list of 1344, Bon, *La Morée*, 388-39.



tradition between the two was perceived in Italian cities. By 1320, therefore, as a result of the integration of Greeks and Italians within its ranks, the knightly class of Morea had already lost its homogeneous character.<sup>136</sup>

The dual process of social integration and subjective identification of the Greek *archontes* and feudatories with the Frankish feudatories also had an important effect on the Greek society of Frankish Morea. It stripped this society of a leadership willing to favor Byzantine expansion in the Peloponnesus. Moreover, as the Greek Church lacked the support of the *archontes*, it could not become the backbone of opposition to foreign rule, as it did in Venetian Crete at the same period.<sup>137</sup> The interests of the Greek feudatories obviously lay with the Franks, which explains why the Greeks of Frankish Morea on the whole remained as faithful to their rulers and fought as bravely in their armies as they did in 1264 and 1304. Only two instances of regional revolts on their part have been recorded during the two centuries of Latin rule. With the reappearance of Byzantine forces in the Peloponnesus in 1262, when it seemed as if the Empire would regain its lost territories in the peninsula, unrest was generated within the Greek and Slav population. The Greek inhabitants of Lacedaemonia left their city for Byzantine Mistra, and the Greeks of Skorta, in the western Peloponnesus, rose in arms against their rulers. After the Byzantine armies had been repelled and the revolt crushed, Greeks remained in peace for about forty years.<sup>138</sup> Those of Skorta rebelled again in 1304 under the leadership of a Greek priest and several *archontes* when Prince Philip of Savoy imposed taxes on the *archontes*, a step that was rightly considered a breach of faith. The *archontes* of Skorta had been promised fiscal exemption, presumably at the time of the conquest; moreover, the prince's action also deprived them of their privileged status and of the benefits of integration into the class of feudatories.<sup>139</sup> Other revolts against the Franks broke out only in areas inhabited by Slav populations, which had retained their firm tribal structure, traditional leadership, and spirit of independence. Such was the case of the Slavs of Tsaconia and the Taygetos Mountains who participated in the rising of 1263-64<sup>140</sup> and of others who seized Kalamata in 1292 or 1293.<sup>141</sup> Two cases of treason should also be mentioned in this connection. In 1296 a Greek who wished to avenge an outrage inflicted on him by a Frankish

<sup>136</sup> See Jacoby, *La féodalité*, 86.

<sup>137</sup> On Crete, see Freddy Thiriet, *La Romanie vénitienne au moyen âge. Le développement et l'exploitation du domaine colonial vénitien (XII<sup>e</sup>-XV<sup>e</sup> siècles)* (Paris, 1959), 288-93, 301-02, 403-06, 429-33.

<sup>138</sup> See Bon, *La Morée*, 129-34, and on Skorta, 363-406.

<sup>139</sup> *French chronicle*, pars. 920-51, especially 921-23 and 950; see also Bon, *La Morée*, 178, and, for the time of the conquest, Jacoby, "Les archontes," 430-31 and n. 50, where the date should be corrected.

<sup>140</sup> See Bon, *La Morée*, 129-34, 498-502, on these Slavs.

<sup>141</sup> *French chronicle*, pars. 693-745; see also Bon, *La Morée*, 168, 505-07; the erroneous date "1295" on 506 should be corrected.



knight bribed officials of the prince of the Morea, inducing them to deliver the castle of St. George to Byzantium.<sup>142</sup> Then, after the castle had been returned to Frankish rule, it was once again handed over to Byzantium, in 1319 or 1320, by its venal commander, a Greek official of the prince.<sup>143</sup> On both occasions, personal reasons prompted these Greeks to betray their ruler. These cases obviously do not reflect the attitude of Greek society at large. Furthermore, no Greek revolt has been recorded for the principality of the Morea after 1304, precisely the period of the impressive integration of Greek feudatories. The encounter of Western conquerors and local population had produced profound changes both in the knightly class and in the Greek society of the Morea.

THE ENCOUNTER OF Latin conquerors and indigenous populations in the areas bordering the eastern Mediterranean has on the whole not yet been properly explored. Even so, it is quite possible to identify some distinctive features of the relationship between the two communities and to describe their respective evolution. In all these areas, just as in Frankish Morea, religious affiliation provided the basic criterion of social stratification. The Latins or Franks constituted the society of the rulers. Whether one was Muslim or belonged to one of the numerous Oriental churches, a local inhabitant and his descendants were relegated to a lower status. Beyond this, one might have expected that nonfeudal groups of conquerors, who would not be bound by the social prejudice that stems from a strong class-consciousness, would be more accommodating toward the ruled. Evidence, however, points to the contrary. Exclusiveness is the dominant feature of the attitude adopted by the conquerors in Venetian Crete, Catalan Athens, and Genoese Chios. Local inhabitants were strictly excluded from joining the ruling elite and prevented from infiltrating into its ranks or encroaching upon its positions. Even adoption of the Roman creed could not alter their social status.

In Venetian Crete, where intermarriage between Latins and Greeks was prohibited and strictly enforced during the thirteenth and fourteenth centuries, Greeks were also barred from holding military tenures provided by the Venetian government and, as a result, from becoming members of the Cretan *Maggior Consiglio*, the most important local assembly participating in the government of the island. Only a small number of families of Greek *archontes* were granted special privilege to hold military tenures, and fewer still could wed Latin women. Besides, Greeks had no access to important administrative posts and could not serve as

<sup>142</sup> *French chronicle*, pars. 802–16. For the site of this castle, see Bon, *La Morée*, 381, 513.

<sup>143</sup> The exact date cannot be ascertained. See Jacoby, "Quelques considérations," 142 n. 37, and Bon, *La Morée*, 202.

mercenaries, while Latins who had married Greek women were to be dismissed. In practice the local Venetian authorities did not always hold to these rules and occasionally had to be reminded of them by the central government, but the official Venetian policy was clear, and, as far as the *archontes* were concerned, it was indeed implemented. As I have already suggested, this situation accounts for the role played by the latter as leaders of the Greek opposition to Venetian rule.<sup>144</sup>

In Catalan Greece the marriage of Latin women to Greeks was forbidden, a restriction that remained valid even in the rare cases when the latter were granted the status enjoyed by Franks or conquerors.<sup>145</sup> Catalan legislation was more lenient than was Venetian legislation in Crete, as it did not totally prohibit intermarriage, and Latins could marry Greek women; but it nevertheless prevented Greeks from integrating into the Latin elite. In Chios members of the Greek upper class were ousted from the citadel of the city shortly after its conquest in 1346 by the Genoese, and the descendants of those who participated in the conquest combined to form an association, the *albergo* of the Giustiniani, to which only few Greeks gained access.<sup>146</sup>

The special significance of these three cases is that the ruling elite did not belong to a feudal nobility. Unlike the feudalized areas, no link existed here between occupation and social status. The conquerors and those who joined them dwelt in urban centers, together with the indigenous Greek population, and pursued various economic activities similar to those of their neighbors. Everyday life brought them into close contact with the ruled, and hardly any factor save religion differentiated the two groups. Constant social and economic intercourse exerted a corrosive influence upon the social supremacy and political prerogatives acquired by conquest and permanently threatened them. As the danger of assimilation to the local Greek society, which was numerically far superior, was clearly perceived by the conquerors at the outset, they resorted to such measures of self-defense as total or partial prohibition of intermarriage to create an efficient barrier between themselves and the ruled, especially the upper class. Segregation was the most suitable means to preserve their separate group identity.

By contrast, the areas into which feudalism was transplanted underwent a different social development. In the Latin Kingdom of Jerusalem the feudal conquerors did not face any problem of integration, as the members of the local elite were exterminated or fled the cities captured by the Crusaders at the time of the conquest. Exclusiveness and class-

<sup>144</sup> For the time being, see Thiriet, *La Romanie vénitienne*, 131-32, 134-35, 291-93.

<sup>145</sup> As attested by a privilege bestowed upon a Greek inhabitant of Livadia, presumably in 1311; for the dating of this document, see David Jacoby, "La 'Compagnie catalane' et l'état catalan de Grèce. Quelques aspects de leur histoire," *Journal des Savants* (Apr.-June 1966), 88-89.

<sup>146</sup> See Philip P. Argenti, *The Occupation of Chios by the Genoese and their Administration of the Island, 1346-1566* (Cambridge, 1958), 1: 332-33, 582-612.

consciousness as displayed by the knights were encouraged by the rapid success of the conquest and the encounter with a basically agrarian population, whose occupation was considered an attribute of lowly social status.<sup>147</sup> Almost since its conquest by the Crusaders in 1191, Cyprus was closely connected with the Kingdom of Jerusalem, from which there came both the special brand of feudalism introduced into the island and many of the knights who settled it. In Cyprus as in Frankish Morea the knightly class confronted an existing Greek elite, although conditions in Cyprus were different from those in the Peloponnesus in the early thirteenth century. The conquest of the island was rapid, and two revolts were ruthlessly crushed thereafter, within less than a year. These events account for the uncompromising policy adopted toward the indigenous population. Besides, one may safely assume that the attitude and concepts of the Cypriot knightly class were molded to a large extent by those that had previously evolved in the Kingdom of Jerusalem. Integration of local notables was therefore excluded, the few exceptions encountered in the fourteenth century merely confirming the rule.<sup>148</sup>

Of all the areas mentioned above, Norman Italy is certainly the one whose social evolution most closely resembles that of the Morea. Descendants of the Greek and Lombard notables were fully integrated into the knightly class and held fiefs, both feudal and patrimonial land, or only the latter. In this last case, they were considered "knights who hold nothing" (*milites qui nihil tenent*), an expression that implies a tendency to assimilate these notables and their patrimonial estates respectively to the Norman conquerors and their fiefs, which carried military service. The survival of Byzantine and Lombard law in Norman Italy and its application to patrimonial estates offers yet another parallel to Frankish Morea,<sup>149</sup> although the analogous social evolution should not be ascribed to the influence of the Angevins, heirs of the Norman kingdom of Sicily, who also ruled Frankish Morea since 1278. It is at least partially attributable to the conditions of the conquest, which was slow and progressive in both areas and which, therefore, induced the conquerors to adopt a realistic and conciliatory attitude toward the indigenous population, especially the local upper class.

As is evident from this brief discussion, many complex problems arose from the encounter of Western conquerors and local society in the eastern Mediterranean, problems that suggest the benefits to be gained from

<sup>147</sup> The latest study on the subject is by Joshua Prawer, *The Crusaders' Kingdom. European Colonialism in the Middle Ages* (New York, 1972), especially 46-76, 504-33. The author does not distinguish, however, between the attitude of the knights and that of the burgesses toward the local population. His assertion (on p. 68) that knights married local women is not supported by any evidence, and the *pullani*, or *poulains*, of mixed stock were obviously children of Frankish burgesses and native mothers.

<sup>148</sup> See Louis de Mas Latrie, *Histoire de l'île de Chypre sous le règne de la maison de Lusignan* (Paris, 1852-61), 1: 135-40; George Hill, *A History of Cyprus* (Cambridge, 1948), 2: 8.

<sup>149</sup> See Claude Cahen, *Le régime féodal de l'Italie normande* (Paris, 1940), 53-54, 124-27.

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## The New Deal, National Socialism, and the Great Depression

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JOHN A. GARRATY

THE GREAT DEPRESSION of the 1930s was a unique phenomenon in that it happened simultaneously over almost the entire globe. It was experienced directly, not merely through its repercussions, by the people of nearly every nation and social class. Neither of the so-called world wars of this century was so pervasive, and while many distinct combinations of past events, such as the French Revolution, may be said to have had global results, these usually have been felt only over extended periods of time, long after the "event" itself has ended. The depression therefore presents a remarkable opportunity for historians interested in comparative study and analysis. It provides a kind of independent variable; when we look at how different nations or groups of people responded to the Great Depression, we can be sure, at least in a sense, that we are examining one single "thing," the existence of which was universally recognized at the time. Contemporaries disagreed among themselves about the causes of the depression (to say nothing of their disagreements about how it might be ended), but that there *was* a world-wide depression and that their own depression was related directly to those of their fellows, few denied.<sup>1</sup>

In this article I shall compare the response to the depression in the United States and Germany during the period from 1933 to about 1936 or 1937—that is, during the early years of the regimes of Franklin Roosevelt and Adolf Hitler. The choice is neither capricious nor perverse. I hope to

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<sup>1</sup> See, for example, League of Nations, *The Course and Phases of the World Economic Depression* (Geneva, 1931) and the annual volumes of the league's *World Economic Survey* (Geneva, 1931–39); Paul Einzig, *The World Economic Crisis: 1929–1931* (London, 1931); Lionel Robbins, *The Great Depression* (London, 1934); Eugen Varga, *The Great Crisis and Its Political Consequences* (New York, 1934); Richard Lewisohn, *Histoire de la crise* (Paris, 1934); Bertrand Nogaro, *La crise économique dans le monde et en France* (Paris, 1936); Adolf Sturmthal, *Die grosse Krise* (Zurich, 1937); H. V. Hodson, *Slump and Recovery: 1929–1937* (Oxford, 1938); Jean Lescure, *Des crises générales et périodiques de surproduction* (5th ed.; Paris, 1938); Joseph A. Schumpeter, *Business Cycles: A Theoretical, Historical, and Statistical Analysis of the Capitalist Process* (New York, 1939). There is also an enormous contemporary literature on the depression in individual nations, a common theme of which is the universality of the phenomenon.

demonstrate that Nazi and New Deal antidepression policies displayed striking similarities. Since the two systems, seen in their totality, were fundamentally different, these similarities tell us a great deal about the depression and the way people reacted to it.

The differences between nazism and the New Deal scarcely need enumeration; within the context of Western industrial society two more antithetical systems would be hard to imagine. The Nazis destroyed democratic institutions. They imprisoned and murdered dissidents, even those, such as the Jews, who simply did not fit their image of a proper German. The New Dealers, whatever their limitations, threw no one in jail for his political beliefs and actually widened the influence of underprivileged elements in the society. Furthermore the historical experience, the traditions, and the social structure of the two nations could hardly have been more unlike. The Great War and its aftermath affected them in almost diametrically opposite ways. All the major economic groups in the two countries—farmers, industrialists, factory workers, and so on—confronted the problems of the depression with sets of expectations and values that differed greatly.

But these were the industrial nations most profoundly affected by the Great Depression, measured by such criteria as the percentage decline of output, or by the degree of unemployment. When Hitler and Roosevelt came to power both nations were in desperate straits; Hitler and Roosevelt followed leaders who had spectacularly failed to inspire public confidence in their policies. Both the severity of the depression and the sense of despair and crisis that existed in Germany and America in early 1933 set the stage for what followed.

I have focused on the early New Deal and Nazi years because at that time the new governments were primarily concerned with economic problems resulting from the depression. Hitler's expansionist ambitions no doubt existed from the beginning, but it was not until after the adoption of the Four Year plan in 1936 that he turned the German economy toward large-scale preparation for war.<sup>2</sup> Similarly, although his motive was clearly defensive, after 1937 Roosevelt also began to be influenced by military considerations.

Needless to say, by considering the similarities in American and German experiences during the depression, I do not mean to suggest that the New Deal was a form of fascism or still less that nazism was anything but an unmitigated disaster. I slight the basic differences between the New Deal and Nazi experiments here partly because they are well known but also because the differences did not affect economic policy as much as might be expected. The worse horrors of nazism were unrelated

<sup>2</sup> David Schoenbaum, *Hitler's Social Revolution: Class and Status in Nazi Germany, 1933-1939* (New York, 1967), 113; Dieter Petzina, *Autarkiepolitik im Dritten Reich: Der national-sozialistische Vierjahresplan* (Stuttgart, 1968), 26-49. Cf. Gerhard Meinck, *Hitler und die deutsche Aufrüstung: 1933-1937* (Wiesbaden, 1959).

to Nazi efforts to overcome the depression. Hitler's destruction of German democracy and his ruthless persecution of Jews had little impact on the economy as a whole. Discharging a Jew and giving his job to an "Aryan" did not reduce unemployment. The seizure of Jewish property merely transferred wealth within the country; it did not create new wealth. Moreover, actions undertaken by New Dealers and Nazis for different reasons often produced similar results. My argument concentrates on policies and their effects, not on the motives of the policy makers.

Finally, the fact that countless Germans were deluded by Nazi rhetoric (or that large but lesser numbers were repelled by the system) does not mean that nothing the Nazis did helped anyone but themselves and their sympathizers. Moral abhorrence should no more blind us to the success of some Nazi policies than should admiration of the objectives of the New Deal to its failures. As the English economic historian C. W. Guillebaud warned in *The Social Policy of Nazi Germany*, written in the midst of the Battle of Britain, "Modern Germany is a highly complex phenomenon, with much that is good and bad in it, and nothing is achieved except distortion and absence of reality by any attempt to reduce it to a simple picture of a vast population deluded and oppressed by a small number of brutal gangsters."<sup>3</sup>

CONSIDER FIRST how the two governments dealt with poverty and mass unemployment. Both combined direct relief for the indigent with public-works programs to create jobs. The Americans stressed the former, the Germans the latter, with the result that while acute suffering was greatly reduced in both nations, unemployment declined much more rapidly in Germany. Congress appropriated \$3.3 billion for public works in 1933, but Roosevelt, unconvinced that public works would stimulate the economy and fearful of waste and corruption, did not push the program. Briefly, during the winter of 1933-34, he allowed Harry Hopkins to develop his Civil Works Administration, which found jobs for over four million people, but in the spring the program was closed down to save money. Only in 1935 did federal public works become important. Then, under Hopkins's Works Progress Administration and Harold L. Ickes's Public Works Administration, countless roads, schools, bridges, dams, and public buildings were constructed.<sup>4</sup> The Germans, on the other hand, immediately launched an all-out assault on unemployment. Expanding upon policies initiated under Franz von Papen and Kurt von Schleicher, they stimulated private industry through subsidies and tax rebates, encouraged consumer spending by such means as marriage loans, and plunged into the massive

<sup>3</sup> C. W. Guillebaud, *The Social Policy of Nazi Germany* (Cambridge, 1942), 132.

<sup>4</sup> Searle F. Charles, *Minister of Relief: Harry Hopkins and the Depression* (Syracuse, 1963), 44-52, 61, 65, 159-65, 230-31; William E. Leuchtenburg, *Franklin D. Roosevelt and the New Deal: 1932-1940* (New York, 1963), 121-30, 133.

public-works program that produced the autobahns, and housing, railroad, and navigation projects.<sup>5</sup> If some New Deal projects seemed to critics wasteful and unnecessary, so did the Nazi penchant for gigantic stadiums and other public buildings, as described in Albert Speer's memoirs. The American boondoggle had its parallel in what the Germans called *Pyramidenbau*, pyramid-building.<sup>6</sup>

It is fashionable, and not of course inaccurate, to note the military aspect of German public-works policies, although in fact relatively little was spent on rearmament before 1935.<sup>7</sup> It is less fashionable, but no less accurate, to point out that the aircraft carriers *Yorktown* and *Enterprise*, four cruisers, many lesser warships, as well as over one hundred army planes and some fifty military airports (including Scott Field in Illinois, the new Air Force headquarters) were built with Public Works Administration money—more than \$824 million of it.<sup>8</sup> There was, furthermore, little difference in appearance or intent between the Nazi work camps and those set up in America under the Civilian Conservation Corps. Unlike the public-works programs, these camps did not employ many industrial workers who had lost their jobs, nor were they expected to have much of a stimulating effect on private business. Both employed enrollees at forestry and similar projects to improve the countryside and were essentially designed to keep young men out of the labor market. Roosevelt described work camps as a means for getting youth “off the city street corners,” Hitler as a way of keeping them from “rotting helplessly in the streets.” In both countries much was made of the beneficial social results of mixing

<sup>5</sup> Dieter Petzina, “Hauptprobleme der deutschen Wirtschaftspolitik 1932/33,” *Vierteljahrshefte für Zeitgeschichte*, 15 (1967): 18–30, 43–50; C. W. Guillebaud, *The Economic Recovery of Germany: From 1933 to the Incorporation of Austria in March 1938* (London, 1939), 38–42, 51–52; René Erbe, *Die nationalsozialistische Wirtschaftspolitik, 1933–1939, im Lichte der modernen Theorie* (Zurich, 1958), 91–92, 182–86; Heinrich Bennecke, *Wirtschaftliche Depression und politischer Radikalismus: 1918–1938* (Munich, 1970), 300–01; Leo Grebler, “Work Creation Policy in Germany: 1932–1935,” *International Labour Review*, 35 (1937): 329–51, 505–27; Albert Speer, *Inside the Third Reich: Memoirs* (New York, 1971), 114–19, *passim*; Fritz Thyssen, *I Paid Hitler* (New York, 1941), 143–47.

<sup>6</sup> For a convenient tabular summary of both pre-Hitler and Nazi public-works activities, see Heinrich Dräger, *Arbeitsbeschaffung durch produktive Kreditschöpfung* (4th ed.; Düsseldorf, [n.d.]), 165. On the antecedents of the Nazi program, see Gerhard Kroll, *Von der Weltwirtschaftskrise zur Staatskonjunktur* (Berlin, 1958), 421–55, 458, 461–63.

<sup>7</sup> Burton J. Klein, *Germany's Economic Preparation for War* (Cambridge, Mass., 1959), 16–17, 254; Hjalmar Schacht, 1933: *Wie eine Demokratie stirbt* (Düsseldorf, 1968), 86–91. Through 1934, Nazi expenditure for armaments totaled 4.9 million marks. Wolfram Fischer, *Deutsche Wirtschaftspolitik: 1819–1945* (Opladen, 1968), 102.

<sup>8</sup> Public Works Administration, *America Builds: The Record of PWA* (Washington, 1939), 36–37, 105–06, 290–91; Arthur M. Schlesinger, jr., *The Coming of the New Deal* (Boston, 1959), 287–88. The first New Deal authorization for public works was part of the National Industrial Recovery Act and provided for “the construction of naval vessels within the terms and/or limits of the London Naval Treaty of 1930 and of aircraft required therefor and construction of heavier-than-air aircraft and technical construction for the Army Air Corps and such Army housing projects as the President may approve, and provision of original equipment for the mechanization or motorization of such Army tactical units as he may designate.” Henry S. Commager, ed., *Documents of American History* (2d ed.; New York, 1941), 2: 455.

thousands of young people from different walks of life in the camps and of the generally enthusiastic response of youth to the camp experience.<sup>9</sup>

Furthermore, both were organized on semimilitary lines with the subsidiary purposes of improving the physical fitness of potential soldiers and stimulating public commitment to national service in the emergency. Putting the army in control of hundreds of thousands of young civilians roused considerable concern in the United States. This concern proved to be unfounded; indeed, the army undertook the task with great reluctance and performed it with admirable restraint. It is also difficult to imagine how so large a program could have been inaugurated in so short a time in any other way. The CCC program nevertheless served paramilitary and patriotic functions not essential to its announced purpose. Corpsmen were required to stand "in a position of alertness" while speaking to superiors and to address them as "Sir." Camp commanders possessed mild but distinctly military powers to discipline their men, including the right to issue dishonorable discharges. Morning and evening flag-raising ceremonies were held as "a mark," the civilian director of the CCC, Robert Fechner, explained, "of patriotism, of good citizenship and of appreciation by these young men of the thoughtful care being given them by their government." Army authorities soon concluded that six months' CCC service was worth a year's conventional military training, and Secretary of War George Dern claimed that running the camps provided the army with the best practical experience in handling men it had ever had. Summing up the military contribution of the CCC, John A. Salmond, the most sympathetic historian of the agency, wrote:

To a country engaged in a bloody war, it had provided the sinews of a military force. It had given young officers valuable training in command techniques, and the nearly three million young men who had passed through the camps had received experience of military life upon which the Army was well able to build.<sup>10</sup>

New Deal and Nazi attempts to stimulate industrial recovery also resembled each other in a number of ways. There was at the start much jockeying for position between small producers and large, between

<sup>9</sup> Roosevelt stressed "the moral and spiritual value" of camp life, Hitler its "class reconciling" function. Samuel I. Rosenman, ed., *The Public Papers and Addresses of Franklin D. Roosevelt* (New York, 1938), 2: 81; Max Domarus, ed., *Hitler: Reden und Proklamationen* (Munich, 1965), 1: 321.

<sup>10</sup> Guillebaud, *Economic Recovery of Germany*, 41, 201, 225-26; Schoenbaum, *Hitler's Social Revolution*, 63-64, 79; Erbe, *Wirtschaftspolitik*, 90; Domarus, *Reden und Proklamationen*, 1: 212; John A. Salmond, *The Civilian Conservation Corps, 1933-1942: A New Deal Case Study* (Durham, 1967), 221, *passim*; Charles W. Johnson, "The Army and the Civilian Conservation Corps, 1933-42," *Prologue*, 4 (1972): 139-56; Schlesinger, *Coming of the New Deal*, 337-40; Rosenman, *Public Papers*, 2: 82; Frances Perkins, *The Roosevelt I Knew* (New York, 1946), 177-80; William E. Leuchtenburg, "The New Deal and the Analogue of War," in John Braeman, Robert H. Bremner, and Everett Walters, eds., *Change and Continuity in Twentieth-Century America* (Columbus, 1964), 114-17.



manufacturers and merchants, between inflationists and deflationists, between planners, free enterprisers, and advocates of regulated competition. In Germany the great financiers and the leaders of the cartelized industries, most of them bitterly opposed to democratic institutions, demanded an authoritarian solution that would eliminate the influence of organized labor and increase their own control over the economy, whereas small operators, shopkeepers, and craftsmen wanted to reduce the power of bankers and to destroy not only the unions but also the industrial monopolies and chain stores. The former sought to manipulate the Nazis, the latter comprised, in the main, the Nazis' enthusiastic supporters, but Hitler and the party felt and responded to pressures from both camps.<sup>11</sup> In the United States most big business interests had no open quarrel with the existing order, but by 1933 many were calling for suspension of the antitrust laws in order to end the erosion of profits by competitive price cutting. Other interests wanted to strengthen the antitrust laws, still others favored various inflationary schemes, still others some attempt at national economic planning. All clamored for the attention of the new administration.<sup>12</sup>

The ideas of these groups were contradictory, and neither Roosevelt nor Hitler tried very hard to resolve the differences. Roosevelt's method was to suggest that the contestants lock themselves in a room until they could work out a compromise. But Hitler, who freely admitted to being an economic naïf, was no more forceful. "I had to let the Party experiment," he later recalled in discussing the evolution of his industrial recovery program. "I had to give the people something to do. They all wanted to help. . . . Well, let them have a crack at it."<sup>13</sup>

Out of the resulting confusion emerged two varieties of corporatism, a conservative, essentially archaic concept of social and economic organization that was supposed to steer a course between socialism and capitalist plutocracy. Corporatist theory argued that capitalists and workers (organized in industry-wide units) should join together to bring order and profit to each industry by eliminating competition and wasteful squabbling

<sup>11</sup> Schoenbaum, *Hitler's Social Revolution*, 115-24; Arthur Schweitzer, *Big Business in the Third Reich* (Bloomington, 1964), 4-5, 75-79; L. Hamburger, *How Nazi Germany Has Controlled Business* (Washington, 1943), 113-14; Raimund Rämisch, "Der berufsständische Gedanke als Episode in der nationalsozialistischen Politik," *Zeitschrift für Politik*, 4 (1957): 263-72. Schoenbaum describes the situation as "a real, if muffled, struggle, not about ideological principles but about the control and direction of the economy" (p. 122), and Schweitzer writes: "We must give up the notion of the [Nazi] state as a unified . . . entity developing an economic policy. . . . Instead we must see who stood behind the state and who originated each economic goal and supported the subsequent economic policy" (p. 4).

<sup>12</sup> Ellis W. Hawley, *The New Deal and the Problem of Monopoly: A Study in Economic Ambivalence* (Princeton, 1966), 21-25; Schlesinger, *Coming of the New Deal*, 179-85.

<sup>13</sup> Schlesinger, *Coming of the New Deal*, 98; Schoenbaum, *Hitler's Social Revolution*, 123-24. Hitler's intention to use economic policy to achieve his political objectives was clear from the start, Petzina writes, but the particular policies adopted in 1933 were "contradictory and heterogeneous." The idea that the Nazis had a thoroughly developed recovery program in 1933 became current only after the recovery had taken place. Petzina, "Hauptprobleme," 50.

between labor and management. These associations should be supervised by the government in order to protect the public against monopolistic exploitation. In 1933 corporatism was already being experimented with by the Portuguese dictator Antonio de Oliveira Salazar and more tentatively by Benito Mussolini. It also had roots in American and German experience. The American trade association movement of the 1920s reflected basic corporatist ideas (with the important exception that industrialists were opposed to government representation in their councils). When the depression undermined the capacity of these "voluntary" associations to force individual companies to honor the associations' decisions, some trade association leaders became willing to accept government policing as a necessary evil. Among others, Gerard Swope of the General Electric Company attracted considerable attention in 1931 with his Swope Plan for a nationwide network of compulsory trade associations supervised by the Federal Trade Commission. President Herbert Hoover, who had been among the most ardent supporters of trade associations, denounced the Swope Plan as both a threat to industrial efficiency and "the most gigantic proposal of monopoly ever made." He considered all such compulsory schemes fascistic. But a number of early New Dealers—Hugh Johnson, Donald Richberg, and Lewis Douglas among others—found corporatism appealing. In Germany the concept of government-sponsored cartels that regulated output and prices had a long tradition, but the existence of powerful trade unions precluded the possibility of a truly corporative organization before 1933. Hitler's success changed that swiftly. Nazi ideologues such as Gottfried Feder combined with big industrialists like Fritz Thyssen and leaders of small business interests like Dr. Heinrich Meusch to push the corporative approach. The works of one of the leading theorists of corporatism, Professor Othmar Spann of the University of Vienna, were widely discussed in Germany in 1933, and the Nazis established a complex system of "estates" governing all branches of industry.<sup>14</sup>

In America the process went not nearly so far, but the system of self-governing industrial codes established under the National Recovery Administration was obviously in the same pattern.<sup>15</sup> Production controls,

<sup>14</sup> Herman Lebowics, *Social Conservatism and the Middle Classes in Germany: 1914-1933* (Princeton, 1969), 109-10, 133-34; Taylor Cole, "Corporative Organization in the Third Reich," *Review of Politics*, 2 (1940): 438-62; Thyssen, *I Paid Hitler*, 122-27; Schweitzer, *Big Business*, 79-81, 135-36, 248-49; J. George Frederick, ed., *The Swope Plan: Details, Criticisms, Analysis* (New York, 1931), 19-45; W. S. Myers and W. H. Newton, *The Hoover Administration: A Documented Narrative* (New York, 1936), 119, 155; Herbert Hoover, *The Memoirs of Herbert Hoover: The Great Depression, 1929-1941* (New York, 1952), 420; Joseph Dorfman, *The Economic Mind in American Civilization: 1918-1933* (New York, 1959), 4: 48-50; 5: 632-34; Hawley, *New Deal and Monopoly*, 42. See also, Matthew H. Elbow, *French Corporative Theory, 1789-1948: A Chapter in the History of Ideas* (New York, 1953), 122-67; and Roland Sarti, *Fascism and the Industrial Leadership in Italy, 1919-1940: A Study in the Expansion of Private Power under Fascism* (Berkeley, 1971), 69-104.

<sup>15</sup> When a reporter asked Roosevelt in November 1933, for his opinion of an updated version of the Swope Plan, he replied: "Mr. Swope's plan is a very interesting theoretical suggestion in regard to some ultimate development of N.R.A." Rosenman, *Public Papers*,

limitation of entry, and price and wage manipulation were common characteristics of government policy in both countries. So were the two governments' justifications of drastic and possibly illegal or unconstitutional<sup>16</sup> changes in the way the economy functioned on the ground that a "national emergency" existed, and the enormous propaganda campaigns they mounted to win public support.

The drafters of the National Industrial Recovery Act were not deliberately imitating fascist corporatism (although Hugh Johnson, a key figure among them, was an admirer of Mussolini). *Fortune*, which devoted an entire issue in 1934 to an analysis of the Italian system, was scarcely exaggerating when it stated that corporatism was "probably less well known in America than the geography of Tibet." As Gilbert H. Montague, a lawyer who had played a small role in the design of the code system, later wrote, the NRA was only "unconsciously" fascistic. It would be hard to find a better illustration of the common impact of the depression on two industrial nations committed to the preservation of capitalism.<sup>17</sup>

During the early stages big business interests dominated the new organizations and succeeded in imposing their views on government. In Germany the radical Nazi artisan socialists who wanted to smash the cartels and nationalize the banks, led by Gregor Strasser and Gottfried Feder, lost out to the powerful bankers and industrialists, represented by Hjalmar Schacht. In the United States victory went to the large corporations in each industry, which dominated the new code authorities.

But bewildering crosscurrents of interest and faction hampered the functioning of corporatism. In theory the system promised harmony and efficiency within industries, but in practice it seldom provided either. It did not even pretend to solve interindustry conflicts, yet these were often more disturbing to government authorities. Under corporatism workers were supposed to share fairly in decision making and in the rewards resulting from the elimination of conflict and competition; in both countries industrialists resisted allowing them to do so, with the con-

2: 447. Hoover also noted the connection between the Swope Plan and NRA. He entitled the chapter on the NRA in his memoirs, "Fascism Comes to Business." Hoover, *Great Depression*, 420-29.

<sup>16</sup> Although many Supreme Court decisions declaring New Deal laws unconstitutional reflected the unreasonably narrow views of five conservative justices, much of the early "emergency" legislation was very loosely worded. Furthermore the decision invalidating the NIRA (*Schechter Poultry Corp. v. United States*, 295 U.S. 495) was a unanimous one. On Roosevelt's unwillingness to abide by conventional constitutional limitations on federal power, see Arthur M. Schlesinger, jr., *The Politics of Upheaval* (Boston, 1960), 258, 452-53, and Paul L. Murphy, *The Constitution in Crisis Times: 1918-1969* (New York, 1972), 128-38, 246-47. Roosevelt "was under no illusion about the constitutional status of . . . his program," Schlesinger writes (p. 260), and Murphy calls him "a constitutional opportunist and pragmatist" (p. 246).

<sup>17</sup> Hawley, *New Deal and Monopoly*, 53-55, 66; Schlesinger, *Coming of the New Deal*, 96, 112-16, 153-54; *Fortune*, 10 (1934): 45; Gilbert H. Montague, "Is NRA Fascistic?" in American Academy of Political and Social Science, *Annals*, 180 (1935): 159.

sequence that the governments found themselves being pushed to enforce compliance. In America workingmen were a potent political force and a vital element in the New Deal coalition. German workers did not count as voters after 1933, but their cooperation and support remained essential to Nazi ambitions. Small businessmen also maintained a steady drum-fire of complaint, and both New Dealers and Nazis were sensitive to their pressure. Even the great industrialists were sometimes at odds with the system. Many German tycoons objected to sharing authority with labor and small producers, others to particular decisions imposed on the new estates by the government. German steel and chemical manufacturers like the Krupps and the I. G. Farben interests benefited from Hitler's emphasis on building up war-oriented industry and backed him enthusiastically, but producers dependent upon foreign raw materials or primarily concerned with the manufacture of consumer goods suffered from Nazi trade and monetary policies and held back. And as for the American industrialists, however much they profited under NRA codes, most of them came increasingly to resent the regimentation that codes entailed and to fear the growing interference in their affairs by bureaucrats.<sup>18</sup>

To the Nazis corporatism seemed at first compatible with the political process called *Gleichschaltung*, or coordination, a process by which nearly every aspect of life in their totalitarian state was brought under the control of Hitler and the party. It quickly became apparent, however, that the autonomous character of any corporatist organization made direct control from above difficult. America, fortunately, was never *gleichgeschaltet*. But, in any case, by 1935 and 1936 the Roosevelt and Hitler governments were abandoning corporatism and taking a more anti-big-business stance. In America this meant, aside from the demise of the NRA, more support for industrial labor, stricter regulation of public utilities, higher taxes on the rich and on corporations, rhetorical attacks on "economic royalists," and—by 1938—revived enforcement of the antitrust laws. In Germany, although the traditional cartel structure was retained, it involved limitations on corporate dividends; forced reductions in the interest rates paid on government bonds; government construction and operation of steel, automobile, and certain other facilities in competition with private enterprise; and higher taxes on private incomes and on corporate profits. As in the United States, but to a much greater degree, freedom of managerial decision making was sharply curtailed.<sup>19</sup>

<sup>18</sup> Schweitzer, *Big Business*, 114–30, 142–46, 156–76, 538–47; Schoenbaum, *Hitler's Social Revolution*, 115, 123; Cole, "Corporative Organization," 449–50; Lebovics, *Social Conservatism*, 134–35; Hawley, *New Deal and Monopoly*, 53–93; Irving Bernstein, *Turbulent Years: A History of the American Worker, 1933–1941* (Berkeley, 1969), 38–41, 217–18.

<sup>19</sup> Hawley, *New Deal and Monopoly*, 153–55, 301, 346–57, 420–55; Sidney Ratner, *Taxation and Democracy in America* (New York, 1967), 470–78; Irving Bernstein, *The New Deal Collective Bargaining Policy* (Berkeley, 1950), 112–28; J. Joseph Huthmacher, *Senator Robert F. Wagner and the Rise of Urban Liberalism* (New York, 1968), 189–98; Erbe, *Wirtschaftspolitik*,

The success or failure of American and German efforts to stimulate industrial recovery is a separate question not central to my argument here. What is central to the argument is this: both were marked by vacillation, confusion, and contradictions, by infighting within the administering bureaucracies, by an absence of any consistently held theory about either the causes of the depression or how to end it. Both also subordinated economic to political goals. The "primacy of politics" in Nazi Germany is a commonplace, its most glaring expression occurring in 1936 when shortages of raw materials and foreign exchange led Hitler to choose between guns and butter. He chose, of course, guns. The problem could be solved by an act of will, he insisted; it was the task of the economy to supply the military needs of the state—so be it! When Schacht, his chief economic adviser, urged a more balanced use of available resources, the Führer fired him. Such ruthless subordination of economic interests to the state did not occur in the United States, although when military considerations began to dominate American policy after 1939 Roosevelt was also prepared to substitute guns for butter. I need only mention his famous announcement that he was replacing "Dr. New Deal" with "Dr. Win the War" as his prime consultant.<sup>20</sup>

But conventional "politics"—the accommodation of political leaders to the pressures of interest groups—affected economic policy in both nations. Beset by business interests seeking aid, by trust busters eager to break up the corporate giants, by planners brimming with schemes to rationalize the economy, the Roosevelt administration survived in a state of constant flux, making concessions to all views, acting in contradictory and at times self-defeating ways. "The New Dealers," writes Ellis W. Hawley, "failed to arrive at any real consensus about the origins and nature of economic concentration." Nor did they follow any consistent policy in the fight against the industrial depression. And Roosevelt's inconsistency, as Hawley also notes, "was the safest method of retaining political power, . . . a political asset rather than a liability." The Nazis, as I have shown, also permitted pressures from various economic interests to influence policy. They did so partly because even a totalitarian dictatorship could profit from the active cooperation of powerful economic groups and partly because the Nazi party had no fixed economic beliefs. Roosevelt responded to pressure groups, Hitler for a time suffered them to exist—a most vital distinction—but the practical result was the same. Put differently, Hitler

83-87, 143-44, 181-82; Klein, *Germany's Economic Preparation for War*, 42-44; Enno Georg, *Die wirtschaftlichen Unternehmungen der SS* (Stuttgart, 1963), 42-58; Hamburger, *How Nazi Germany Controlled Business*, 67-70; Rämisch, "Berufsständische Gedanke," 270-72. The effects of fascist policy on the power of business leaders are brilliantly described in Ernst Basch [E. B. Ashton] *The Fascist: His State and His Mind* (New York, 1937), 110-16.

<sup>20</sup> T. W. Mason, "The Primacy of Politics—Politics and Economics in National Socialist Germany," in S. J. Woolf, ed., *The Nature of Fascism* (New York, 1968), 165-95; Petzina, *Autarkiepolitik*, 35-36, 49; Wilhelm Grottkopf, *Die grosse Krise: Lehren aus der Überwindung der Wirtschaftskrise, 1929/32* (Dusseldorf, 1954), 299-300.

had a clear political objective—it was actually an obsession—but he was almost as flexible about specific economic policies as Roosevelt. “As regards economic questions,” he boasted in 1936, “our theory is very simple. We have no theory at all.”<sup>21</sup>

New Deal and Nazi labor policies were also shaped by the Great Depression in related ways. On the surface this statement may appear not simply incorrect but perverse, but only because of our tendency to identify labor with unionization. It is true that Hitler totally destroyed the German unions and that Roosevelt, in part unwittingly and surely with some reluctance, enabled American unions to increase their membership and influence enormously. But New Deal and Nazi policies toward unions had little to do directly with the depression and throw little light on the national policies toward workingmen. Hitler would no doubt have destroyed the Weimar unions as autonomous organizations in any case—he destroyed all autonomous organizations in Germany. But it was because they were anti-Nazi that he smashed the unions so quickly. Roosevelt was at first indifferent to organized labor; he encouraged the American unions in order to gain labor’s support, not to speed economic recovery. In each instance the decision was essentially political.<sup>22</sup>

It is not difficult to demonstrate Nazi concern for industrial workers. The “battle against unemployment” had first priority in 1933, and it was won remarkably swiftly; by 1936 something approaching full employment existed in Germany and soon thereafter an acute shortage of labor developed. Of course the military draft siphoned thousands of men out of the German labor market, contributing to the shortage, but this was also true in the United States after 1940. Certainly full employment was never approached in America until the economy was shifted to all-out war production.<sup>23</sup>

Moreover, Nazi ideology (and Hitler’s prejudices) inclined the regime to favor the ordinary German over any elite group. Workers—as distinct from “Marxist” members of unions—had an honored place in the system. To the extent that the Nazis imposed restrictions on labor, they did so for

<sup>21</sup> Hawley, *New Deal and Monopoly*, 470, 476; Mason, “Primacy of Politics,” 173–74; Cole, “Corporative Organization,” 449; Daniel R. Fusfeld, *The Economic Thought of Franklin D. Roosevelt and the Origins of the New Deal* (New York, 1956), 254. This does not mean, of course, that the ideas of American and German economists were not taken up by the politicians. Roosevelt was influenced by dozens of them; he did not, however, adopt any consistent line of economic reasoning. Both the Nazi program to create work and their post-1936 autarchic policies were apparently anticipated in Robert Friedlaender-Precht’s *Die Wirtschaftswende* (Leipzig, 1931), although Friedlaender-Precht, whose father was Jewish, was unable to publish or exert any direct influence on economic policy after 1933. See Grottkopf, *Die grosse Krise*, 35–36, 262; Kroll, *Weltwirtschaftskrise*, 435–55.

<sup>22</sup> Schoenbaum, *Hitler’s Social Revolution*, 74–75; Bernstein, *Turbulent Years*, 699, 778–79; Bernstein, *New Deal Collective Bargaining*, 82, 121–22, 127–28, 130–31.

<sup>23</sup> American unemployment never fell much below 8 million during the New Deal. In 1939 about 9.4 million were out of work, and at the time of the 1940 census (in March) unemployment stood at 7.8 million, almost fifteen per cent of the work force. *Historical Statistics of the United States, Colonial Times to 1957* (Washington, 1960), 73; Broadus Mitchell, *Depression Decade: From New Era through New Deal, 1929–1941*, (New York, 1947), 95.

the benefit of the state, not of employers. In a sense the Nazi Courts of Social Honor may even be compared with the New Deal National Labor Relations Board. These courts did not alter power relationships between capital and labor as the NLRB did; they represented the interests of the Nazi party rather than those of labor. But they did adjudicate disputes between workers and bosses, and there is considerable evidence that the Courts of Social Honor tended more often than not to favor workingmen in these disputes. Furthermore the very existence of these courts put considerable psychological pressure on employers to treat labor well.<sup>24</sup>

It is beyond argument that the Nazis encouraged working-class social and economic mobility. They made entry into the skilled trades easier by reducing the educational requirements for many jobs and by expanding vocational training. They offered large rewards and further advancement to efficient workers, and, in the Strength Through Joy movement, they provided extensive fringe benefits, such as subsidized housing, low-cost excursions, sports programs, and more pleasant factory facilities. Eventually the Nazi stress on preparation for war meant harder work, a decline in both the quantity and quality of consumer goods, and the loss of freedom of movement for German workers, but the hierarchy imposed these restrictions and hardships belatedly and very reluctantly because of its desire to win and hold the loyalty of labor. If the question is: "Did the Nazi system give workers more power?" the answer of course is that it did not. But that question, albeit important, has little to do with the actual economic position of workingmen or with the effectiveness of the Nazi system in ending the depression.<sup>25</sup>

NEW DEAL AND NAZI methods of dealing with the agricultural depression also had much in common. Both sought to organize commercial agriculture in order to increase farm income, under the New Deal Agricultural Adjustment Act through supposedly democratic county committees to control production, in Germany through the centralized Estate for Agriculture. The purpose was to raise agricultural prices and thus farm income through

<sup>24</sup> Schoenbaum, *Hitler's Social Revolution*, 73-90; Basch [Ashton], *The Fascist*, 92, 94; Erbe, *Wirtschaftspolitik*, 88-95; T. W. Mason, "Labour in the Third Reich," *Past and Present*, no. 33 (1966): 112-41; Guillebaud, *Economic Recovery of Germany*, 187-95; Karl D. Bracher, *The German Dictatorship: The Origins, Structure, and Effects of National Socialism* (New York, 1970), 332-35, 339.

<sup>25</sup> Schoenbaum, *Hitler's Social Revolution*, 95-112; Guillebaud, *Economic Recovery of Germany*, 196-212; Hans-Gerd Schumann, *Nationalsozialismus und Gewerkschaftsbewegung: Die Vernichtung der deutschen Gewerkschaften und der Aufbau der "Deutschen Arbeitsfront"* (Hanover, 1958), 132-34, 138-41, 144-45; Herbert Steinwarz, "The Amenities of Industry and Labour in Germany," *International Labour Review*, 36 (1937): 772-79; K. Mandelbaum, "An Experiment in Full Employment: Controls in the German Economy, 1933-1938," in Oxford University Institute of Statistics, *The Economics of Full Employment* (New York, 1967), 181, 194-201.

a system of subsidies, paid for in each instance by processing taxes that fell ultimately on consumers. Both governments also made agricultural credit cheaper and more readily available and protected farmers against loss of their land through foreclosures.

These similarities are not remarkable; nearly every nation sought, more or less in these ways, to bolster agricultural prices and protect its farmers. What is interesting, given the profound differences between American and German agriculture, is the attitudes of the two governments toward the place of farmers in the society and toward rural life. Although there was no American counterpart to Hitler's racist, anti-intellectual glorification of the German peasantry, Nazi thinking was at least superficially similar to that of generations of American farm radicals. (David Schoenbaum has aptly called Gottfried Feder "a kind of Central European William Jennings Bryan.") The typical American farmer was no more like a German peasant than the owner of a Southern plantation was like a Junker, but under the impact of the depression farmers large and small in both countries were expressing the same resentments and demands, and these affected Nazi and New Deal policies in related ways. Furthermore, the ideas of Roosevelt and Hitler about farmers were quite alike. Both tended to romanticize rural life and the virtues of an agricultural existence. They hoped to check the trend of population movement to the cities and to disperse urban-centered industries. Roosevelt spoke feelingly of the value of close contact with nature and of the "restful privilege of getting away from pavements and from noise." Only in the country, he believed, did a family have a decent chance "to establish a real home in the traditional American sense." He did not deny the attractions of city life, but he argued that electricity, the automobile, and other modern conveniences made it possible for rural people to enjoy these attractions without abandoning the farm. While governor of New York he set up a program for subsidizing unemployed city families on farms so that "they may secure through the good earth the permanent jobs they have lost in over-crowded cities and towns."<sup>26</sup>

Hitler called the German peasantry "the foundation and life source" of the state, "the counterbalance to communist madness," and "the source of national fertility." The superiority of rural over urban life was a Nazi dogma—especially the life of the self-sufficient small farmer, free from the dependency and corruption of a market economy. "The fact that a people is in a position to nourish itself from its own land and through that to lead its own life independent of foreign nations has always in

<sup>26</sup> Frank Freidel, *Franklin D. Roosevelt: The Triumph* (Boston, 1956), 224-26; Fusfield, *Economic Thought of FDR*, 84, 124-30; Franklin D. Roosevelt, "Back to the Land," *Review of Reviews*, 84 (1931): 63-64; State of New York, *Public Papers of Governor Franklin D. Roosevelt: 1931* (Albany, 1937), 752-59, 781-82. "Is it worthwhile," Roosevelt asked in a radio address late in 1931, "for us to make a definite effort to get people in large numbers to move out of cities . . . ? It seems to me that to that question we must answer an emphatic YES." *Ibid.*, 782.



history been significant," a Nazi agricultural expert wrote in 1935. "Families on the land also have the biological strength to maintain themselves and to compensate for population losses resulting from migration to cities and from war." Nazi leaders referred to Berlin as "Moloch Berlin" and deplored the influx of Germans from the east into the capital. The Nazis' housing policy sought to stimulate suburban development in order to bring industrial workers closer to the land and to reduce urban crowding. They placed all construction under government control, made funds available for low-interest, state-guaranteed mortgage loans, and provided tax relief to builders of small apartments and private homes.<sup>27</sup>

The Tennessee Valley Authority and the rural electrification program made important progress toward improving farm life, but efforts to reverse the population trend yielded very limited results. As president, Roosevelt dreamed of decentralizing industry and of relocating a million families on small farms, but during the whole of the New Deal his Resettlement Administration placed fewer than 11,000 families on the land; even the best-known of the settlements, Arthurdale in West Virginia, which benefited from the particular interest and financial support of Eleanor Roosevelt, never became a viable community until the outbreak of the war. That the Resettlement Administration was run by Rexford Tugwell, who considered the back-to-the-land movement impracticable, contributed to the ineffectiveness of this program, but the agency's greenbelt town program of planned suburban development, which Tugwell did think practicable, also produced miniscule results—only three of the sixty originally planned greenbelt towns were built.<sup>28</sup>

Although Nazi ideologues hoped to reverse Germany's urban-rural ratio, which was seventy per cent urban in 1933, their rural resettlement and "rurban" programs proved equally disappointing. Between 1933 and 1938 the Nazis resettled about 20,000 families, but this was scarcely more than half the number the Weimar government had managed to relocate between 1927 and 1932. Nor did the German "rurban" development program ever get very far off the ground. In both the American and German cases efforts to check the movement of population to the cities foundered

<sup>27</sup> Domarus, *Reden und Proklamationen*, 1: 174, 234-35, 304; Schoenbaum, *Hitler's Social Revolution*, 153-54; Martin Busse, "Bauer und Boden," in Otto Mönckmeier, ed., *Jahrbuch für nationalsozialistische Wirtschaft* (Stuttgart, 1935), 65-66; Johann W. Ludowici, *Das deutsche Siedlungswerk* (Heidelberg, 1935), 15; Walter Fey, "Leistungen und Aufgaben im Deutschen Wohnungs- und Siedlungsbau," *Institut für Konjunkturforschung*, special vol. 42 (Berlin, 1936), 24-29.

<sup>28</sup> Sidney Baldwin, *Poverty and Politics: The Rise and Decline of the Farm Security Administration* (Chapel Hill, 1968), 74-75, 88, 104-08; Joseph L. Arnold, *The New Deal in the Suburbs: A History of the Greenbelt Town Program, 1935-1954* (Columbus, 1971), 28-50, *passim*; Paul K. Conkin, *Tomorrow a New World: The New Deal Community Program* (Ithaca, 1959), 164-67, 237-55, 305-31; Joseph P. Lash, *Eleanor and Franklin: The Story of Their Relationship, Based on Eleanor Roosevelt's Private Papers* (New York, 1971), 393-417; Edward S. Shapiro, "Decentralist Intellectuals and the New Deal," *Journal of American History*, 58 (1972): 948-49. In a speech in April 1934, Roosevelt called the resettlement program "one of my pet children." Rosenman, *Public Papers*, 3: 199.

on the opposition of real-estate and construction interests and still more on the conflicting objectives of government policy makers and their unwillingness to allocate sufficient funds to enable much progress to be made. In 1937 Roosevelt established the Farm Security Administration to coordinate the various New Deal rural rehabilitation programs, but again relatively little was accomplished. Large sums were made available to help tenants buy their own farms, but local agents, concerned for the sake of their own records with making sure that the money was repaid, tended to make loans to tenants who were better off rather than to the most poverty stricken. While many families benefited, the overall impact upon American agriculture was negligible. The sums spent were measured in the millions, whereas, as one critic put it, only if billions had been appropriated could "the drift into tenancy and degradation be stopped and reversed." In Germany the very success of the Nazis in ending unemployment and their post-1936 drive to build their war machine created a shortage of industrial labor that made a meaningful back-to-the-soil movement impossible.<sup>29</sup>

There were significant differences between the objectives of American and National Socialist agricultural policies, the former, for example, seeking to limit output, the latter to increase it. All in all, the New Deal was the more successful in solving farm problems; far less was accomplished in Germany toward modernizing and mechanizing agriculture during the thirties. On the other hand, Nazi efforts in behalf of farm laborers were more effective than those of the New Deal; the AAA programs actually hurt many American agricultural laborers and also tenants and sharecroppers. In both nations agricultural relief brought far more benefits to large landowners than to small.<sup>30</sup>

The complications of German and American monetary and fiscal policies during the depression and of questions relating to foreign trade preclude their detailed discussion here. I shall only mention a few common themes. Both nations increased government control of the banking system but did not nationalize the banks. Both, following the precepts of economic nationalism, sought to improve the competitive position of their export industries, the Americans by devaluing the dollar, the Germans by subsidies, both by sequestering the national gold supplies and prohibiting the export of gold. Both paid most of the costs of their recovery programs by deficit

<sup>29</sup> Schweitzer, *Big Business*, 202-65; Arthur Schweitzer, "On Depression and War: Nazi Phase," *Political Science Quarterly*, 62 (1947): 332-38; Ludovici, *Das deutsche Siedlungswerk*, 1-12, 66-85; Baldwin, *Poverty and Politics*, 212, 218; Shapiro, "Decentralist Intellectuals," 950-51.

<sup>30</sup> Edwin Nourse, Joseph Davis, and John D. Black, *Three Years of the Agricultural Adjustment Act* (Washington, 1937); David E. Conrad, *The Forgotten Farmers: The Story of Sharecroppers in the New Deal* (Urbana, 1966); Schoenbaum, *Hitler's Social Revolution*, ch. 5. See also, Karl D. Bracher, Wolfgang Sauer, and Gerhard Schulz, *Die nationalsozialistische Machtergreifung* (Cologne, 1960), 570-78.

financing. However, both also ignored the newly developing Keynesian economics and remained inordinately fearful of inflation.<sup>31</sup>

Nazi and New Deal policies were not essentially different from those of other industrial nations in these respects. However, they adopted them sooner and pursued them more vigorously than, for example, the British or the French. Thus when Roosevelt decided against international stabilization of foreign exchange rates and thus "torpedoed" the London Economic Conference, the British and French were bitterly disappointed but the Germans were delighted. Roosevelt's opinion, expressed in his "bomb-shell" message, that "the sound internal economic situation of a nation is a greater factor in its well-being than the price of its currency," was Nazi orthodoxy. In a radio message beamed to the United States the German foreign minister, Konstantin von Neurath, praised Roosevelt's "fearlessness" and spoke of the "heroic effort of the American people . . . to overcome the crisis and win a new prosperity." Reichsbank president Hjalmar Schacht told a *Völkischer Beobachter* reporter that FDR had adopted the philosophy of Hitler and Mussolini: "Take your economic fate in your own hand and you will help not only yourself but the whole world."<sup>32</sup>

In the early months of the New Deal Roosevelt toyed with the idea of stimulating exports by means of subsidized dumping and by barter agreements, trade tactics that the Nazis adopted wholeheartedly. A mighty behind-the-scenes battle was fought within the administration in 1933 and 1934 between supporters of this approach and those who believed in lowering tariff barriers by making reciprocal trade agreements based on the unconditional most-favored-nation principle. Rexford Tugwell, Raymond Moley, and George W. Peek argued the former position; the secretary of state, Cordell Hull, and the secretary of agriculture, Henry Wallace, the latter; and the president—after one of his typical attempts to get the protagonists to reconcile the irreconcilable—finally sided with Hull and Wallace. Roosevelt nevertheless continued for some time to flirt with the idea of bilateral agreements, especially one suggested by Schacht involving 800,000 bales of American cotton. The rejection of this and similar proposals resulted more from Roosevelt's growing political and moral distaste for Hitlerism than from economic considerations. But despite Roosevelt's

<sup>31</sup> Erbe, *Wirtschaftspolitik*, 166–72; Schweitzer, *Big Business*, 342–52; James M. Burns, *Roosevelt: The Lion and the Fox* (New York, 1956), 328–36; Robert Lekachman, *The Age of Keynes* (New York, 1968), 113–16, 138–43; Perkins, *Roosevelt I Knew*, 225–26; J. Ronnie Davis, *The New Economics and the Old Economists* (Ames, Iowa, 1971), 151–53; Rexford G. Tugwell, *The Democratic Roosevelt* (New York, 1957), 373–75; Roy F. Harrod, *The Life of John Maynard Keynes* (London, 1951), 448–49.

<sup>32</sup> Rosenman, *Public Papers*, 2: 264–65; Leuchtenburg, *Roosevelt and the New Deal*, 202–03; Maurice Vaïsse, "Le mythe de l'or en France: les aspects monétaires du New Deal vus par les Français," *Revue d'Histoire Moderne et Contemporaine*, 16 (1969): 464–65; Hans-Jürgen Schröder, *Deutschland und die Vereinigten Staaten, 1933–1939: Wirtschaft und Politik in der Entwicklung des Deutsch-Amerikanischen Gegensatzes* (Wiesbaden, 1970), 88, 91. See also, Hjalmar Schacht, *76 Jahre meines Lebens* (Bad Wörishofen, 1953), 395.

antifascism and the internationalist, free-trade rhetoric of the reciprocal trade program, it seems clear that New Deal foreign policy was as concerned with advancing national economic interests as was German policy. State Department alarm at Nazi "penetration" of Latin America, publicly expressed in strategic and moral terms, had a solid base in lost and threatened markets for American exports.<sup>33</sup>

THERE REMAINS the question of leadership, that is, of the personal roles of Roosevelt and Hitler in their nations' campaigns against the Great Depression. To overemphasize Roosevelt and Hitler as individuals would be to approach the problem simplistically, but certain parallels merit examination. It cannot be proved that neither would have achieved national leadership without the depression, but the depression surely contributed to the success of each. Yet on the surface they seem most improbable leaders of the two countries at that particular time. In an economic crisis of unprecedented severity, neither had a well-thought-out plan. Both lacked deep knowledge of or even much interest in economics.<sup>34</sup>

It is no less than paradoxical that the American electorate, provincial in outlook, admiring of self-made men and physical prowess, and scornful of "aristocrats," should, at a time when millions were existing on the edge of starvation, choose for president a man who lived on inherited wealth, who came from the top of the upper crust, who had been educated in the swankest private schools, who had a broad cosmopolitan outlook, and (to descend to a lesser but not politically unimportant level) who was a cripple. But no more a paradox than that a country whose citizens

<sup>33</sup> Schröder, *Deutschland und die Vereinigten Staaten*, 155-59, 253; Hans-Jürgen Schröder, "Die Vereinigten Staaten und die nationalsozialistische Handelspolitik gegenüber Lateinamerika 1937/38," *Jahrbuch für Geschichte von Staat, Wirtschaft und Gesellschaft Lateinamerikas*, 7 (1970): 309-23, 355-57; Arnold A. Offner, *American Appeasement: United States Foreign Policy and Germany, 1933-1938* (Cambridge, Mass., 1969), 99-102; Lloyd C. Gardner, *Economic Aspects of New Deal Diplomacy* (Boston, 1971), 42-43, 100; Cordell Hull, *The Memoirs of Cordell Hull* (New York, 1948), 1: 353-77; Schlesinger, *Coming of the New Deal*, 187-88, 191-93, 255-58. The reciprocal trade program, despite the high hopes of its supporters, had little effect upon American trade or the world economy. Furthermore the argument of Secretary Hull and others that the "non-discriminatory" reciprocal trade policy was particularly high-minded and that bilateralism and barter agreements were per se destructive of the interests of underdeveloped nations makes little sense. See Schlesinger, *ibid.*, 259-60; and Mandelbaum, "Experiment in Full Employment," 186-87.

<sup>34</sup> Mason, "Primacy of Politics," 173-74; Perkins, *Roosevelt I Knew*, 34; Burns, *Roosevelt*, 329, 334-36. Even Fustfeld, who argues that Roosevelt "had a well-developed economic philosophy," admits that it was not derived from the writings of economists or other thinkers. Roosevelt's own opinion is perhaps revealed in a remark he made to Marriner Eccles after listening to a debate between Eccles and a conservative senator: "You made the problem so simple that even I was able to understand it." Fustfeld, *Economic Thought of FDR*, 5; Davis, *New Economics*, 152. John D. Heyl argues that "Hitler's economic ideas are worth considering," but in essence claims little more than that "Hitler acquired a certain familiarity with economic issues." And he quotes Hitler as saying in 1934: "Don't allow yourself to be deceived by cut-and-dried [economic] theories. Certainly I know less today about these matters than I thought I knew a few years ago." John D. Heyl, "Hitler's Economic Thought: A Reappraisal," *Central European History*, 6 (1973): 83, 92.

were supposed to have an exaggerated respect for hard work, for education and high culture and family lineage, and who had a reputation for orderliness and social discipline should follow the lead of a high-school dropout, a lazy ne'er-do-well, a low-born Austrian who could not even speak good German, the head of a rowdy movement openly committed to disorder and violence. Equally strange, Roosevelt and Hitler appealed most strongly to their social and economic opposites: Roosevelt to industrial workers, to farmers, to the unemployed and the rejected; Hitler to hard-working shopkeepers and peasants, and, eventually, to industrialists, great landowners, and the military.

It may of course be true that these seeming contradictions are of no significance. Probably any Democrat would have defeated Hoover in 1932, and although Hitler became chancellor in a technically legal way, his subsequent seizure of total power was accomplished without the consent, if not necessarily without the approval, of a majority of the German people. Yet the personal impact of Roosevelt and Hitler on the two societies in the depths of the Great Depression was very large. Their policies aside, both exerted enormous psychological influence upon the citizenry. Roosevelt's patrician concern for mass suffering, his charm, his calm confidence, his gaiety, even his cavalier approach to the grave problems of the day had, according to countless witnesses, an immediate and lasting effect upon the American people. Hitler's resentment of the rich and well born, however psychotic in origin, appealed powerfully to millions of Germans. His ruthless, terrifying determination, always teetering on the edge of hysteria, combined with the aura of encapsulated remoteness that he projected to paralyze those who opposed him, to reduce most of his close associates to sycophancy, and to inspire awe among masses of ordinary Germans. Both the euphoria of the Hundred Days and the nationalistic fervor that swept Germany in the early months of 1933 made millions almost incapable of thought, let alone of judgment. Bills swept through Congress ill drafted and scarcely debated, basic rights were abolished in Germany without even an attempt at resistance, and both were possible largely because of the personalities of the two leaders.<sup>35</sup>

Much of this was probably spontaneous, but not all. Roosevelt and Hitler employed the latest technologies to dramatize themselves and to influence public opinion. Roosevelt's flight to Chicago to accept the Democratic nomination and Hitler's whirlwind tours testify to their swift grasp of the psychological as well as the practical value of air travel to politicians. And no greater masters of the radio ever lived—Roosevelt with his low-keyed, fatherly, intimate fireside chats, Hitler with his shrill harangues beneath the massed swastikas at Nuremberg. Both were terrible

<sup>35</sup> See, among many examples, Leuchtenburg, *Roosevelt and the New Deal*, 44-45, and Helmut Krausnick, "Stages of Co-ordination," in Lawrence Wilson, tr., *The Road to Dictatorship* (London, 1964), 132-33.

administrators in the formal sense but virtuosos at handling subordinates. Their governments were marked by confusion, overlapping jurisdictions, and factional conflicts, yet somehow they transformed these inadequacies into political assets—symbols not of weakness or inefficiency but of energy and zeal in a time of grave emergency.<sup>36</sup>

Both also made brilliant use of the crisis psychology of 1933, emphasizing the suffering of the times rather than attempting to disguise or minimize it. "The misery of our people is horrible," Hitler said in his first radio address after becoming chancellor. "To the hungry unemployed millions of industrial workers is added the impoverishment of the whole middle class and the artisans. If this decay also finally finishes off the German farmers we will face a catastrophe of incalculable size."<sup>37</sup> Roosevelt's personal style was more reassuring than alarmist, but he also stressed the seriousness of the situation and the urgent need for decisive action: "Action, and action now," as he put it in his inaugural.

Both the Roosevelt and Hitler governments tried to influence public opinion in new and forceful ways. Roosevelt did not create a propaganda machine even remotely comparable to Goebbels's, but under the New Deal the government undertook efforts unprecedented in peacetime to sell its policies to the public. The NRA slogan "We Do Our Part" served the same function as the Nazis' incessantly repeated *Gemeinnutz geht vor Eigennutz*. With Roosevelt's approval, General Hugh Johnson, head of the NRA and designer of its Blue Eagle symbol, organized a massive campaign to rally support for the NRA. "Those who are not with us are against us," Johnson orated, "and the way to show that you are a part of this great army of the New Deal is to insist on this symbol of solidarity." Johnson denounced "chiselers" and "slackers"; his office plastered the land with billboard displays; distributed posters, lapel buttons, and stickers; dispatched volunteer speakers across the country; and published *Helpful Hints* and *Pointed Paragraphs* to provide them with The Word. Roosevelt

<sup>36</sup> Judgments of Roosevelt's and Hitler's abilities as administrators are of course highly subjective. Furthermore the internal workings of any government seem confused when examined in detail. It is clear nevertheless that both Roosevelt and Hitler were exceptionally prone to set up confused lines of responsibility among their subordinates and to tolerate and at times encourage interdepartmental and intradepartmental rivalries. Waste and ineffectiveness frequently resulted. Thus, Burns writes: "Again and again Roosevelt flouted the central rule of administration that the boss must co-ordinate the men and agencies under him. . . . [He] put into the same office or job men who differed with each other in temperament and viewpoint." Bracher concludes that "friction, waste, duplication" were deliberate Hitlerian techniques, whereas Fischer argues that when Hitler "made at least two and most often many more boards, agencies, and men responsible for each assignment" he was merely revealing the senselessness and lack of guiding principles of his system, but they (and other scholars) agree as to the facts. Burns considers Roosevelt "an artist in government," master of the technique of divide and rule, whose "first concern was power," the subjection of the bureaucracy to executive control. Bracher writes that Hitler displayed "matchless virtuosity" in making others dependent upon him. "The Leader was the sole figure standing above the confusion of jurisdictions and command chains." Burns, *Roosevelt*, 371-75; Bracher, *German Dictatorship*, 212; Fischer, *Deutsche Wirtschaftspolitik*, 77.

<sup>37</sup> Domarus, *Reden und Proklamationen*, 1: 192.



*Fig. 1.* Photograph by Dorothea Lange. Evicted sharecropper from Arkansas, 1936.  
Library of Congress.



*Fig. 2. Painted in Germany, 1930s. Artist unidentified. Photograph courtesy  
Suddeutscher Verlag, Munich.*



# IS HOUSING A PUBLIC RESPONSIBILITY?

**Government Money & Regulation Guarantee...**

## FREE SCHOOLS



## CHEAP POSTAL SERVICE

The postal system has cost the American taxpayer an average of more than 125 million dollars a year for the last five years.

## BETTER TRAINS

State and federal governments have given more than a billion dollars to the railroads, in the form of land grants and other aids. Gifts of land totalled more than 175 million acres, an area larger than the state of Texas.

## Is the American Home LESS IMPORTANT?



## BETTER SHIPS

The government today is paying about \$20,000,000 annually for the carrying of mails which would cost only \$3,000,000. The difference, \$27,000,000, is a subsidy, and nothing but a subsidy.

—President Franklin D. Roosevelt, March 1935

## BETTER HIGHWAYS

The federal government has spent an average of 26 million dollars a year on highways for the last five years.

## BETTER AIR LINES

To aid American airlines, the federal government has paid as much as 75% of the cost of mail service for the last five years.



Fig. 3. Poster from a traveling series of information panels prepared by the U.S. Resettlement Administration, 1935. Library of Congress.

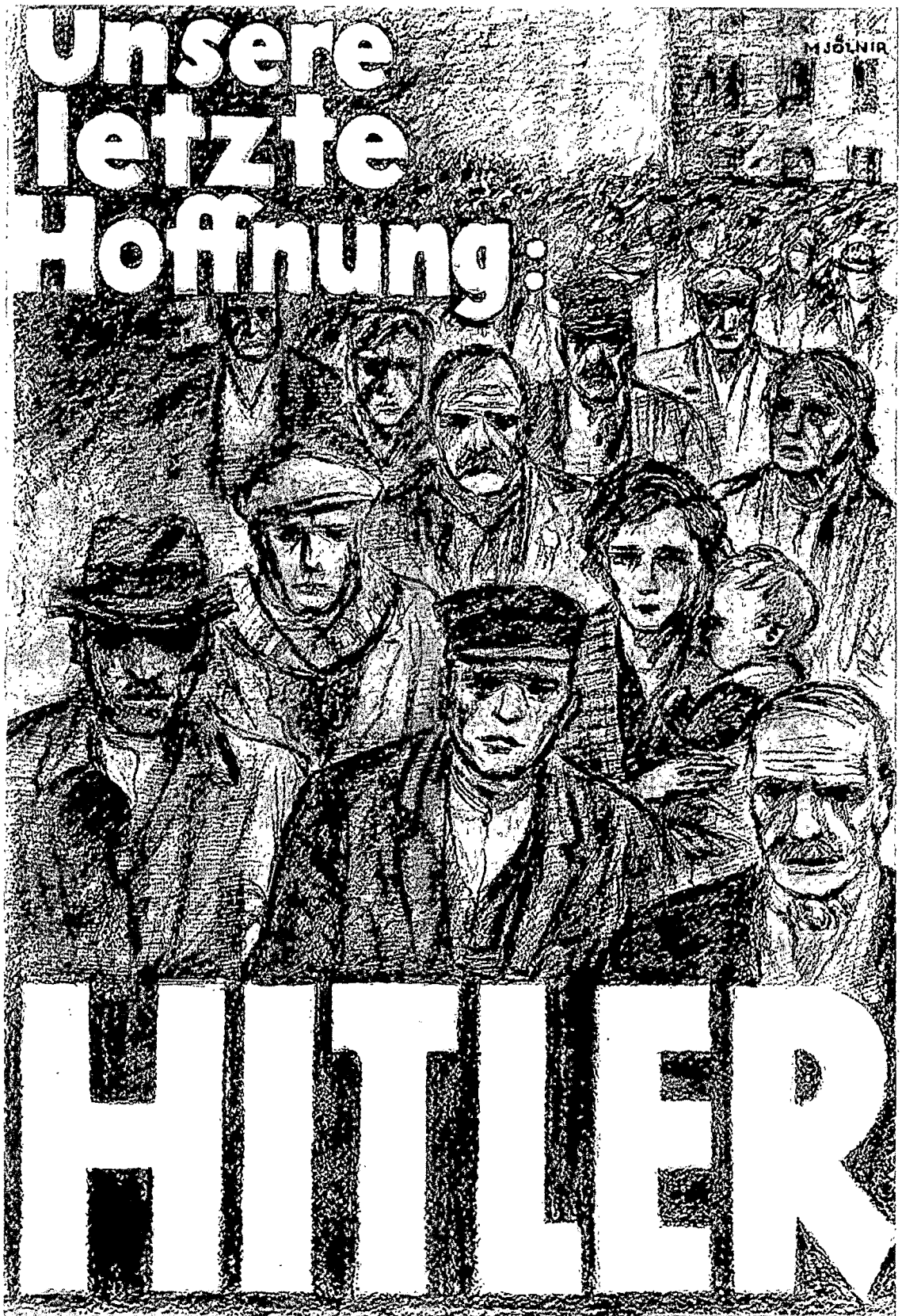


Fig. 4. "Our last hope: Hitler." German election poster, 1932. Library of Congress.

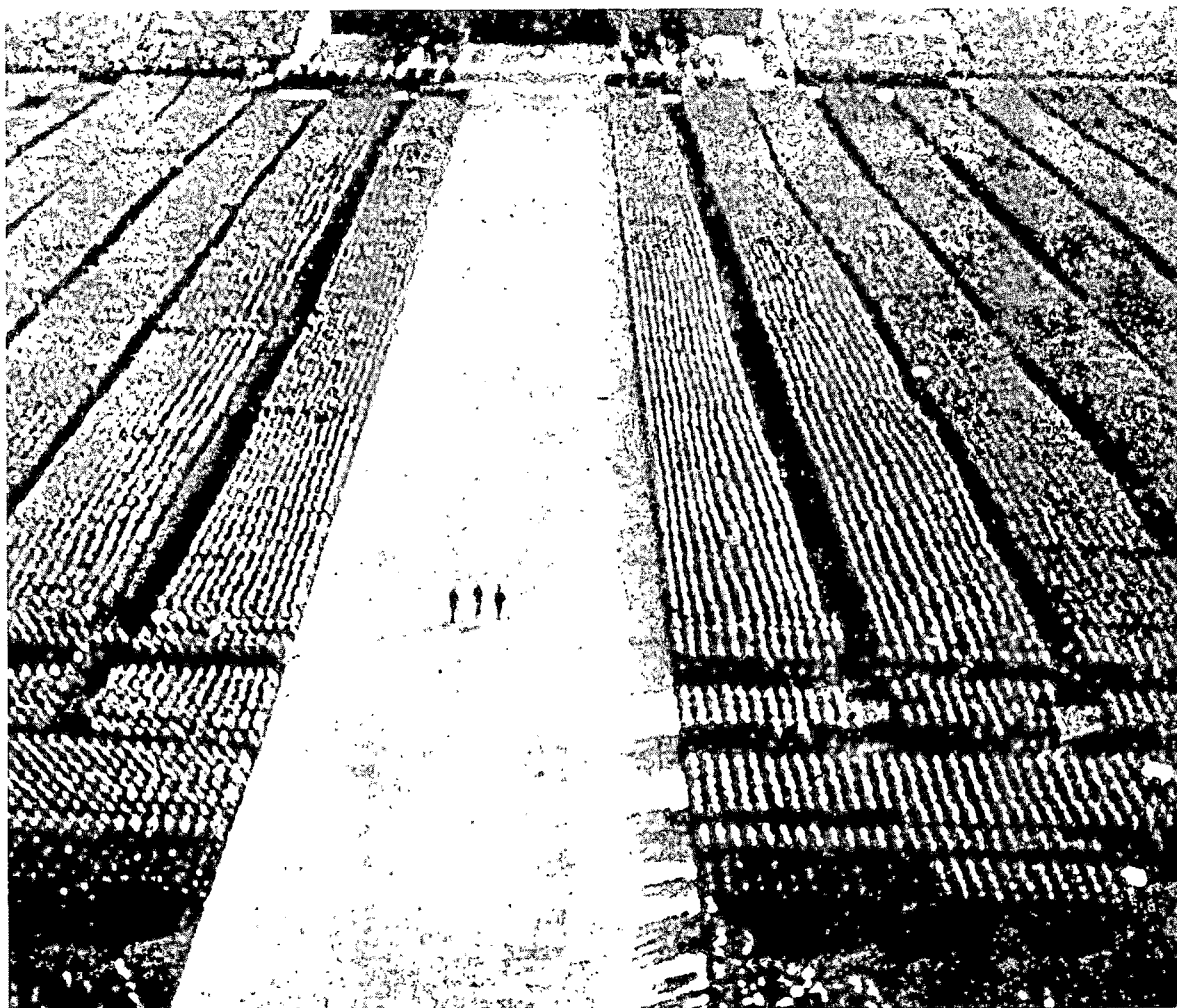


Fig. 5. Hitler reviewing troops during Nuremberg rally, 1936. Still from *Triumph of the Will*. Photograph courtesy Museum of Modern Art, Film Stills Archive.

himself, in a fireside chat, compared the Blue Eagle to a “bright badge” worn by soldiers in night attacks to help separate friend from foe. Placed beside the awesome Nazi displays at Nuremberg, even the ten-hour, 250,000-person NRA parade up Fifth Avenue in September 1933 may seem insignificant, but it and other NRA parades and hoopla were designed to serve the same functions: rousing patriotic feelings and creating in the public mind the impression of so extensive a support for government policies as to make disagreement appear close to treason. As Johnson himself explained, the purpose was to “put the enforcement of this law into the hands of the *whole* people.”<sup>38</sup>

<sup>38</sup> Hawley, *New Deal and Monopoly*, 53–55; Schlesinger, *Coming of the New Deal*, 112–16; Hugh S. Johnson, *The Blue Eagle from Egg to Earth* (New York, 1935), 255–58, 261, 264; Rosenman, *Public Papers*, 2: 301; Leuchtenburg, “Analogue of War,” 121, 133–34. A 1935

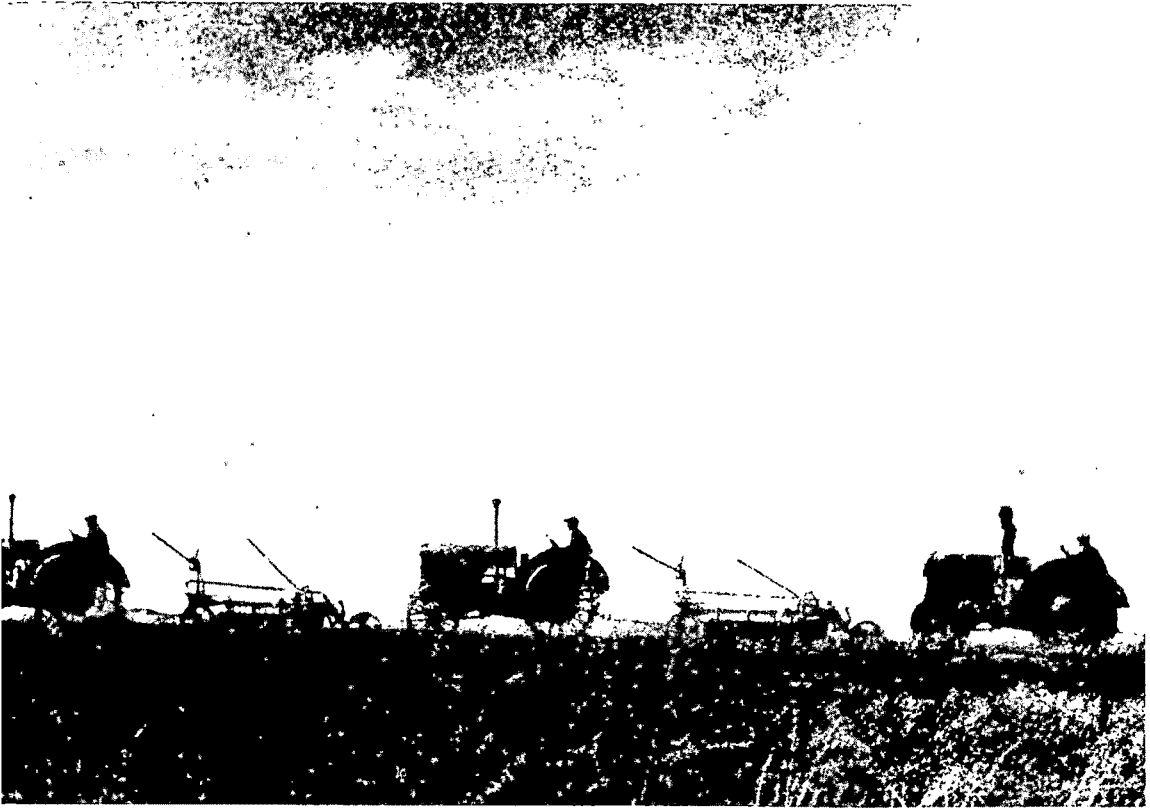


Fig. 6. Part of a battalion of tractors. Still from *The Plow That Broke the Plains*, 1936. Photograph courtesy Museum of Modern Art, Film Stills Archive.

Another example of New Deal propaganda is provided by the efforts of the Resettlement Administration and the Farm Security Administration under Rexford Tugwell. Because Pare Lorenz's government-sponsored films, *The Plow That Broke the Plains* (1936) and *The River* (1938), and the still photographs of Dorothea Lange, Walker Evans, Margaret Bourke-White, Gordon Parks, and others were esthetic achievements of the highest order, we tend to forget that they were a form of official advertising designed to explain and defend the New Deal approach to rural social and economic problems. They differed from Leni Riefenstahl's *Triumph of the Will* (also a cinematic masterpiece) and the annual volumes of photographs celebrating National Socialism chiefly in style—"soft" rather than "hard" sell—and point of view.<sup>39</sup>

Brookings Institution study of the NRA put it this way: "The work of 'selling' it [the NRA] to the country brought into play demonstrations of emotionalism, pageantry, and oratorical appeals usually associated with war-time propaganda rather than with the ordinary functionings of peace-time government." Leverett S. Lyon, *et al.*, *The National Recovery Administration: An Analysis and Appraisal* (Washington, 1935), 52.

<sup>39</sup> Robert L. Snyder, *Pare Lorentz and the Documentary Film* (Norman, Okla., 1968), 21–95; Baldwin, *Poverty and Politics*, 118–19; Siegfried Kracauer, *From Caligari to Hitler: A Psychological History of the German Film* (Princeton, 1970), 297–303; Paul Rotha, *Documentary*

The New Deal efforts at mass persuasion were unparalleled among democracies in peacetime—nothing comparable was attempted in France or Great Britain before the outbreak of war in 1939.<sup>40</sup> They reflect the attitude of the Roosevelt government, shared by Hitler's, that the economic emergency demanded a common effort above and beyond politics. The crisis justified the casting aside of precedent, the nationalistic mobilization of society, and the removal of traditional restraints on the power of the state, as in war, and it required personal leadership more forceful than that necessary in normal times. That all these attitudes were typical of Hitler goes without saying, but Roosevelt held them too. Consider this passage in his first inaugural:

I assume unhesitatingly the leadership of this great army of our people. . . . Our true destiny is not to be administered unto but to minister to ourselves. . . . In the event that Congress shall fail . . . I shall ask the Congress for the one remaining instrument to meet the crisis—broad executive power to wage a war against the emergency, as great as the power that would be given to me if we were in fact invaded by a foreign foe.<sup>41</sup>

Roosevelt was neither a totalitarian nor a dictator, real or potential, but his tactics and his rhetoric made it possible for anti-New Dealers and outright fascists to argue that he was both. Many of the accusations of conservatives and Communists in the United States were politically motivated, as were, of course, Nazi comments on the president. But during the first years of the New Deal the German press praised him and the New Deal to the skies. Before Hitler came to power he was, although impressed by Henry Ford's automobiles and the racially oriented American immigration laws, basically contemptuous of the United States, which he considered an overly materialistic nation dominated by Jews, "millionaires, beauty queens, stupid [phonograph] records, and Hollywood."<sup>42</sup>

*Film* (New York, 1963), 199–200, 308, 317–19. See also Erich Koch, ed., *Nürnberg 1934: Ein Bildbericht vom Parteitag 1934* (Berlin [n.d.]). In 1934 Harry Hopkins negotiated a contract with Pathé News for a series of short commercial films for the WPA. This led *Newsweek* to comment: "New Deal Goes Hollywood." Snyder, *Pare Lorentz*, 12–13.

<sup>40</sup> As early as 1908 the Department of Agriculture was producing short educational and instructional movies, and the Department of the Interior and other government agencies also made such films in considerable numbers beginning in 1911. These were not, however, designed for general distribution, as both *The Plow* and *The River* clearly were. During the 1920s and early 1930s the British Empire Marketing Board and later the British Post Office produced films advertising their activities, but not for commercial distribution. Snyder, *Pare Lorentz*, 8–11; Rotha, *Documentary Film*, 96–99; Dartington Hall Trustees, *The Factual Film* (London, 1947), 43–45, 51–52.

<sup>41</sup> Rosenman, *Public Papers*, 2: 14–15. This last sentence evoked the loudest cheering Roosevelt's speech produced. Eleanor Roosevelt found the response "a little terrifying." Commenting on it later, she said: "You felt they would do *anything*—if only someone would tell them *what* to do." Lash, *Eleanor and Franklin*, 360. Leuchtenburg writes: "Roosevelt personified the state as protector." *Roosevelt and the New Deal*, 331. For a full discussion of the "war psychology" of New Deal policies, see Leuchtenburg, "Analogue of War," 81–143.

<sup>42</sup> Gerhard L. Weinberg, "Hitler's Image of the United States," *AHR*, 69 (1963–64): 1008–09; Joachim Remak, "Hitlers Amerikapolitik," *Aussenpolitik*, 6 (1955): 706–08; Offner, *American Abbeasement*, 13–14, 32–33.

Nevertheless, he and his party were impressed by New Deal depression policies. "Mr. Roosevelt . . . marches straight to his objectives over Congress, lobbies, and the bureaucracy," Hitler told Anne O'Hare McCormick of the *New York Times* in July 1933. In July 1934 the *Völkischer Beobachter* described Roosevelt as "absolute lord and master" of the nation, his position "not entirely dissimilar" to a dictator's. Roosevelt's books, *Looking Forward* (1933) and *On Our Way* (1934) were translated into German and enthusiastically reviewed, the critics being quick to draw attention to parallels in New Deal and National Socialist experiences.<sup>43</sup>

A friendly German biography, *Roosevelt: A Revolutionary with Common Sense*, by Helmut Magers, appeared in 1934. Magers described the New Deal as "an authoritarian revolution," a revolution "from above," and pointed up what he called the "surprising similarities" it bore to the Nazi revolution. That there appeared to be some basis for this view at the time is suggested by the fact that Ambassador William E. Dodd wrote a foreword to Magers's book in which he praised the author's "outstanding success" in describing both conditions in the United States and the nation's "unique [*einzigartig*] leader" and spoke of the "heroic efforts being made in Germany and the United States to solve the basic problem of social balance."<sup>44</sup>

Dodd was vehemently anti-Nazi, but he hoped that German moderates like Schacht and Neurath would be able to overthrow Hitler or at least restrain him. He considered the Magers volume an "excellent, friendly, unpartisan book . . . without a sentence that could have been quoted to our disadvantage" and allowed his foreword to be published despite State Department objections. The Germans, for their part, went out of their way to welcome Dodd. A throng of reporters and Foreign Office officials greeted him when he arrived in Berlin. He was put up in the six-room royal suite of the Hotel Esplanade and charged only ten dollars a day. He was invited to lecture at the University of Munich. Hitler assured him that Germany had no warlike intentions. When he criticized authoritarian rule and economic nationalism in a speech, the German press reported his remarks fairly and accurately.<sup>45</sup>

At the end of Roosevelt's first year in office Hitler sent him a message through diplomatic channels offering sincere congratulations for "his

<sup>43</sup> *New York Times*, July 10, 1933, quoted in James V. Compton, *The Swastika and the Eagle: Hitler, the United States, and the Origins of World War II* (Boston, 1967), 20; Schröder, *Deutschland und die Vereinigten Staaten*, 99-114. Mussolini wrote a widely publicized review of *Looking Forward* in which he noted a number of similarities between Roosevelt's thinking on economic policy and his own. He concluded, however, that while Roosevelt's ideas were superficially related to "fascist Corporatism . . . it would be an exaggeration to say anything more." John P. Diggins, *Mussolini and Fascism: The View from America* (Princeton, 1972), 281.

<sup>44</sup> Helmut Magers, *Roosevelt: Ein Revolutionär aus Common Sense* (Leipzig, 1934), 5, 10, *passim*.

<sup>45</sup> Robert Dallek, *Democrat and Diplomat: The Life of William E. Dodd* (New York, 1968), 196-211, 225, 227. Dallek incorrectly implies that Dodd yielded to State Department pressure with regard to his foreword to Magers's biography.

heroic efforts in the interests of the American people. The President's successful battle against economic distress is being followed by the entire German people with interest and admiration," Hitler announced. In November 1934 the *Völkischer Beobachter* characterized Democratic gains in the Congressional elections as an "exceptionally personal success" for Roosevelt. The tone of this article was almost worshipful, the rhetoric hyperbolic. The president (a man of "irreproachable, extremely responsible character and immovable will" [*tadelnsfreie verantwortungsvolle Gesinnung und . . . unverrückbarer Wille*]) had shown himself to be a "warmhearted leader of the people with a profound understanding of social needs" as well as an energetic politician.<sup>46</sup> This attitude ended in 1936, although even after Roosevelt's "quarantine" speech the Nazi propaganda machine refrained for tactical reasons from attacking him personally. It is clear, however, that early New Deal depression policies seemed to the Nazis essentially like their own and the role of Roosevelt not very different from the Führer's.<sup>47</sup>

The importance of these leaders in the fight against the depression lies less in what they did to revive the economy than in the shift in public mood they triggered. In early 1933 that mood was profoundly pessimistic. For four years business conditions had been growing almost steadily worse. The promises and optimistic predictions of innumerable political and business leaders that the tide would turn had all proved illusory. Millions had lost not merely their jobs and savings, but hope itself. It was the duration more than the depth of the decline that was truly depressing.

The Great Depression was totally unlike any earlier economic slump. Men had noted as early as the eighteenth century that economic activity tended to rise and fall in recurrent patterns, and during the nineteenth century the concept of the business cycle was firmly established. Cycles were variously explained, and the terminology was not precise, but it was accepted that the world economy moved in an irregular but unending path through periods of expansion, crisis or panic, recession or depression, and then returned to expansion. Before the collapse of the 1930s a cycle was usually identified by its most dramatic phase, the crisis or panic: witness the American "panics" of 1819, 1837, 1857, 1873, 1893, and 1907. The business slump that followed panics was characteristically

<sup>46</sup> Schröder, *Deutschland und die Vereinigten Staaten*, 112; *Foreign Relations of the United States: Diplomatic Papers, 1934* (Washington, 1951), 2: 419; *Völkischer Beobachter*, Nov. 9, 1934. I am indebted to Professor Hans-Jürgen Schröder of the University of Mainz for copies of this and other articles about the New Deal that appeared in the German press.

<sup>47</sup> Schröder, *Deutschland und die Vereinigten Staaten*, 118-19. In May 1940 *Das Reich* published an article comparing Nazi and New Deal policies to combat the depression: "Hitler and Roosevelt: A German Success—An American Attempt." The anonymous author blamed what he called the weaknesses of the New Deal not on Roosevelt but on the "sacrosanct Constitution" of the United States and on the "parliamentary-democratic system" that forced Roosevelt to cater to conflicting interests. "We began with an idea and carried out the practical measures without regard for consequences. America began with many practical measures that without inner coherence covered over each wound with a special bandage." *Das Reich*, May 26, 1940.

precipitous but mercifully brief. In his classic study of *Business Cycles* (1913), Wesley Clair Mitchell wrote: "The lowest ebb of the physical volume of industrial production usually comes in either the first or the second year after a severe crisis."<sup>48</sup> The German and French words *die Krise* and *la crise* reflect this same focus on the panic aspect of the "normal" business cycle. However, the recession that came after the "panic of 1929" did not follow the expected pattern. Interminably, or so it seemed, it continued. By the end of 1932 industrial production in both the United States and Germany was scarcely more than half of what it had been in 1929.

By the early 1930s professional economists were beginning to realize that the character of business cycles was changing. The first edition of the *Encyclopedia of the Social Sciences*, published between 1930 and 1935, contains an article on *crises* by Jean Lescure, a French expert on business cycles. "A crisis," Lescure wrote, "may be defined as a grave and sudden disturbance of economic equilibrium." Lescure went on to discuss the nature and history of crises, paying special attention to the impact of industrialization and of the growth of cartels and trusts, which he believed had reduced the acuteness of crises but also delayed the process of recovery. "It would seem," he concluded, "that for the term crisis one may henceforth substitute that of depression; it is reasonable to speak today of a world depression rather than of a world crisis."<sup>49</sup>

Lescure's emphasis on the word "depression" highlights the psychological impact of the long economic decline, the pessimism, the sense of hopelessness that had little to do with the size of an individual's pocketbook. A constricting pall appeared to have descended upon the world. Among economists, stagnation theorists flourished and learned authorities spoke of a "mature" economy and the end of the era of economic growth spawned by the Industrial Revolution. Many recommended what the French called a "Malthusian" approach, the reduction of output to the current level of consumption rather than the attempt to increase consumption.<sup>50</sup> Governments, faced with the most extended fall in prices since the 1890s, responded not with inflationary measures but by adopting deflationary monetary policies and by slashing already shrunken budgets.<sup>51</sup>

<sup>48</sup> Wesley C. Mitchell, *Business Cycles and their Causes* (Berkeley, 1963), 133. The general acceptance of this theory helps to explain President Hoover's often-derided optimistic prognostications in 1930 and 1931. Businessmen, according to Joseph Schumpeter, were "by no means over-pessimistic" even in late 1930. The collapse of the American banking system in early 1933, Schumpeter claimed, finally destroyed hope. "The psychic framework of society, which till then had borne up well, was at last giving way." Schumpeter, *Business Cycles*, 2: 906, 944.

<sup>49</sup> Jean Lescure, "Crises," in E. R. A. Seligman and Alvin Johnson, eds., *Encyclopedia of the Social Sciences* (New York, 1930-35), 4: 595-99.

<sup>50</sup> Dorfman, *Economic Mind in American Civilization*, 5: 723-25; Alfred Sauvy, *Histoire économique de la France entre les deux guerres* (Paris, 1967), 2: 359-67. For a brief summary of the mood of early New Deal economists, see Schlesinger, *Coming of the New Deal*, 180-81.

<sup>51</sup> "The deflationary spiral was, in most countries, accentuated by orthodox Government financial policy as then conceived. The automatic effect of a depression was to reduce tax



Businessmen feared to make new investments. Trade languished. Unions dealt with mounting unemployment by urging that youths be kept longer in school, that working women return to the home, that older men retire early. The mood of unemployed workers, some twenty million in the United States and Germany alone by early 1933, was more apathetic than rebellious.<sup>52</sup> It was this general pall of despair and listlessness that the New Deal and the Nazi revolution, personified by Roosevelt and Hitler, dispelled. Long before their economic policies had much effect on the stalled business cycle, they had revitalized the two societies.

COMPARISON WITH Great Britain and France is suggestive. Economic historians disagree about the character of Britain's economic recovery in the 1930s, but the argument concerns the growth *rate* and its causes, not the fact of expansion.<sup>53</sup> By 1937 industrial output was over thirty per cent larger than in 1933, and unemployment had been almost halved. Even allowing for the facts that the British economy had been sluggish in the 1920s, unemployment extremely high, and, therefore, that the world depression seemed a less dramatic collapse in Britain than it did in the United States or Germany, the improvement between 1933 and 1937 marked a very substantial change.<sup>54</sup>

Furthermore, during these years the British government made many efforts to improve conditions. To aid industry it adopted tactics strikingly similar to those of the NRA. It allowed coal operators to limit and allocate output, fix prices, and amalgamate companies; it encouraged cotton textile manufacturers to scrap inefficient machinery, and the steel industry to cartelize its operations. Both the ailing shipbuilding industry and the healthy automobile industry received government subsidies. Electric utility companies were assisted in consolidating their activities by the Central Electricity Board. The remarkable British housing boom, it is true, was largely the work of private enterprise, but government construction was significant—and focused where it was most needed, on slum clearance and homes for the poor. Agriculture was also assisted through a complex mixture of import quotas, tariffs, subsidies, and marketing schemes.

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revenue and increase expenditure for the relief of the unemployed, and hence to produce a budgetary deficit. Orthodox finance demanded that the budget should be balanced annually, by rigid economies in expenditure and the imposition of additional taxes." H. W. Arndt, *The Economic Lessons of the Nineteen-Thirties* (Oxford, 1944), 254.

<sup>52</sup> For convenient discussions of union attitudes in various countries, see the essays in Denise Fauvel-Rouif, ed., *Mouvements ouvriers et dépression économique de 1929 à 1939* (Assen, 1966). On the attitudes of unemployed workers, see Philip Eisenberg and Paul F. Lazarsfeld, "The Psychological Effects of Unemployment," *Psychological Bulletin*, 35 (1938): 358-90.

<sup>53</sup> The most recent summary of the controversy is B. W. E. Alford, *Depression and Recovery? British Economic Growth, 1918-1939* (London, 1972).

<sup>54</sup> G. D. A. MacDougall, "General Survey, 1929-1937," in British Association, *Britain in Recovery* (London, 1938), 1-84; Charles L. Mowat, *Britain Between the Wars: 1918-1940* (Boston, 1971), 432-35.

The government also acted to help British labor. Unemployment insurance and relief services were well established long before 1933, but beginning with the Unemployment Act of 1934 the system was considerably improved. Insurance was put on a sounder financial basis, and an Unemployment Assistance Board was set up to administer the relief program. In 1936 half a million agricultural wage earners were brought into the insurance system. Other laws sought to encourage the movement of labor from the economically stagnant north and west to the more prosperous southeast, and manufacturers willing to build factories in the depressed areas received subsidies.<sup>55</sup> Economic recovery was thus accompanied by considerable social reform; by 1937 the combination of a progressive tax structure and extended social services was transferring five or six per cent of the national income from rich to poor, raising the real income of the working classes by some eight to fourteen per cent. In the United States, by way of contrast, New Deal legislation had almost no measurable effect on income distribution.<sup>56</sup>

Yet the people of Great Britain had no sense of experiencing a new era. It seems clear (although such things are difficult to measure) that the national mood remained depressed, despite economic progress. No political leader was able to generate a sense of common commitment to the battle against the depression. When David Lloyd George announced a plan for a "New Deal for Britain" in 1935—it was little more than a rehash of proposals he had made repeatedly in the 1920s—Ramsay MacDonald's cabinet was thrown almost into a panic and gave serious consideration to inviting both Lloyd George and Winston Churchill to join the government, but it did not do so. Chancellor of the Exchequer Neville Chamberlain wrote in his diary at this time: "The P.M. [MacDonald] is ill and tired, S[tanley] B[aldwin] is tired and won't apply his mind to problems." (Contrast this state of mind with the mood in government circles in Washington and Berlin.) "It is certainly time there was a change," Chamberlain also wrote, having himself in mind as the person to institute it. But after Chamberlain became prime minister in 1937, the national mood was no different. Chamberlain was a conservative of the finest type, hard working and public spirited; no one in Great Britain contributed more to social reform in the interwar years. But he was by this time also aging and ailing—unable to inspire public enthusiasm. As he said of himself, he could not "unbutton."<sup>57</sup>

<sup>55</sup> A. J. Youngson, *Britain's Economic Growth: 1920-1966* (2d ed.; London, 1968), 96-120, 127-31; Bentley B. Gilbert, *British Social Policy: 1914-1939* (Ithaca, 1970), 178-88, 191, 252-54. Although critical of the limitations of British social policy and of most of the politicians of the period, Gilbert concludes that by 1939 "the British State had committed itself to the maintenance of all its citizens according to need as a matter of right" (p. 308). See also, H. W. Richardson, *Economic Recovery in Britain, 1932-9* (London, 1967).

<sup>56</sup> Mowat, *Britain between the Wars*, 492; Youngson, *Britain's Economic Growth*, 136-37; Simon Kuznets, *National Income: A Survey of Findings* (New York, 1946), 99.

<sup>57</sup> Gilbert, *British Social Policy*, 185; Keith Feiling, *The Life of Neville Chamberlain* (London, 1947), 242-43.

Thus the 1930s passed into memory in Great Britain as a time of inactivity and decline—the Great Slump. “The man in the street’s view of Britain’s experience . . . is that activity was stagnant and very depressed,” the economic historian Harry W. Richardson writes—this even though “any glance at the evidence shows it to be a misconception.”<sup>58</sup> Surely this helps to explain why fascism became a more formidable force in Britain than in the United States, despite economic recovery and the far more serious and immediate threat that the Nazis posed to the British.

The French experience provides another opportunity to study this aspect of the depression. Its full force struck France late, but by 1935 conditions were very bad. Farm prices had collapsed. Industrial output was down sharply. French workers were probably worse off than those of any other industrial nation: wage rates as low as eighty centimes an hour (the franc was worth about five cents) were not unknown; employers were autocratic, superficially well organized, and adamantly opposed to collective bargaining; unemployment was increasing rapidly and was far greater than French statistics indicated. Over 503,000 persons were receiving relief payments in February 1935, an increase of more than 150,000 in one year, and many of the unemployed were unable to meet all the eight “general conditions” required to qualify for aid. Furthermore, in counting the unemployed the French government made no allowance for those who had given up looking for work, for individuals who had lost their jobs and returned to family farms, or for unemployed foreigners who had no work permits. The Ministry of Labor, which compiled the unemployment statistics, itself confessed that the number of *employed* Frenchmen had declined by 1,880,000 since 1930, and this estimate was probably too low.<sup>59</sup>

Moreover, French governments, aside from the fact that no party or coalition was capable of staying in power for more than a few months, were not merely ineffective but complacent in dealing with the problems of the depression. In 1935, when organized business sought a law (*le projet Marchandeaup*) much like the NRA codes enforcing restrictive trade

<sup>58</sup> Richardson, *Economic Recovery*, 21. Richardson argues that government policies had little to do with recovery and were not even primarily designed to fight the depression. “In Britain new policies were implemented either on grounds of expediency or because short-run considerations forced them on a reluctant government.” H. W. Richardson, “The Economic Significance of the Depression in Britain,” *Journal of Contemporary History*, 9 (1969): no. 4, p. 18.

<sup>59</sup> Sauvy, *Histoire économique*, 2: 111–37, 165–80; Alfred Sauvy, “The Economic Crisis of the 1930s in France,” *Journal of Contemporary History*, 9 (1969): no. 4, pp. 21–26; Simone Weil, *La condition ouvrière* (Paris, 1951), 35–108; Jean Touchard and Louis Bodin, “L’État de l’opinion au début de l’année 1936,” in *Cahiers de la Fondation Nationale des Sciences Politiques, Léon Blum: chef de gouvernement, 1936–1937* (Paris, 1967), 53–54; Georges Lefranc, ed., *Juin 36* (Paris, 1966), 209–10; Henry W. Ehrmann, *Organized Business in France* (Princeton, 1957), 15–32; Ministère du Travail, *Bulletin*, 42 (1935): 1, 7–8. See also, M. Daclin, *La crise des années 30 à Besançon* (Paris, 1968), 80–83; Walter Galenson and Arnold Zellner, “International Comparison of Unemployment Rates,” in National Bureau of Economic Research, *The Measurement and Behavior of Unemployment* (Princeton, 1957), 453, 508–26; Gabrielle Letellier et al., *Le chômage en France de 1930 à 1936* (Paris, 1938), 35–42, 246–62.

agreements, the bill was defeated. The official position of the Ministry of Labor on unemployment, repeatedly enunciated in its annual reports, ran as follows: "The state cannot pretend to be able to eliminate or diminish unemployment since its activities do not get at the causes of the evil." The minister's report of June 22, 1936, praised local relief officers for their "very important" services in "not allowing unemployed workers to get aid unless they met all the requirements." Thus "public funds have been safeguarded."<sup>60</sup>

Then quite suddenly in the spring of 1936 the electoral victory of the Popular Front unleashed a spontaneous, grass-roots outburst of protest, signalized by a wave of sit-down strikes that brought the economy to a standstill. Thoroughly alarmed, the leaders of industry swiftly capitulated, throwing themselves on the mercy of the new Socialist premier, Léon Blum. Within a matter of days a new system of labor-management relations, buttressed by a host of laws similar to those of the New Deal, was hammered out by representatives of big business and the unions and pushed through parliament by the Blum government: state-supervised collective bargaining, large wage increases, a forty-hour week, and paid vacations, along with banking reform and a program to support agricultural prices were all instituted in one hectic burst of activity.<sup>61</sup>

This transformation was, of course, shortlived; by 1939 France was more divided and lacking in any sense of commitment to common national purposes than in 1935. To what extent a lack of leadership and particularly Léon Blum's personal inadequacies caused this reversal is a very difficult question. Blum's performance can be criticized from two perspectives. First of all, should he, as a lifelong socialist, have attempted to use the crisis to change France more radically? In a brilliant article, "*Tout est possible*," the left-wing socialist Marceau Pivert urged him to try. "The masses are much more advanced than we imagine," Pivert insisted. They are ready not merely for "an insipid cup of medicinal tea," but for "drastic surgery," including the nationalization of banks, utility companies, and "trusts," and the confiscation of the wealth of "deserters of

<sup>60</sup> André Piettre, *L'Evolution des Ententes industrielles en France depuis la Crise* (Paris, 1936), 85-93, 166-83; Ehrmann, *Organized Business in France*, 370-71; Ministère du Travail, *Bulletin*, 43 (1936): 173, 171. In February 1935 Premier Flandin explained his unemployment "policy" to the National Assembly: Large-scale direct relief on the model of the British dole would be impossible in France because of the rapid increase in French unemployment, because France "lacked the resources" of Britain, and because it would unbalance the budget. Extensive public works on the American model would not work because "the future earnings" of such projects would not equal their cost, because France lacked the necessary capital and was too deeply in debt to borrow it, and because France did not need more public works. Yet Flandin admitted that he was receiving "hundreds of heartrending letters each day" from unemployed workers. He professed to be feeling "*une angoisse quotidienne . . . de jour et denuit*." Quoted in *ibid.*, 42 (1935): 114-19.

<sup>61</sup> Joel Colton, *Léon Blum: Humanist in Politics* (New York, 1966), 129-59; Georges Lefranc, *Histoire du Front Populaire* (Paris, 1965), 139-86. See also two excellent collections of documents and eyewitness reports, Louis Bodin and Jean Touchard, eds., *Front Populaire: 1936* (2d ed.; Paris, 1965), and Lefranc, *Juin* 36.

the franc."<sup>62</sup> Blum rejected these proposals, being backed by most of the Socialists and also by the Communists, the union leaders, and the Radicals who made up his coalition. He did so on the reasonable, indeed honorable ground that the Popular Front parties, having campaigned on a platform of moderate reform within the capitalist system, had no mandate for revolutionary change. "Our duty," he later said, "was . . . to show ourselves scrupulously faithful to the program." He felt that he must "keep loyally, publicly, the promise that I had made." But it can be argued that the upheavals of 1936 *were* revolutionary, even that (as Pierre Mendès France said many years later) the election of 1936 was not a plebiscite for any particular reform but "an affirmation of a popular desire to see the country break out of its deflationary rut and conservative structures." Socialist critics, re-reading some of Blum's modest and diffident comments about the responsibilities of his high office, have been impressed by a "crushing masochism" in his character, a defeatist attitude, an exaggerated concern for punctilio. Blum was too much the "grand bourgeois," Pivert recollected, "too subtle, too refined to be a revolutionary leader."<sup>63</sup> Georges Lefranc, both a participant in and one of the leading historians of the events of 1936, put the question this way: perhaps everything was not possible, "but can one say that everything that was possible was tried?"

One can reject this line of argument, but it remains true that Blum failed not merely to build upon the reforms of 1936 but even to protect them adequately against counterattack. He of course faced staggering difficulties: the antediluvian mentality of French industrialists, the doctrinaire rigidity of union leaders, the slavish commitment of the Communists to the policies of the Soviet Union, the tragic divisions resulting from the Spanish Civil War, the noisy rightist "patriotic" groups, the perverse individualism of nearly every Frenchman. Probably no political leader could have overcome the shortsighted selfishness and inertia or resisted the splintering factionalism that plagued French society in the late thirties.

Yet Blum's efforts were pitifully inadequate, no better or worse than those of the uninspired premiers who preceded and followed him.<sup>64</sup> Long

<sup>62</sup> The text is conveniently reprinted in Lefranc, *Histoire du Front Populaire*, 450-53. On the position of the Pivert faction and other leftist groups during the Blum regime, see Pierre Brouc and Nicole Dorey, "Critiques de gauche et opposition révolutionnaire au Front Populaire (1936-1938)," *Le Mouvement Social*, 54 (1966): 91-133.

<sup>63</sup> Colton, *Blum*, 136-39; Léon Blum, *L'histoire jugera* (Paris, 1945), 266, 282; Daniel Guérin, *Front populaire révolution manquée: témoignage militant* (2d ed.; Paris, 1970), 111-18; Georges Lefranc, "Le courant planiste dans le mouvement ouvrier français de 1933 à 1936," *Le Mouvement Social*, 54 (1966): 89. Mendès France's remark was made in 1965 during a discussion of papers at a colloquium on Blum and his government. Fondation Nationale des Sciences Politiques, *Léon Blum*, 90.

<sup>64</sup> Tom Kemp, in a recent analysis of French economic problems in the 1930s, makes no distinction when he writes: "A prevailing crisis of confidence . . . paralyzed the decision-making powers of businessmen and politicians and made them . . . incapable of facing up to the depression." Tom Kemp, "The French Economy under the Franc Poincaré," *Economic History Review*, 24 (1971): 97-98.

years of balancing his socialist principles against his political ambitions and responsibilities had made him, in Joel Colton's words, a "tightrope-walker." Before the formation of the Popular Front he had, when forced to make a choice, always put his socialism above political accommodation. After he finally made the other choice in 1936, he became too much the politician, telling workers, despite the still-lagging French economy, that it was time for a "pause," that they must exercise "moderation and patience." It was not of central importance that, as the historian Alfred Sauvy has said, Blum's ignorance of economics was matched only by his sincerity. He could muster neither the flexibility of Roosevelt (whom he greatly admired) nor the ruthlessness of Hitler (whom he detested).<sup>65</sup>

During Blum's brief second term as premier, he revised his economic thinking. Aided by Georges Boris, author of an admiring study of the New Deal, *La Révolution Roosevelt* (1934), and one of the few Frenchmen familiar with the new Keynesian economics, Blum drafted a comprehensive program involving tax relief and government credits for defense industries, tax relief for the construction industry and small business, suspension of redemptions of the national debt, a special capital levy on the rich, a more progressive income tax, and rudimentary exchange controls. A massive common effort was necessary, he said, in order to build up French defenses, expand production, and maintain "social solidarity." The Assembly supported Blum's plan, but when the Senate voted it down he meekly resigned without even demanding the vote of confidence that might have compelled the Senate to yield. "To make the project succeed," Sauvy writes, "would have required a resounding appeal to all the forces of a country threatened with collapse. Unfortunately, Blum was not capable of such an effort."<sup>66</sup>

What he lacked was not courage but firmness, and the daring to step beyond the comfortable security of conventional political procedures. After the fall of his second government he confessed: "Perhaps, if I committed errors it was because of not having been enough of a leader."<sup>67</sup> None of this proves that a Blum like Roosevelt or—God forbid—Hitler could have provided France with the kind of *élan* that developed in the United States and Germany. It does, however, point up the psychological importance of Roosevelt and Hitler in their own countries.

<sup>65</sup> Colton, *Blum*, 89, 192; Sauvy, *Histoire économique*, 2: 303, 276–78. It is ironic that flexibility was the Rooseveltian quality that Blum admired most. On May 29, 1936, a few days before he assumed the premiership, Blum told Léon Jouhaux, head of the French labor unions: "What inspires me at the present moment is the example of Roosevelt . . . and especially his boldness, which has enabled him to change his methods when he realizes that they are not working out as planned." Quoted in Bernard Georges, "La C.G.T. et le gouvernement Léon Blum," *Le Mouvement Social*, 54 (1966): 55.

<sup>66</sup> Colton, *Blum*, 297–304; Georges Lefranc, *Le mouvement socialiste sous la troisième république: 1875–1940* (Paris, 1963), 352n; Sauvy, *Histoire économique*, 2: 277–78. See also, Tom Kemp, *The French Economy, 1919–1939: The History of a Decline* (London, 1972), 115–28. Kemp sees the failure of the Popular Front as essentially a political failure and writes of Blum's "lack of confidence in his own solution for the crisis" (p. 125).

<sup>67</sup> Colton, *Blum*, 284.

THESE PARALLELS suggest a number of generalizations about how the Great Depression influenced the United States and Germany. They do not, as I said at the start, indicate that the New Deal was a variant of fascism. The extraordinary expansion of the role of the federal government that took place in America cannot be equated with the Nazi totalitarian system, nor Hitler's despotism with the new executive power that Roosevelt exercised. The differences are qualitative not merely quantitative. But both governments experienced the depression as a tremendous crisis, and this fact shaped their responses in related ways. Furthermore the two regimes suffered from common intellectual, emotional, and organizational limitations that also led to analogous reactions to the depression.

Before Hitler and Roosevelt achieved power, more rigid and conservative leaders had tended to see the depression as a world-wide disease that would yield to international rather than national cures and indeed as one that governmental medicines could not alone eradicate. Hoover, despite his belief that European selfishness and shortsightedness had caused the depression, proposed his moratorium of 1931 "to give the forthcoming year to the economic recovery of the world," and Heinrich Brüning defended what he himself called his "draconian" emergency decrees as necessary to enable Germany "to meet its international obligations" and "conquer the economic crisis." Brüning, like Hoover, also believed that governmental policies of any kind could influence the economy relatively little. After the disastrous German elections of 1930, which made the Nazis a formidable political force, Brüning informed Hitler complacently: "According to our estimation, the crisis will last about four or five years more." And in a message on New Year's Day, 1931, he told the nation: "I am anxious to stress the limitations of any policy so that you will not indulge in any illusions."<sup>68</sup>

Nazis and New Dealers adopted more parochial but also more intense tactics, placing the economic well-being of their own societies ahead of world recovery and taking a far more optimistic view of what government could accomplish. While assuming the continuance of capitalism and in many ways adding to the wealth of private business groups, each nation sharply restricted the individual's freedom to pursue his economic interests and construed the power of government, and of executive power within the political system, in very broad terms. In addition, New Dealers and Nazis insisted that economic recovery could not be achieved without a certain amount of social restructuring and, furthermore, that society could be changed without exacerbating class conflicts. Indeed in both cases social reform was supposed to moderate such conflicts. But in both

<sup>68</sup> Hoover, *Great Depression*, 70; Kroll, *Weltwirtschaftskrise*, 364; Heinrich Brüning, *Memoiren: 1918-1934* (Stuttgart, 1970), 222-23, 192; Andreas Dorpalen, *Hindenburg and the Weimar Republic* (Princeton, 1964), 216. See also, Henning Köhler, "Arbeitsbeschaffung, Siedlung und Reparationen in der Schlussphase der Regierung Brüning," *Vierteljahrshefte für Zeitgeschichte*, 17 (1969): 304-06.

Roosevelt's America and Hitler's Germany economic and social objectives were subordinated whenever necessary to political aims.

That other nations adopted many of the tactics employed by New Dealers and Nazis scarcely needs demonstration. Depression policies everywhere were certainly based on national self-interest narrowly conceived, despite the obvious fact that the same plague was ravaging all. The tendency of governments to extend their sway in economic affairs and of leaders to be heavily influenced by political considerations was virtually universal. Whether conservative, moderate, or radical, few if any of the statesmen of the thirties remained indifferent to the suffering of their constituents or unwilling to sanction changes designed to alleviate it. The difference between the American and German depression experiences and those of other nations was in large measure psychological, resulting from Roosevelt's and Hitler's personal qualities of leadership and from their responses to the particular conditions in the two countries.

Again the comparison with Hoover and Brüning is at least suggestive: both lacked political tact and the ability to project an impression of warmth, sympathy, and self-assurance. In 1931 Walter Lippmann described Hoover as "indecisive and hesitant in dealing with political issues," and Arthur Krock commented on his "awkwardness of manner and speech and lack of mass magnetism." A recent student of the Hoover administration, Albert U. Romasco, remarks on his "inability to master the political techniques of leadership." As for Brüning, the historian Theodor Eschenburg, who knew him personally and considered him a "statesman of the highest intellectual gifts," admits that he had neither "the psychological talent" to win public backing nor "the tactical ability" to manage politicians. "He thought in terms of policy, not of human beings." And Andreas Dorpalen, another historian who lived through the Brüning period as a student in Germany, describes him as a "shy, withdrawn man [who] was unable to arouse the nation," a person lacking in warmth and imagination. Dorpalen's statement that the German public "mistook the chancellor's sober factualness for cynical coldness" could as well be applied to Hoover.<sup>69</sup>

So far as the depression is concerned, Roosevelt and Hitler, the one essentially benign, the other malevolent, justified far-reaching constitutional changes as being necessary to the improvement of economic conditions in a grave emergency but used change also as a device for mobilizing the psychic energies of the people. Yet both their administrations were plagued by infighting and confusion, partly because of genuine conflicts of interest and philosophy within the two diverse societies, but partly

<sup>69</sup> Walter Lippmann, *Interpretations: 1931-1932* (New York, 1932), 67; Albert U. Romasco, *The Poverty of Abundance: Hoover, the Nation, the Depression* (New York, 1965), 202, 213; Theodor Eschenburg, "The Role of Personality in the Crisis of the Weimar Republic: Hindenburg, Brüning, Groener, Schleicher," in Hajo Holborn, ed., *Republic to Reich: The Making of the Nazi Revolution* (New York, 1972), 49-50; Dorpalen, *Hindenburg*, 195, 216-17.



because of ignorance. No one really knew how to end the depression or even how best to serve the different interests the governments presumed to represent. Time after time major American and German policies produced results neither anticipated nor desired, some of them—the effect of New Deal farm policy on share croppers and of its public housing policy on racial segregation,<sup>70</sup> and that of Nazi rearmament on urban concentration, for example—directly contrary to the leaders' intentions.

Hitler papered over confusion, doubts, and rivalries with the *Führerprinzip*, unquestioning obedience to the leader, who was presumed to know what was best. Roosevelt, on the other hand, made a virtue of flexibility and experimentation. Both, however, masterfully disguised the inadequacies and internal disagreements in their entourages and to a remarkable extent succeeded in convincing ordinary citizens of their own personal wisdom and dedication.

The differences in the degree and intensity with which psychological pressures were applied by Nazis and New Dealers were so great as to become differences in kind—leaving aside the brute Nazi suppression not merely of those who resisted or disagreed, but of all who did not fit the insane Hitlerian conception of the proper order of things. The two movements nevertheless reacted to the Great Depression in similar ways, distinct from those of other industrial nations. Of the two the Nazis were the more successful in curing the economic ills of the 1930s. They reduced unemployment and stimulated industrial production faster than the Americans did and, considering their resources, handled their monetary and trade problems more successfully, certainly more imaginatively. This was partly because the Nazis employed deficit financing on a larger scale<sup>71</sup> and partly because their totalitarian system better lent itself to the mobilization of society, both by force and by persuasion. By 1936 the depression was substantially over in Germany, far from finished in the United States. However, neither regime solved the problem of maintaining prosperity without war. The German leaders wanted war and used the economy to make war possible. One result was "prosperity": full employment, increased output, hectic economic expansion. The Americans lacked this motivation, but when war was forced upon them they took the same approach and achieved the same result.

<sup>70</sup> "The New Deal's inclusion of Negroes in programs designed to relieve the special problems created by the Depression must be balanced against certain adverse side effects. . . . The housing projects encouraged residential segregation . . . and played a crucial role in spreading slum conditions." Christopher G. Wye, "The New Deal and the Negro Community: Toward a Broader Conceptualization," *Journal of American History*, 59 (1972): 621–22.

<sup>71</sup> Between 1933 and 1939 the German national debt increased from 12.9 billion marks to 42.7 billion, the American from \$22.5 billion to \$40.4 billion. Fischer, *Deutsche Wirtschaftspolitik*, 101; Erbe, *Wirtschaftspolitik*, 34–35; *Historical Statistics*, 720.

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## The Atomic Bomb and the Origins of the Cold War: U.S. Atomic-Energy Policy and Diplomacy, 1941-45

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MARTIN J. SHERWIN

DURING THE SECOND WORLD WAR the atomic bomb was seen and valued as a potential rather than an actual instrument of policy. Responsible officials believed that its impact on diplomacy had to await its development and, perhaps, even a demonstration of its power. As Henry L. Stimson, the secretary of war, observed in his memoirs: "The bomb as a merely probable weapon had seemed a weak reed on which to rely, but the bomb as a colossal reality was very different."<sup>1</sup> That policy makers considered this difference before Hiroshima has been well documented, but whether they based wartime diplomatic policies upon an anticipated successful demonstration of the bomb's power remains a source of controversy.<sup>2</sup> Two questions delineate the issues in this debate. First, did the development of the atomic bomb affect the way American policy makers conducted diplomacy with the Soviet Union? Second, did diplomatic considerations related to the Soviet Union influence the decision to use the atomic bomb against Japan?

These important questions relating the atomic bomb to American diplomacy, and ultimately to the origins of the cold war, have been addressed almost exclusively to the formulation of policy during the early months of the Truman administration. As a result, two anterior

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<sup>1</sup> Henry L. Stimson and McGeorge Bundy, *On Active Service in Peace and War* (New York, 1947), 637.

<sup>2</sup> Compare the evidence and interpretations in the following studies: Gar Alperovitz, *Atomic Diplomacy: Hiroshima and Potsdam* (New York, 1965); Herbert Feis, *The Atomic Bomb and the End of World War II* (Princeton, 1966); Len Giovannitti and Fred Freed, *The Decision To Drop the Bomb* (New York, 1965); Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, 1939/1946: A History of the United States Atomic Energy Commission*, 1 (University Park, Pa., 1962); and Walter Smith Schoenberger, *Decision of Destiny* (Athens, Ohio, 1969).

questions of equal importance, questions with implications for those already posed, have been overlooked. Did diplomatic considerations related to Soviet postwar behavior influence the formulation of Roosevelt's atomic-energy policies? What effect did the atomic legacy Truman inherited have on the diplomatic and atomic-energy policies of his administration?

To comprehend the nature of the relationship between atomic-energy and diplomatic policies that developed during the war, the bomb must be seen as policy makers saw it before Hiroshima, as a weapon that might be used to control postwar diplomacy. For this task our present view is conceptually inadequate. After more than a quarter century of experience we understand, as wartime policy makers did not, the bomb's limitations as a diplomatic instrument. To appreciate the profound influence of the unchallenged wartime assumption about the bomb's impact on diplomacy we must recognize the postwar purposes for which policy makers and their advisers believed the bomb could be used. In this effort Churchill's expectations must be scrutinized as carefully as Roosevelt's, and scientists' ideas must be considered along with those of politicians. Truman's decision to use the atomic bomb against Japan must be evaluated in the light of Roosevelt's atomic legacy, and the problems of impending peace must be considered along with the exigencies of war. To isolate the basic atomic-energy policy alternatives that emerged during the war requires that we first ask whether alternatives were, in fact, recognized.

What emerges most clearly from a close examination of wartime formulation of atomic-energy policy is the conclusion that policy makers never seriously questioned the assumption that the atomic bomb should be used against Germany or Japan. From October 9, 1941, the time of the first meeting to organize the atomic-energy project, Stimson, Roosevelt, and other members of the "top policy group" conceived of the development of the atomic bomb as an essential part of the total war effort.<sup>3</sup> Though the suggestion to build the bomb was initially made by scientists who feared that Germany might develop the weapon first, those with political responsibility for prosecuting the war accepted the circumstances of the bomb's creation as sufficient justification for its use against any enemy.

Having nurtured this point of view during the war, Stimson charged those who later criticized the use of the bomb with two errors. First, these critics asked the wrong question: it was not whether surrender could have been obtained without using the bomb but whether a different diplomatic and military course from that followed by the Truman administration would have achieved an earlier surrender. Second, the

<sup>3</sup> Vannevar Bush to James B. Conant, Oct. 9, 1941, Atomic Energy Commission (hereafter AEC), doc. 17, AEC Archives, Washington; Schoenberger, *Decision*, *passim*.

basic assumption of these critics was false: the idea that American policy should have been based primarily on a desire not to employ the bomb seemed as "irresponsible" as a policy controlled by a positive desire to use it. The war, not the bomb, Stimson argued, had been the primary focus of his attention; as secretary of war his responsibilities permitted no alternative.<sup>4</sup>

Stimson's own wartime diary nevertheless indicates that from 1941 on, the problems associated with the atomic bomb moved steadily closer to the center of his own and Roosevelt's concerns. As the war progressed, the implications of the weapon's development became diplomatic as well as military, postwar as well as wartime. Recognizing that a monopoly of the atomic bomb gave the United States a powerful new military advantage, Roosevelt and Stimson became increasingly anxious to convert it to diplomatic advantage. In December 1944 they spoke of using the "secret" of the atomic bomb as a means of obtaining a *quid pro quo* from the Soviet Union. But viewing the bomb as a potential instrument of diplomacy, they were not moved to formulate a concrete plan for carrying out this exchange before the bomb was used. The bomb had "this unique peculiarity," Stimson noted several months later in his diary; "Success is 99% assured, yet only by the first actual war trial of the weapon can the actual certainty be fixed."<sup>5</sup> Whether or not the specter of postwar Soviet ambitions created "a positive desire" to ascertain the bomb's power, until that decision was executed "atomic diplomacy" remained an idea that never crystallized into policy.<sup>6</sup>

Although Roosevelt left no definitive statement assigning a postwar role to the atomic bomb, his expectations for its potential diplomatic value can be recalled from the existing record. An analysis of the policies he chose from among the alternatives he faced suggests that the potential diplomatic value of the bomb began to shape his atomic-energy policies as early as 1943. He may have been cautious about counting on the bomb as a reality during the war, but he nevertheless consistently chose policy alternatives that would promote the postwar diplomatic potential of the bomb if the predictions of scientists proved true. These policies were based on the assumption that the bomb could be used effectively to secure postwar diplomatic aims; and this assumption was carried over from the Roosevelt to the Truman administration.

Despite general agreement that the bomb would be an extraordinarily important diplomatic factor after the war, those closely associated with its

<sup>4</sup> Stimson and Bundy, *On Active Service*, 628-29.

<sup>5</sup> Stimson, diary, Dec. 31, 1944, Apr. 6-11, 1945, Henry L. Stimson Papers, Yale University Library.

<sup>6</sup> The term "atomic diplomacy" has often been used, but to my knowledge it has never been specifically defined. I understand it to mean either the overt diplomatic or military brandishing of atomic weapons for the purpose of securing foreign-policy objectives, or a covert diplomatic strategy based upon considerations related to atomic weapons. In this article "atomic diplomacy" refers exclusively to United States-Soviet relations.

development did not agree on how to use it most effectively as an instrument of diplomacy. Convinced that wartime atomic-energy policies would have postwar diplomatic consequences, several scientists advised Roosevelt to adopt policies aimed at achieving a postwar international control system. Churchill, on the other hand, urged the president to maintain the Anglo-American atomic monopoly as a diplomatic counter against the postwar ambitions of other nations—particularly against the Soviet Union. Roosevelt fashioned his atomic-energy policies from the choices he made between these conflicting recommendations. In 1943 he rejected the counsel of his science advisers and began to consider the diplomatic component of atomic-energy policy in consultation with Churchill alone. This decision-making procedure and Roosevelt's untimely death have left his motives ambiguous. Nevertheless it is clear that he pursued policies consistent with Churchill's monopolistic, anti-Soviet views.

The findings of this study thus raise serious questions concerning generalizations historians have commonly made about Roosevelt's diplomacy: that it was consistent with his public reputation for cooperation and conciliation; that he was naive with respect to postwar Soviet behavior; that, like Wilson, he believed in collective security as an effective guarantor of national safety; and that he made every possible effort to assure that the Soviet Union and its allies would continue to function as postwar partners.<sup>7</sup> Although this article does not dispute the view that Roosevelt desired amicable postwar relations with the Soviet Union, or even that he worked hard to achieve them, it does suggest that historians have exaggerated his confidence in (and perhaps his commitment to) such an outcome. His most secret and among his most important long-range decisions—those responsible for prescribing a diplomatic role for the atomic bomb—reflected his lack of confidence. Finally, in light of this study's conclusions, the widely held assumption that Truman's attitude toward the atomic bomb was substantially different from Roosevelt's must also be revised.

LIKE THE GRAND ALLIANCE itself, the Anglo-American atomic-energy partnership was forged by the war and its exigencies. The threat of a German atomic bomb precipitated a hasty marriage of convenience between British research and American resources. When scientists in Britain proposed a theory that explained how an atomic bomb might quickly be built, policy makers had to assume that German scientists were build-

<sup>7</sup> These views are represented in the following books and articles: Alperovitz, *Atomic Diplomacy*, 12-13; William Bullitt, "How We Won the War and Lost the Peace," *Life*, Aug. 30, 1948, pp. 82-97; Arthur Schlesinger, jr., "Origins of the Cold War," *Foreign Affairs*, 46 (1967): 26-29; and Herbert Feis, *Churchill, Roosevelt, Stalin: The War They Waged and the Peace They Sought* (Princeton, 1957), 596-98.

ing one.<sup>8</sup> "If such an explosive were made," Vannevar Bush, the director of the Office of Scientific Research and Development, told Roosevelt in July 1941, "it would be thousands of times more powerful than existing explosives, and its use might be determining." Roosevelt assumed nothing less. Even before the atomic-energy project was fully organized he assigned it the highest priority. He wanted the program "pushed not only in regard to development, but also with due regard to time. This is very much of the essence," he told Bush in March 1942. "We both felt painfully the dangers of doing nothing," Churchill recalled, referring to an early wartime discussion with Roosevelt about the bomb.<sup>9</sup>

The high stakes at issue during the war did not prevent officials in Great Britain or the United States from considering the postwar implications of their atomic-energy decisions. As early as 1941, during the debate over whether to join the United States in an atomic-energy partnership, members of the British government's atomic-energy committee argued that the matter "was so important for the future that work should proceed in Britain."<sup>10</sup> Weighing the obvious difficulties of proceeding alone against the possible advantages of working with the United States, Sir John Anderson, then lord president of the council and the minister responsible for atomic-energy research, advocated the partnership. As he explained to Churchill, by working closely with the Americans British scientists would be able "to take up the work again [after the war], not where we left off, but where the combined effort had by then brought it."<sup>11</sup>

As early as October 1942 Roosevelt's science advisers exhibited a similar concern with the potential postwar value of atomic energy. After conducting a full-scale review of the atomic-energy project, James B. Conant, the president of Harvard University and Bush's deputy, recommended discontinuing the Anglo-American partnership "as far as development and manufacture is concerned." Conant had in mind three considerations when he suggested a more limited arrangement with the British: first, the project had been transferred from scientific to military control; second, the United States was doing almost all the developmental work; and third, security dictated "moving in a direction of holding much more closely the information about the development of this program." Under these conditions it was difficult, Conant observed, "to see how a joint British-American project could be sponsored in this

<sup>8</sup> The critical break-through was made by Otto R. Frisch and Rudolph E. Peierls in April 1940. For details of the British contribution, see Margaret Gowing, *Britain and Atomic Energy* (London, 1964), pt. 1, app. 1.

<sup>9</sup> Bush to Roosevelt, July 16, 1941; Roosevelt to Bush, Mar. 11, 1942, President's Secretary's File (hereafter PSF), Bush folder, Franklin D. Roosevelt Library (hereafter FDRL), Hyde Park, N.Y.; Winston S. Churchill, *The Second World War* (New York, 1962), 4: 330.

<sup>10</sup> This option was impractical due to scarce resources and the danger to project sites from German bombing. Quotation from Gowing, *Britain*, 73, 78.

<sup>11</sup> "Minute from Sir John Anderson to Prime Minister, 30.7.42," in *ibid.*, app. 3, pp. 437-38.

country."<sup>12</sup> What prompted Conant's recommendations, however, was his suspicion—soon to be shared by other senior atomic-energy administrators—that the British were rather more concerned with information for postwar industrial purposes than for wartime use.<sup>13</sup> What right did the British have to the fruits of American labor? "We were doing nine-tenths of the work," Stimson told Roosevelt in October.<sup>14</sup> By December 1942 there was general agreement among the president's atomic-energy advisers that the British no longer had a valid claim to all atomic-energy information.

Conant's arguments and suggestions for a more limited partnership were incorporated into a "Report to the President by the Military Policy Committee." Roosevelt approved the recommendations on December 28. Early in January the British were officially informed that the rules governing the Anglo-American atomic-energy partnership had been altered on "orders from the top."<sup>15</sup>

By approving the policy of "restricted interchange" Roosevelt undermined a major incentive for British cooperation. It is not surprising, therefore, that Churchill took up the matter directly with the president and with Harry Hopkins, "Roosevelt's own, personal Foreign Office." The prime minister's initial response to the new policy reflected his determination to have it reversed: "That we should each work separately," he threatened, "would be a sombre decision."<sup>16</sup>

Conant and Bush understood the implications of Churchill's intervention and sought to counter its effect. "It is our duty," Conant wrote Bush, "to see to it that the President of the United States, in writing, is informed of what is involved in these decisions." Their memorandums no longer concentrated on tortuous discussions differentiating between the scientific research and the manufacturing stages of the bomb's development but focused on what to Conant was "the major consideration . . . that of *national security and postwar strategic significance*." Information on manufacturing an atomic bomb, Conant noted, was a "military secret which is in a totally different class from anything the world

<sup>12</sup> Conant to Bush, "Some thoughts concerning the S-1 project," Oct. 26, 1942, AEC doc. 295.

<sup>13</sup> Conant to Bush, Mar. 25, 1943, Harry Hopkins Papers (hereafter HHP), A-Bomb folder, FDRL; Conant to Bush, "U.S.—British Relations on S-1 Project," Nov. 13, 1942, AEC doc. 310; Leslie R. Groves, "Diplomatic History of the Manhattan Project" (hereafter "DHMP"), 7, 9, in Manhattan Engineer District Files (hereafter MED Files), National Archives. The Manhattan Engineer District, most commonly referred to as the Manhattan Project, was the cover name assigned by the United States Army to the atomic-energy project.

<sup>14</sup> Memorandum by Stimson, Oct. 29, 1942, in Groves, "DHMP," annex 5.

<sup>15</sup> "Excerpt from Report to the President by the Military Policy Committee, Dec. 15, 1942, with Particular Reference to Recommendations Relating to Future Relations with the British and Canadians," in *ibid.*, annex 6; Roosevelt to Bush, Dec. 28, 1942, PSF, Bush folder; Conant, "Memorandum on the interchange with the British and Canadians on S-1," Jan. 7, 1943, AEC doc. 152.

<sup>16</sup> Robert E. Sherwood, *Roosevelt and Hopkins, An Intimate History* (New York, 1948), 202, 704; Churchill to Roosevelt, Apr. 1, 1943, HHP, A-Bomb folder.

has ever seen if the potentialities of this project are realized." To provide the British with detailed knowledge about the construction of a bomb "might be the equivalent to joint occupation of a fortress or strategic harbor in perpetuity."<sup>17</sup> Though British and American atomic-energy policies might coincide during the war, Conant and Bush expected them to conflict afterward.

The controversy over the policy of "restricted interchange" of atomic-energy information shifted attention to postwar diplomatic considerations. As Bush wrote to Hopkins, "We can hardly give away the fruits of our developments as a part of postwar planning except on the basis of some overall agreement on that subject, which agreement does not now exist."<sup>18</sup> The central issue was clearly drawn. The atomic-energy policy of the United States was related to the very fabric of Anglo-American postwar relations and, as Churchill would insist, to postwar relations between each of them and the Soviet Union. Just as the possibility of British postwar commercial competition had played a major role in shaping the U.S. policy of restricted interchange, the specter of Soviet postwar military power played a major role in shaping the prime minister's attitude toward atomic-energy policies in 1943.

"We cannot," Sir John Anderson wrote Churchill, "afford after the war to face the future without this weapon and rely entirely on America should Russia or some other power develop it."<sup>19</sup> The prime minister agreed. The atomic bomb was an instrument of postwar diplomacy that Britain had to have. He could cite numerous reasons for his determination to acquire an independent atomic arsenal after the war, but Great Britain's postwar military-diplomatic position with respect to the Soviet Union invariably led the list. When Bush and Stimson visited London in July, Churchill told them quite frankly that he was "vitally interested in the possession of all [atomic-energy] information because this will be necessary for Britain's independence in the future as well as for success during the war." Nor was Churchill evasive about his reasoning: "It would never do to have Germany or Russia win the race for something which might be used for international blackmail," he stated bluntly and then pointed out that "Russia might be in a position to accomplish this result unless we worked together."<sup>20</sup> In Washington, two months earlier, Churchill's science adviser Lord Cherwell had told Bush and Hopkins virtually the same thing. The British government, Cherwell stated, was considering "the whole [atomic-energy] affair on an after-the-war military basis." It intended, he said, "to manufacture and produce the weapon."<sup>21</sup>

<sup>17</sup> Conant to Bush, Mar. 23, 1943, HHP, A-Bomb folder. My italics.

<sup>18</sup> Bush to Hopkins, Mar. 31, 1943, *ibid.*

<sup>19</sup> Anderson, quoted in Gowing, *Britain*, 168.

<sup>20</sup> Harvey Bundy, "Memorandum of Meeting at 10 Downing Street on July 23, 1943," in Groves, "DHMP," annex 11; Bush to Conant, July 23, 1943, AEC doc. 312.



Prior to the convening of the Quebec Conference, Anderson explained his own and Churchill's view of the bomb to the Canadian prime minister, Mackenzie King. The British knew, Anderson said, "that both Germany and Russia were working on the same thing," which, he noted, "would be a terrific factor in the postwar world as giving an absolute control to whatever country possessed the secret."<sup>22</sup> Convinced that the British attitude toward the bomb would undermine any possibility of postwar cooperation with the Soviet Union, Bush and Conant vigorously continued to oppose any revival of the Anglo-American atomic-energy partnership.<sup>23</sup>

On July 20, however, Roosevelt chose to accept a recommendation from Hopkins to restore full partnership, and he ordered Bush to "renew, in an inclusive manner, the full exchange of information with the British."<sup>24</sup> A garbled trans-Atlantic cable to Bush reading "review" rather than "renew" gave him the opportunity to continue his negotiations in London with Churchill and thereby to modify the president's order.<sup>25</sup> But Bush could not alter Roosevelt's intentions. On August 19, at the Quebec Conference, the president and the prime minister agreed that the British would share the atomic bomb. Despite Bush's negotiations with Churchill, the Quebec Agreement revived the principle of an Anglo-American atomic-energy partnership, albeit the British were reinstated as junior rather than equal partners.<sup>26</sup>

The president's decision was not a casual one taken in ignorance. As the official history of the Atomic Energy Commission notes: "Both Roosevelt and Churchill knew that the stake of their diplomacy was a technological breakthrough so revolutionary that it transcended in importance even the bloody work of carrying the war to the heartland of the Nazi foe."<sup>27</sup> The president had been informed of Churchill's position as well as of Bush's and Conant's.<sup>28</sup> But how much closer Roosevelt was to Churchill than to his own advisers at this time is suggested by a report

<sup>21</sup> Bush, "Memorandum of Conference with Mr. Harry Hopkins and Lord Cherwell at the White House, May 25, 1943," HHP, A-Bomb folder; Bush, "Memorandum of Conference with the President," June 24, 1943, AEC doc. 133.

<sup>22</sup> Anderson, quoted in J. W. Pickersgill, *The Mackenzie King Record, 1939-1944*, 1 (Chicago, 1960): 532.

<sup>23</sup> Their arguments can be followed in Hewlett and Anderson, *New World*, 270-80; but see also Bush to Hopkins, Mar. 31, 1943, HHP, A-Bomb folder.

<sup>24</sup> Hopkins to Roosevelt, July 20, 1943; Roosevelt to Bush, July 20, 1943; Roosevelt to Churchill, July 20, 1943, HHP, A-Bomb folder.

<sup>25</sup> Bush to Bundy, July 27, 28, 1943, AEC docs. 313, 314; Bush, "Memorandum for the File: Sequence of events concerning interchange with the British on the subject of S-1," Aug. 4, 1943, AEC doc. 168; Hewlett and Anderson, *New World*, 227.

<sup>26</sup> Articles of Agreement Governing Collaboration between the Authorities of the U.S.A. and the U.K. in the Matter of Tube Alloys (hereafter Quebec Agreement), in Groves, "DHMP," annex 18, also in Gowing, *Britain*, app. 4. "Tube Alloys" was the British code name for the atomic-energy project.

<sup>27</sup> Hewlett and Anderson, *New World*, 280.

<sup>28</sup> Bush, "Memorandum for the File: Sequence of events concerning interchange with the British on the subject of S-1," Aug. 4, 1943, AEC doc. 168.

written after the war by General Leslie R. Groves, military director of the atomic-energy project. "It is not known what if any Americans President Roosevelt consulted at Quebec," Groves wrote. "It is doubtful if there were any. All that is known is that the Quebec Agreement was signed by President Roosevelt and that, as finally signed, it agreed practically in toto with the version presented by Sir John Anderson to Dr. Bush in Washington a few weeks earlier."<sup>29</sup>

The debate that preceded the Quebec Agreement is noteworthy for yet another reason: it led to a new relationship between Roosevelt and his atomic-energy advisers. After August 1943 the president did not consult with them about the diplomatic aspects of atomic-energy policy. Though he responded politely when they offered their views, he acted decisively only in consultation with Churchill. Bush and Conant appear to have lost a large measure of their influence because they had used it to oppose Churchill's position. What they did not suspect was the extent to which the president had come to share the prime minister's view.

It can be argued that Roosevelt, the political pragmatist, renewed the wartime atomic-energy partnership to keep relations with the British harmonious rather than disrupt them on the basis of a postwar issue. Indeed it seems logical that the president took this consideration into account. But it must also be recognized that he was perfectly comfortable with the concept Churchill advocated—that military power was a prerequisite to successful postwar diplomacy. As early as August 1941, during the Atlantic Conference, Roosevelt had rejected the idea that an "effective international organization" could be relied upon to keep the peace; an Anglo-American international police force would be far more effective, he told Churchill.<sup>30</sup> By the spring of 1942 the concept had broadened: the two "policemen" became four, and the idea was added that every other nation would be totally disarmed. "The Four Policemen" would have "to build up a reservoir of force so powerful that no aggressor would dare to challenge it," Roosevelt told Arthur Sweetser, an ardent internationalist. Violators first would be quarantined, and, if they persisted in their disruptive activities, bombed at the rate of a city a day until they agreed to behave. The president told Molotov about this idea in May, and in November he repeated it to Clark Eichelberger, who was coordinating the activities of the American internationalists. A year later, at the Teheran Conference, Roosevelt again discussed his idea, this time with Stalin. As Robert A. Divine has noted: "Roosevelt's concept of big power domination remained the central idea in his approach to international organization throughout World War II."<sup>31</sup>

<sup>29</sup> Groves, "DHMP," 15.

<sup>30</sup> *Foreign Relations of the United States* (hereafter *FRUS*), 1941, 1 (Washington, 1958): 363, 365-66.

<sup>31</sup> Roosevelt, quoted in "Mr. Sweetser's Notes," May 29, 1942, Arthur Sweetser Papers, box 39, Library of Congress (hereafter LC). This memorandum was brought to my attention by Mr.

Precisely how Roosevelt expected to integrate the atomic bomb into his plans for keeping the peace in the postwar world is not clear. However, against the background of his atomic-energy policy decisions of 1943 and his peace-keeping concepts, his actions in 1944 suggest that he intended to take full advantage of the bomb's potential as a postwar instrument of Anglo-American diplomacy. If Roosevelt thought the bomb could be used to create a more peaceful world order, he seems to have considered the threat of its power more effective than any opportunities it offered for international cooperation. If Roosevelt was less worried than Churchill about Soviet postwar ambitions, he was no less determined than the prime minister to avoid any commitments to the Soviets for the international control of atomic energy. There could still be four policemen, but only two of them would have the bomb.

THE ATOMIC-ENERGY POLICIES Roosevelt pursued during the remainder of his life reinforce this interpretation of his ideas for the postwar period. The following three questions offer a useful framework for analyzing his intentions. Did Roosevelt make any additional agreements with Churchill that would further support the view that he intended to maintain an Anglo-American monopoly after the war? Did Roosevelt demonstrate any interest in the international control of atomic energy? Was Roosevelt aware that an effort to maintain an Anglo-American monopoly of the atomic bomb might lead to a postwar atomic arms race with the Soviet Union?

An examination of the wartime activities of the eminent Danish physicist, Niels Bohr, who arrived in America early in 1944 as a consultant to the atomic-bomb project, will help answer these questions. "Officially and secretly he came to help the technical enterprise," noted J. Robert Oppenheimer, the director of the Los Alamos atomic-bomb laboratory, but "most secretly of all . . . he came to advance his case and his cause."<sup>32</sup> Bohr was convinced that a postwar atomic armaments race with the Soviet Union was inevitable unless Roosevelt and Churchill initiated efforts during the war to establish the international control of atomic energy.

Günter Brauch. For Roosevelt's remarks to Molotov, see *FRUS*, 1942, 3 (Washington, 1961): 573; to Eichelberger, see "President's Conversation at Luncheon with G.G.T. [Grace G. Tully] and S.I.R. [Samuel I. Rosenman], Nov. 13, 1942," in Elliott Roosevelt, ed., *F.D.R.: His Personal Letters, 1928-1945*, 4 (New York, 1950): 1366-67. See also *FRUS: The Conferences at Cairo and Teheran, 1943* (Washington, 1961), 530-32, and Robert A. Divine, *Roosevelt and World War II* (Baltimore, 1970), 58. And see Willard Range, *Franklin D. Roosevelt's World Order* (Athens, Ga., 1959), and Roland Stromberg, *Collective Security and American Foreign Policy: From the League of Nations to NATO* (New York, 1963).

<sup>32</sup> The details of Bohr's escape from Nazi-occupied Denmark can be followed in Gowing, *Britain*, 245-50; Oppenheimer's remarks can be found in his "Niels Bohr and Atomic Weapons," *New York Review of Books*, Dec. 17, 1966, p. 7. For additional information about Bohr's wartime activities, see also Liva Baker, *Felix Frankfurter* (New York, 1969), 271-80, and Ruth Moore, *Niels Bohr: The Man, His Science, and the World They Changed* (New York, 1966), 270-393.

Bohr's attempts to promote this idea in the United States were aided by Justice Felix Frankfurter.

Bohr and Frankfurter were old acquaintances. They had first met in 1933 at Oxford and then in 1939 on several occasions in London and the United States. At these meetings Bohr had been impressed by the breadth of Frankfurter's interests and, perhaps, overimpressed with his influence on Roosevelt. In 1944 the Danish minister to the United States brought them together, once again, at his home in Washington. Frankfurter, who appears to have suspected why Bohr had come to America and why this meeting had been arranged, had learned about the atomic-bomb project earlier in the war when, as he told the story, several troubled scientists had sought his advice on a matter of "greatest importance." He therefore invited Bohr to lunch in his chambers and, by dropping hints about his knowledge, encouraged Bohr to discuss the issue.<sup>33</sup>

After listening to Bohr's analysis of the postwar alternatives—an atomic armaments race or some form of international control—Frankfurter saw Roosevelt. Bohr had persuaded him, Frankfurter reported, that disastrous consequences would result if Russia learned on her own about the atomic-bomb project. Frankfurter suggested that it was a matter of great importance that the president explore the possibility of seeking an effective arrangement with the Soviets for controlling the bomb. He also noted that Bohr, whose knowledge of Soviet science was extensive, believed that the Russians had the capability to build their own atomic weapons. If the international control of atomic energy was not discussed among the Allies during the war, an atomic arms race between the Allies would almost certainly develop after the war. It seemed imperative, therefore, that Roosevelt consider approaching Stalin with a proposal as soon as possible.<sup>34</sup>

Frankfurter discussed these points with the president for an hour and a half, and he left feeling that Roosevelt was "plainly impressed by my account of the matter." When Frankfurter had suggested that the solution to this problem might be more important than all the plans for a world organization, Roosevelt had agreed. Moreover he had authorized Frankfurter to tell Bohr, who was scheduled to return to England, that he might inform "our friends in London that the President was most eager to explore the proper safeguards in relation to X [the atomic bomb]." Roosevelt also told Frankfurter that the problem of the atomic bomb "worried him to death" and that he was very eager for all the help he could have in dealing with it.<sup>35</sup>

<sup>33</sup> Frankfurter to Lord Halifax, Apr. 18, 1945, J. Robert Oppenheimer Papers (hereafter JROP), box 34, Frankfurter-Bohr folder, LC. I have been unable to discover the details surrounding Frankfurter's early informants.

<sup>34</sup> *Ibid.*; see also Gowing, *Britain*, 346-56.

<sup>35</sup> *Ibid.*

The alternatives placed before Roosevelt posed a difficult dilemma. On the one hand, he could continue to exclude the Soviet government from any official information about the development of the bomb, a policy that would probably strengthen America's postwar military-diplomatic position. But such a policy would also encourage Soviet mistrust of Anglo-American intentions and was bound to make postwar cooperation more difficult. On the other hand, Roosevelt could use the atomic-bomb project as an instrument of cooperation by informing Stalin of the American government's intention of cooperating in the development of a plan for the international control of atomic weapons, an objective that might never be achieved.

Either choice involved serious risks. Roosevelt had to balance the diplomatic advantages of being well ahead of the Soviet Union in atomic-energy production after the war against the advantages of initiating war-time negotiations for postwar cooperation. The issue here, it must be emphasized, is not whether the initiative Bohr suggested would have led to successful international control, but rather whether Roosevelt demonstrated any serious interest in laying the groundwork for such a policy.

Several considerations indicate that Roosevelt was already committed to a course of action that precluded Bohr's internationalist approach. First, Frankfurter appears to have been misled. Though Roosevelt's response had been characteristically agreeable, he did not mention Bohr's ideas to his atomic-energy advisers until September 1944, when he told Bush that he was very disturbed that Frankfurter had learned about the project.<sup>36</sup> Roosevelt knew at this time, moreover, that the Soviets were finding out on their own about the development of the atomic bomb. Security personnel had reported an active Communist cell in the Radiation Laboratory at the University of California. Their reports indicated that at least one scientist at Berkeley was selling information to Russian agents.<sup>37</sup> "They [Soviet agents] are already getting information about vital secrets and sending them to Russia," Stimson told the president on September 9, 1943. If Roosevelt was indeed worried to death about the effect the atomic bomb could have on Soviet-American postwar relations, he took no action to remove the potential danger, nor did he make any effort to explore the possibility of encouraging Soviet postwar cooperation on this problem. The available evidence indicates that he

<sup>36</sup> Bush, "Memorandum of Conference," Sept. 22, 1944, AEC doc. 185. Selected portions of this memorandum have been reprinted in *FRUS, The Conference at Quebec, 1944* (Washington, 1972), see especially 492n., but also see 296. Bush left this conference with the erroneous impression that Roosevelt had first become acquainted with Bohr's ideas several weeks earlier.

<sup>37</sup> See the testimony of Groves and of John Lansdale, Jr., in United States Atomic Energy Commission, *In the Matter of J. Robert Oppenheimer: Transcript of Hearing before Personnel Security Board* (Washington, 1954), 163-80, especially 171-74, and 258-81; Nuel Pharr Davis, *Lawrence and Oppenheimer* (New York, 1968), 191-92. Though the exact information being passed was not known, there can be no doubt that by the spring of 1944 Roosevelt was aware of Soviet interest in the Manhattan Project. See also Bohr to Roosevelt, July 3, 1944, JROP, box 34, Frankfurter-Bohr folder.

never discussed the merits of the international control of atomic energy with his advisers after this first or any subsequent meeting with Frankfurter.<sup>38</sup>

How is the president's policy of neither discussing international control nor promoting the idea to be explained if not by an intention to use the bomb as an instrument of Anglo-American postwar diplomacy? Perhaps his concern for maintaining the tightest possible secrecy against German espionage led him to oppose any discussion about the project. Or he may have concluded, after considering Bohr's analysis, that Soviet suspicion and mistrust would be further aroused if Stalin were informed of the existence of the project without receiving detailed information about the bomb's construction. The possibility also exists that Roosevelt believed that neither Congress nor the American public would approve of a policy giving the Soviet Union any measure of control over the new weapon. Finally Roosevelt might have thought that the spring of 1944 was not the proper moment for such an initiative.

Though it would be unreasonable to state categorically that these considerations did not contribute to his decision, they appear to have been secondary. Roosevelt was clearly, and properly, concerned about secrecy, but the most important secret with respect to Soviet-American relations was that the United States was developing an atomic bomb. And that secret, he was aware, already had been passed on to Moscow. Soviet mistrust of Anglo-American postwar intentions could only be exacerbated by continuing the existing policy. Moreover an attempt to initiate planning for international control of atomic energy would not have required the revelation of technical secrets. Nor is it sufficient to cite Roosevelt's well-known sensitivity to domestic politics as an explanation for his atomic-energy policies. He was willing to take enormous political risks, as he did at Yalta, to support his diplomatic objectives.<sup>39</sup>

Had Roosevelt avoided all postwar atomic-energy commitments, his lack of support for international control could have been interpreted as an attempt to reserve his opinion on the best course to follow. But he had made commitments in 1943 supporting Churchill's monopolistic, anti-Soviet position, and he continued to make others in 1944. On June 13, for example, Roosevelt and Churchill signed an Agreement and Declaration of Trust, specifying that the United States and Great Britain would cooperate in seeking to control available supplies of uranium and thorium ore both during and after the war.<sup>40</sup> This commitment, taken against the background of Roosevelt's peace-keeping ideas and his other

<sup>38</sup> Stimson, diary, Sept. 9, 1943. Frankfurter met several times with the president and discussed Bohr's proposal with him in great detail. See Max Freedman, ed., *Roosevelt and Frankfurter: Their Correspondence, 1928-1945* (Boston, 1967), 725.

<sup>39</sup> Diane Shaver Clemens, *Yalta* (New York, 1970).

<sup>40</sup> Agreement and Declaration of Trust, June 13, 1944, in Groves, "DHMP," annex 22a, also in Gowing, *Britain*, app. 7.

commitments, suggests that the president's attitude toward the international control of atomic energy was similar to the prime minister's.

Churchill had dismissed out of hand the concept of international control when Bohr talked with him about it in May 1944. Their meeting was not long under way before Churchill lost interest and became involved in an argument with Lord Cherwell, who was also present. Bohr, left out of the discussion, was frustrated and depressed; he was unable to return the conversation to what he considered the most important diplomatic problem of the war. When the allotted half hour elapsed, Bohr asked if he might send the prime minister a memorandum on the subject. A letter from Niels Bohr, Churchill bitinglly replied, was always welcome, but he hoped it would deal with a subject other than politics. As Bohr described their meeting: "We did not even speak the same language."<sup>41</sup>

Churchill rejected the assumption upon which Bohr's views were founded—that international control of atomic energy could be used as a cornerstone for constructing a peaceful world order. An atomic monopoly would be a significant diplomatic advantage in postwar diplomacy, and Churchill did not believe that anything useful could be gained by surrendering this advantage. The argument that a new weapon created a unique opportunity to refashion international affairs ignored every lesson Churchill read into history. "You can be quite sure," he would write in a memorandum less than a year later, "that any power that gets hold of the secret will try to make the article and this touches the existence of human society. This matter is out of all relation to anything else that exists in the world, and I could not think of participating in any disclosure to third or fourth parties at the present time."<sup>42</sup>

Several months after Bohr met Churchill, Frankfurter arranged a meeting between Bohr and Roosevelt. Their discussion lasted an hour and a half. Roosevelt told Bohr that contact with the Soviet Union along the lines he suggested had to be tried. The president also said he was optimistic that such an initiative would have a "good result." In his opinion Stalin was enough of a realist to understand the revolutionary importance of this development and its consequences. The president also expressed confidence that the prime minister would eventually share these views. They had disagreed in the past, he told Bohr, but they had always succeeded in resolving their differences.<sup>43</sup>

Roosevelt's enthusiasm for Bohr's ideas was more apparent than real. The president did not mention them to anyone until he met with Church-

<sup>41</sup> Bohr, quoted in Gowing, *Britain*, 355; Bohr to Churchill, May 22, 1944, JROP, box 34, Frankfurter-Bohr folder.

<sup>42</sup> Churchill, quoted in Gowing, *Britain*, 360.

<sup>43</sup> Aage Bohr, "The War Years and the Prospects Raised by the Atomic Weapons," in Stefan Rozental, ed., *Niels Bohr* (New York, 1967), 206-07. In preparation for this interview, which took place on August 26, 1944, Bohr sent Roosevelt, through Frankfurter, a long memorandum: see Bohr to Frankfurter, July 5, 1944, JROP, box 34, Frankfurter-Bohr folder.

ill at Hyde Park on September 18, following the second wartime conference at Quebec. The decisions reached on atomic energy at Hyde Park were summarized and documented in an *aide-mémoire* signed by Roosevelt and Churchill on September 19, 1944. The agreement bears the markings of Churchill's attitude toward the atomic bomb and his poor opinion of Bohr. "Enquiries should be made," the last paragraph reads, "regarding the activities of Professor Bohr and steps taken to ensure that he is responsible for no leakage of information particularly to the Russians." If Bohr's activities prompted Roosevelt to suspect his loyalty, there can be no doubt that Churchill encouraged the president's suspicions. Atomic energy and Britain's future position as a world power had become part of a single equation for the prime minister. Bohr's ideas, like the earlier idea of restricted interchange, threatened the continuation of the Anglo-American atomic-energy partnership. With such great stakes at issue Churchill did not hesitate to discredit Bohr along with his ideas. "It seems to me," Churchill wrote to Cherwell soon after Hyde Park, "Bohr ought to be confined or at any rate made to see that he is very near the edge of mortal crimes."<sup>44</sup>

The *aide-mémoire* also contained an explicit rejection of any wartime efforts toward international control: "The suggestion that the world should be informed regarding tube alloys [the atomic bomb], with a view to an international agreement regarding its control and use, is not accepted. The matter should continue to be regarded as of the utmost secrecy."<sup>45</sup> But Bohr had never suggested that the world be informed about the atomic bomb. He had argued in memorandums and in person that peace was not possible unless the Soviet government—not the world—was officially notified only about the project's existence before the time when any discussion would appear coercive rather than friendly.

It was the second paragraph, however, that revealed the full extent of Roosevelt's agreement with Churchill's point of view. "Full collaboration between the United States and the British Government in developing tube alloys for military and commercial purposes," it noted, "should continue after the defeat of Japan unless and until terminated by joint agreement." Finally the *aide-mémoire* offers some insight into Roosevelt's intentions for the military use of the weapon in the war: "When a bomb is finally available, it might perhaps, after mature consideration, be used against the Japanese, who should be warned that this bombardment will be repeated until they surrender."<sup>46</sup>

Within the context of the complex problem of the origins of the cold war the Hyde Park meeting is far more important than historians of

<sup>44</sup> For the *aide-mémoire*, see Gowing, *Britain*, app. 8; for Churchill's communication to Cherwell, see page 358.

<sup>45</sup> *Ibid.*, 447.

<sup>46</sup> *Ibid.*



the war generally have recognized.<sup>47</sup> Overshadowed by the Second Quebec Conference on one side and by the drama of Yalta on the other, its significance often has been overlooked. But the agreements reached in September 1944 reflect a set of attitudes, aims, and assumptions that guided the relationship between the atomic bomb and American diplomacy during the Roosevelt administration and, through the transfer of its atomic legacy, during the Truman administration as well. Two alternatives had been recognized long before Roosevelt and Churchill met in 1944 at Hyde Park: the bomb could have been used to initiate a diplomatic effort to work out a system for its international control, or it could remain isolated during the war from any cooperative initiatives and held in reserve should cooperation fail. Roosevelt consistently favored the latter alternative. An insight into his reasoning is found in a memorandum Bush wrote following a conversation with Roosevelt several days after the Hyde Park meeting: "The President evidently thought he could join with Churchill in bringing about a US-UK postwar agreement on this subject [the atomic bomb] by which it would be held closely and presumably to control the peace of the world."<sup>48</sup> By 1944 Roosevelt's earlier musings about the four policemen had faded into the background. But the idea behind it, the concept of controlling the peace of the world by amassing overwhelming military power, appears to have remained a prominent feature of his postwar plans.

IN THE SEVEN MONTHS between his meeting with Churchill in September and his death the following April Roosevelt did not alter his atomic-energy policies. Nor did he reverse his earlier decision not to take his advisers into his confidence about diplomatic issues related to the new weapon. They were never told about the Hyde Park agreements, nor were they able to discuss with him their ideas for the postwar handling of atomic-energy affairs. Though officially uninformed, Bush suspected that Roosevelt had made a commitment to continue the atomic-energy partnership exclusively with the British after the war, and he, as well as Conant, opposed the idea. They believed such a policy "might well lead to extraordinary efforts on the part of Russia to establish its own position in the field secretly, and might lead to a clash, say 20 years from now."<sup>49</sup> Unable to reach the president directly, they sought to influence his policies through Stimson, whose access to Roosevelt's office (though not to his thoughts on atomic energy) was better than their own.

Summarizing their views on September 30 for the secretary of war,

<sup>47</sup> Herbert Feis mentions it in *The Atomic Bomb and the End of World War II* (Princeton, 1966), 33-34. He does not, however, draw out its full implications. See also John Lewis Gaddis, *The United States and the Origins of the Cold War, 1941-1947* (New York, 1972), 87.

<sup>48</sup> Bush to Conant, Sept. 25, 1944, AEC doc. 280.

<sup>49</sup> *Ibid.*

Bush and Conant predicted that an atomic bomb equivalent to from one to ten thousand tons of high explosive could be "demonstrated" before August 1, 1945. They doubted that the present American and British monopoly could be maintained for more than three or four years, and they pointed out that any nation with good technical and scientific resources could catch up; accidents of research, moreover, might even put some other nation ahead. In addition atomic bombs were only the first step along the road of nuclear weapons technology. In the not-too-distant future loomed the awesome prospect of a weapon perhaps a thousand times more destructive—the hydrogen bomb. Every major center of population in the world would then lie at the mercy of a nation that struck first in war. Security therefore could be found neither in secrecy nor even in the control of raw materials, for the supply of heavy hydrogen was practically unlimited.<sup>50</sup>

These predictions by Bush and Conant were more specific than Bohr's, but not dissimilar. They, too, believed that a nuclear arms race could be prevented only through international control. Their efforts were directed, however, toward abrogating existing agreements with the British rather than toward initiating new agreements with the Soviets. Like Bohr they based their hope for Stalin's eventual cooperation on his desire to avoid the circumstances that could lead to a nuclear war. But while Bohr urged Roosevelt to approach Stalin with the carrot of international control before the bomb became a reality, Bush and Conant were inclined to delay such an approach until the bomb was demonstrated, until it was clear that without international control the new weapon could be used as a terribly effective stick.

In their attempt to persuade Roosevelt to their point of view Bush and Conant failed. But their efforts were not in vain. By March 1945 Stimson shared their concerns, and he agreed that peace without international control was a forlorn hope. Postwar problems relating to the atomic bomb "went right down to the bottom facts of human nature, morals and government, and it is by far the most searching and important thing that I have had to do since I have been here in the office of Secretary of War," Stimson wrote on March 5. Ten days later he presented his views on postwar atomic-energy policy to Roosevelt.<sup>51</sup> This was their last meeting. In less than a month a new president took the oath of office.

Harry S. Truman inherited a set of military and diplomatic atomic-energy policies that included partially formulated intentions, several commitments to Churchill, and the assumption that the bomb would be a legitimate weapon to be used against Japan. But no policy was def-

<sup>50</sup> Bush and Conant to Stimson, Sept. 30, 1944, AEC doc. 281; Hewlett and Anderson, *New World*, 328-29.

<sup>51</sup> Stimson, diary, Mar. 5, 15, 1945.

initely settled. According to the Quebec Agreement the president had the option of deciding the future of the commercial aspects of the atomic-energy partnership according to his own estimate of what was fair.<sup>52</sup> Although the policy of "utmost secrecy" had been confirmed at Hyde Park the previous September, Roosevelt had not informed his atomic-energy advisers about the *aide-mémoire* he and Churchill signed. Although the assumption that the bomb would be used in the war was shared by those privy to its development, assumptions formulated early in the war were not necessarily valid at its conclusion. Yet Truman was bound to the past by his own uncertain position and by the prestige of his predecessor.<sup>53</sup> Since Roosevelt had refused to open negotiations with the Soviet government for the international control of atomic energy, and since he had never expressed any objection to the wartime use of the bomb, it would have required considerable political courage and confidence for Truman to alter those policies. Moreover it would have required the encouragement of his advisers, for under the circumstances the most serious constraint on the new president's choices was his dependence upon advice. So Truman's atomic legacy, while it included several options, did not necessarily entail complete freedom to choose from among all the possible alternatives.

"I think it is very important that I should have a talk with you as soon as possible on a highly secret matter," Stimson wrote to Truman on April 24. It has "such a bearing on our present foreign relations and has such an important effect upon all my thinking in this field that I think you ought to know about it without further delay."<sup>54</sup> Stimson had been preparing to brief Truman on the atomic bomb for almost ten days, but in the preceding twenty-four hours he had been seized by a sense of urgency. Relations with the Soviet Union had declined precipitously during the past week, the result, he thought, of the failure of the State Department to settle the major problems between the Allies before going ahead with the San Francisco Conference on the United Nations Organization. The secretary of state, Edward R. Stettinius, Jr., along with the department's Soviet specialists, now felt "compelled to bull the thing through." To get out of the "mess" they had created, Stimson wrote in his diary, they were urging Truman to get tough with the Russians.<sup>55</sup> He had. Twenty-four hours earlier the president met with the Soviet foreign minister, V. M. Molotov, and "with rather brutal frankness" accused his government of breaking the Yalta Agreement. Molotov was

<sup>52</sup> See point four of the Quebec Agreement, in Gowing, *Britain*, app. 4, p. 439.

<sup>53</sup> Kenneth Glazier, Jr., "The Decision to Use Atomic Weapons against Hiroshima and Nagasaki," *Public Policy*, 18 (1969): 463-516. Glazier emphasizes the bureaucratic momentum toward this decision.

<sup>54</sup> Stimson to Truman, Apr. 24, 1945, in Truman, *Memoirs*, vol. 1: *Year of Decisions* (Garden City, 1955), 85.

<sup>55</sup> Stimson, diary, Apr. 23, 1945.

furious. "I have never been talked to like that in my life," he told the president before leaving.<sup>56</sup>

With a memorandum on the "political aspects of the S-1 [atomic bomb's] performance" in hand and General Groves in reserve, Stimson went to the White House on April 25. The document he carried was the distillation of numerous decisions already taken, each one the product of attitudes that developed along with the new weapon. The secretary himself was not entirely aware of how various forces had shaped these decisions: the recommendations of Bush and Conant, the policies Roosevelt had followed, the uncertainties inherent in the wartime alliance, the oppressive concern for secrecy, and his own inclination to consider long-range implications. It was a curious document. Though its language revealed Stimson's sensitivity to the historic significance of the atomic bomb, he did not question the wisdom of using it against Japan. Nor did he suggest any concrete steps for developing a postwar policy. His objective was to inform Truman of the salient problems: the possibility of an atomic arms race, the danger of atomic war, and the necessity for international control if the United Nations Organization was to work. "If the problem of the proper use of this weapon can be solved," he wrote, "we would have the opportunity to bring the world into a pattern in which the peace of the world and our civilizations can be saved." To cope with this difficult challenge Stimson suggested the "establishment of a select committee" to consider the postwar problems inherent in the development of the bomb.<sup>57</sup> If his presentation was the "forceful statement" of the problem that historians of the Atomic Energy Commission have described it as being,<sup>58</sup> its force inhered in the problem itself, not in any bold formulations or initiatives he offered toward a solution. If, as another historian has claimed, this meeting led to a "strategy of delayed showdown," requiring "the delay of all disputes with Russia until the atomic bomb had been demonstrated,"<sup>59</sup> there is no evidence in the extant records of the meeting that Stimson had such a strategy in mind or that Truman misunderstood the secretary's views.

<sup>56</sup> Truman, *Year of Decisions*, 82; William D. Leahy, *I Was There* (New York, 1950), 351; Walter Millis, ed., *The Forrestal Diaries* (New York, 1951), 48-51.

<sup>57</sup> Stimson, "Memorandum discussed with the President," in his diary, Apr. 25, 1945.

<sup>58</sup> Hewlett and Anderson, *New World*, 343.

<sup>59</sup> Alperovitz, *Atomic Diplomacy*, 41-61, 270, *passim*. For sharply drawn essays by Alperovitz based on the themes and research in *Atomic Diplomacy*, see his *Cold War Essays* (New York, 1970), especially "How Did the Cold War Begin?" 35-50, and "The Use of the Atomic Bomb," 51-73. For essays that evaluate the contributions and deficiencies of *Atomic Diplomacy*, see Christopher Lasch, "The Cold War, Revisited and Re-Visioned," *New York Times Magazine*, Jan. 14, 1968, pp. 26-27, 44-59; Charles Maier, "Revisionism and the Interpretation of Cold War Origins," *Perspectives in American History*, 4 (1970): 313-47; Martin J. Sherwin, "The Atomic Bomb As History," *Wisconsin Magazine of History*, 53 (1969-70): 128-34; and Athan Theoharis, "Atomic Diplomacy," *New University Thought*, 5 (1967): 12, 73-77. For a recent, strained attack on Alperovitz's use of evidence, see Robert James Maddox, "Atomic Diplomacy: A Study in Creative Writing," *Journal of American History*, 59 (1973): 925-34; Alperovitz's response can be found on pages 1062-67.

What emerges from a careful reading of Stimson's diary, his memorandum of April 25 to Truman, a summary by Groves of the meeting, and Truman's recollections is an argument for overall caution in American diplomatic relations with the Soviet Union:<sup>60</sup> it was an argument against any showdown. Since the atomic bomb was potentially the most dangerous issue facing the postwar world and since the most desirable resolution of the problem was some form of international control, Soviet cooperation had to be secured. It was imprudent, Stimson suggested, to pursue a policy that would preclude the possibility of international cooperation on atomic-energy matters after the war ended. Truman's overall impression of Stimson's argument was that the secretary of war was "at least as much concerned with the role of the atomic bomb in the shaping of history as in its capacity to shorten the war."<sup>61</sup> These were indeed Stimson's dual concerns on April 25, and he could see no conflict between them.

Despite the profound consequences Stimson attributed to the development of the new weapon, he had not suggested that Truman reconsider its use against Japan. Nor had he thought to mention the possibility that chances of securing Soviet postwar cooperation might be diminished if Stalin did not receive a commitment to international control prior to an attack. The question of why these alternatives were overlooked naturally arises. Perhaps what Frankfurter once referred to as Stimson's habit of setting "his mind at one thing like the needle of an old victrola caught in a single groove" may help to explain his not mentioning these possibilities.<sup>62</sup> Yet Bush and Conant never raised them either. Even Niels Bohr had made a clear distinction between the bomb's wartime use and its postwar impact on diplomacy. "What role it [the atomic bomb] may play in the present war," Bohr had written to Roosevelt in July 1944, was a question "quite apart" from the overriding concern: the need to avoid an atomic arms race.<sup>63</sup>

The preoccupation with winning the war obviously helped to create this seeming dichotomy between the wartime use of the bomb and the potential postwar diplomatic problems with the Soviet Union raised by its development. But a closer look at how Bohr and Stimson each defined the nature of the diplomatic problem created by the bomb suggests that for the secretary of war and his advisers (and ultimately for the president they advised) there was no dichotomy at all. Bohr apprehended the meaning of the new weapon even before it was developed, and he had no doubt

<sup>60</sup> Stimson, diary, Apr. 25, 1945; Groves, "Report of Meeting with the President, Apr. 25, 1945," in Records of the Chief of Engineers, Commanding General's File 24, tab. D, MED Files; Truman, *Year of Decisions*, 87.

<sup>61</sup> Truman, *Year of Decisions*, 87.

<sup>62</sup> Frankfurter, quoted in Elting E. Morison, *Turmoil and Tradition: A Study of the Life and Times of Henry L. Stimson* (Boston, 1960), 167-68.

<sup>63</sup> Bohr to Roosevelt, July 3, 1944, JROP, box 34, Frankfurter-Bohr folder.

that scientists in the Soviet Union would also understand its profound implications for the postwar world. He was also certain that they would interpret the meaning of the development to Stalin just as scientists in the United States and Great Britain had explained it to Roosevelt and Churchill. Thus the diplomatic problem, as Bohr analyzed it, was not the need to convince Stalin that the atomic bomb was an unprecedented weapon that threatened the life of the world but the need to assure the Soviet leader that he had nothing to fear from the circumstances of its development. By informing Stalin during the war that the United States intended to cooperate with him in neutralizing the bomb through international control, Bohr reasoned that its wartime use could be considered apart from postwar problems.

Stimson approached the problem rather differently. Although he believed that the bomb "might even mean the doom of civilization or it might mean the perfection of civilization" he was less confident than Bohr that the weapon in an undeveloped state could be used as an effective instrument of diplomacy. Until its "actual certainty [was] fixed," Stimson considered any prior approach to Stalin as premature.<sup>64</sup> But as the uncertainties of impending peace became more apparent and worrisome, Stimson, Truman, and the secretary of state-designate, James F. Byrnes, began to think of the bomb as something of a diplomatic panacea for their postwar problems. Byrnes had told Truman in April that the bomb "might well put us in a position to dictate our own terms at the end of the war."<sup>65</sup> By June, Truman and Stimson were discussing "further *quid pro quos* which should be established in consideration for our taking them [the Soviet Union] into [atomic-energy] partnership." Assuming that the bomb's impact on diplomacy would be immediate and extraordinary, they agreed on no less than "the settlement of the Polish, Rumanian, Yugoslavian, and Manchurian problems." But they also concluded that no revelation would be made "to Russia or anyone else until the first bomb had been successfully laid on Japan."<sup>66</sup> Truman and Stimson based their expectations on how they saw and valued the bomb; its use against Japan, they reasoned, would transfer this view to the Soviet Union.

Was an implicit warning to Moscow, then, the principal reason for deciding to use the atomic bomb against Japan? In light of the ambiguity of the available evidence the question defies an unequivocal answer. What can be said with certainty is that Truman, Stimson, Byrnes, and several others involved in the decision consciously considered two effects of a combat demonstration of the bomb's power: first, the impact of the atomic attack on Japan's leaders, who might be persuaded thereby to end

<sup>64</sup> Stimson, diary, May 31, 1945.

<sup>65</sup> Byrnes, quoted in Truman, *Year of Decisions*, 87.

<sup>66</sup> Stimson, diary, June 6, 1945.

the war; and second, the impact of that attack on the Soviet Union's leaders, who might then prove to be more cooperative. But if the assumption that the bomb might bring the war to a rapid conclusion was the principal motive for using the atomic bomb, the expectation that its use would also inhibit Soviet diplomatic ambitions clearly discouraged any inclination to question that assumption.

Policy makers were not alone in expecting a military demonstration of the bomb to have a salubrious effect on international affairs. James Conant, for example, believed that such a demonstration would further the prospects for international control. "President Conant has written me," Stimson informed the news commentator Raymond Swing in February 1947, "that one of the principal reasons he had for advising me that the bomb must be used was that that was the only way to awaken the world to the necessity of abolishing war altogether." And the director of the atomic-energy laboratory at the University of Chicago made the same point to Stimson in June 1945: "If the bomb were not used in the present war," Arthur Compton noted, "the world would have no adequate warning as to what was to be expected if war should break out again." Even Edward Teller, who has publicly decried the attack on Hiroshima and declared his early opposition to it, adopted a similar position in July 1945. "Our only hope is in getting the facts of our results before the people," he wrote to his colleague, Léo Szilard, who was circulating a petition among scientists opposing the bomb's use. "This might help to convince everybody that the next war would be fatal," Teller noted. "For this purpose actual combat use might even be the best thing."<sup>67</sup>

Thus by the end of the war the most influential and widely accepted attitude toward the bomb was a logical extension of how the weapon was seen and valued earlier—as a potential instrument of diplomacy. Caught between the remnants of war and the uncertainties of peace, scientists as well as policy makers were trapped by the logic of their own unquestioned assumptions. By the summer of 1945 not only the conclusion of the war but the organization of an acceptable peace seemed to depend upon the success of the atomic attacks against Japan. When news of the successful atomic test of July 16 reached the president at the Potsdam Conference, he was visibly elated.<sup>68</sup> Stimson noted that Truman "was

<sup>67</sup> Stimson to Swing, Feb. 4, 1947, Stimson Papers; Arthur H. Compton, *Atomic Quest: A Personal Narrative* (New York, 1956), 236. The history of Szilard's petition is described in the following works: Leo Szilard, "Reminiscences," ed. Gertrude Weiss Szilard and Katherine R. Winsor, in Donald Fleming and Bernard Bailyn, eds., *The Intellectual Migration: Europe and America, 1930-1960* (Cambridge, Mass., 1969), 129-32; Alice K. Smith, "Behind the Decision to Use the Atomic Bomb: Chicago, 1944-45," *Bulletin of the Atomic Scientists*, Jan. 1957, pp. 303-05; Hewlett and Anderson, *New World*, 399-400; Teller to Szilard, July 2, 1945, JROP, box 71, Teller folder.

<sup>68</sup> Truman scheduled the Potsdam Conference to coincide with the test of the atomic bomb. See Stimson, diary, June 6, 1945; Joseph E. Davies, diary, May 21, 1945, Joseph E. Davies Papers, box 17, LC; Hewlett and Anderson, *New World*, 352; and Alperovitz, *Atomic Diplomacy*, 62-90.

tremendously pepped up by it and spoke to me of it again and again when I saw him. He said it gave him an entirely new feeling of confidence." The day after receiving the complete report of the test Truman altered his negotiating style. According to Churchill the president "got to the meeting after having read this report [and] he was a changed man. He told the Russians just where they got on and off and generally bossed the whole meeting."<sup>69</sup> After the plenary session on July 24 Truman "casually mentioned to Stalin" that the United States had "a new weapon of unusual destructive force."<sup>70</sup> Truman took this step in response to a recommendation by the Interim Committee, a group of political and scientific advisers organized by Stimson in May 1945 to advise the president on atomic-energy policy.<sup>71</sup> But it is an unavoidable conclusion that what the president told the premier followed the letter of the recommendation rather than its spirit, which embodied the hope that an overture to Stalin would initiate the process toward international control.<sup>72</sup> In less than three weeks the new weapon's destructive potential would be demonstrated to the world. Stalin would then be forced to reconsider his diplomatic goals. It is no wonder that upon learning of the raid against Hiroshima Truman exclaimed: "This is the greatest thing in history."<sup>73</sup>

As Stimson had expected, as a colossal reality the bomb was very different. But had American diplomacy been altered by it? Those who conducted diplomacy became more confident, more certain that through the accomplishments of American science, technology, and industry the "new world" could be made into one better than the old. But just how the atomic bomb would be used to help accomplish this ideal remained unclear. Three months and one day after Hiroshima was bombed Bush wrote that the whole matter of international relations on atomic energy "is in a thoroughly chaotic condition."<sup>74</sup> The wartime relationship between atomic-energy policy and diplomacy had been based upon the simple assumption that the Soviet government would surrender important geographical, political, and ideological objectives in exchange

<sup>69</sup> Stimson, diary, July 21, 1945; Churchill is quoted in *ibid.*, July 22, 1945.

<sup>70</sup> Truman, *Year of Decisions*, 416. Stalin knew that Truman was referring to the atomic bomb. See Georgii K. Zhukov, *The Memoirs of Marshal Zhukov*, tr. APN (New York, 1971), 674-75.

<sup>71</sup> The committee was composed of seven official members: Stimson served as chairman (in his absence his alternate was George L. Harrison); Ralph A. Bard, an undersecretary, represented the Navy Department; William L. Clayton, an assistant secretary, represented the State Department; Byrnes served as the president's personal representative; Bush, Conant, and Karl T. Compton, all scientist-administrators, completed the formal committee. In response to a suggestion by Conant the committee appointed a scientific panel composed of Arthur H. Compton, Enrico Fermi, Ernest O. Lawrence, and Oppenheimer.

<sup>72</sup> In addition to Truman's own description of his studied attempt to avoid any serious discussion with Stalin about the atomic bomb, see Charles E. Bohlen to Herbert Feis, Jan. 25, 1960, Herbert Feis Papers, box 14, LC.

<sup>73</sup> Truman, *Year of Decisions*, 421.

<sup>74</sup> Stimson and Bundy, *On Active Service*, 637; Bush to Conant, Nov. 7, 1945, Vannevar Bush Papers, box 27, LC.



for the neutralization of the new weapon. As a result of policies based on this assumption American diplomacy and prestige suffered grievously: an opportunity to gauge the Soviet Union's response during the war to the international control of atomic energy was missed, and an atomic-energy policy for dealing with the Soviet government after the war was ignored. Instead of promoting American postwar aims, wartime atomic-energy policies made them more difficult to achieve. As a group of scientists at the University of Chicago's atomic-energy laboratory presciently warned the government in June 1945: "It may be difficult to persuade the world that a nation which was capable of secretly preparing and suddenly releasing a weapon as indiscriminate as the [German] rocket bomb and a million times more destructive, is to be trusted in its proclaimed desire of having such weapons abolished by international agreement."<sup>75</sup> This reasoning, however, flowed from alternative assumptions formulated during the closing months of the war by scientists far removed from the wartime policy-making process. Hiroshima and Nagasaki, the culmination of that process, became the symbols of a new American barbarism, reinforcing charges, with dramatic circumstantial evidence, that the policies of the United States contributed to the origins of the cold war.<sup>76</sup>

<sup>75</sup> "The Franck Report," June 11, 1945, in Alice Kimball Smith, *A Peril and a Hope: The Atomic Scientists' Movement, 1945-1947* (Chicago, 1965), app. B.

<sup>76</sup> The charge was first made in the West by British physicist P. M. S. Blackett: "So we may conclude that the dropping of the atomic bomb was not so much the last military act of the second World War, as the first major operation of the cold diplomatic war with Russia now in progress." *Fear, War and the Bomb: Military and Political Consequences of Atomic Energy* (London, 1948), 139.

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## Franco Venturi on Russian Populism

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A Review Article by ALEXANDER GERSCHENKRON

*Veritatis cultor . . .*

FRANCO VENTURI. *Il populismo russo*. Volume 1, *Herzen, Bakunin, Černyševskij*; volume 2, *Dalla liberazione dei servi al nihilismo*; volume 3, *Dal'andata nel popolo al terrorismo*. (Piccola biblioteca Einaudi, 188-198) 2d ed.; Turin: Einaudi. 1972. Pp. cxii, 385; 479; 445.

FRANCO VENTURI. *Les intellectuels, le peuple et la révolution: Histoire du populisme russe au XIX<sup>e</sup> siècle*. Translated from the Italian by VIVIANA PAQUES. In two volumes. Paris: Gallimard. 1972. Pp. 667; 674-1166.

I BELIEVE that there are three main features by which a great book is recognizable: it must be interesting, memorable, and rereadable. These three criteria, which must be taken in conjunction—some books are rereadable only because they are promptly forgotten—apply most effectively to belles-lettres. But at times there are also remarkable scholarly books whose greatness can be judged by the same standards. Professor Franco Venturi's book on Russian populism is one of those rare specimens. The book first appeared two decades ago. At that time I read it carefully and reviewed it in the pages of this journal. Furthermore, stimulated by Venturi's study I elaborated the thoughts contained in my review in an essay.<sup>1</sup>

Thus I knew the book extremely well. Nevertheless when last year a second edition was published in Italy, followed immediately by a French translation of that edition, I reread the well-remembered, long pages with the same sense of intellectual excitement and esthetic pleasure I had felt during the first perusal. Indeed: interesting, memorable, and rereadable. But as is true in the case of any truly great book, there is infinitely more to Venturi's study than my perhaps not irrelevant but

<sup>1</sup> *AHR*, 59 (1953-54): 118-20; "The Problem of Economic Development in Russian Intellectual History," in E. J. Simmons, ed., *Continuity and Change in Russian and Soviet Thought* (Cambridge, Mass., 1955), 11-39, reprinted in Alexander Gerschenkron, *Economic Backwardness in Historical Perspective* (Cambridge, Mass., 1962), 152-87, together with an annex, "Realism and Utopia in Russian Economic Thought," 188-97.

rather superficial criteria. For in a very real sense the new edition is a new book, or rather it includes within the covers of volume 1 an Introduction (called *Présentation* in the French edition) that, with its more than one hundred pages, is to all intents and purposes an important book in itself. Moreover the new edition includes throughout the 1,300 pages of the text innumerable additions and corrections (*aggiunte e rettifiche*). But it is the Introduction that primarily justifies this attempt at a second appraisal.

The Introduction reveals with particular clarity the characteristics of the author. Franco Venturi is above all a truly indefatigable worker. His books appear regularly *a bruma ad brumam*, and *nullus annus sine libro* seems to be his device. The scope of his interests and the breadth and depth of his erudition are nothing if not amazing. In fact Russian intellectual history, to which he has made such a singular contribution, is but a side line of his efforts. He is first and foremost the historian of the Enlightenment. Before the appearance of his study on populism and in the years following it he published volumes and volumes on the French and Italian Enlightenment, culminating in the brilliant *Settecento riformatorio da Muratori a Beccaria* (1969).<sup>2</sup> This massive productivity did not prevent him from still continuing his researches on Russia, to which the engaging monograph *Il moto decabrista e i fratelli Poggi* (1956) bears witness. Along with his original work stands his immense editorial performance, in which nothing germane to the subject remains unread and unabsorbed. The magnificent critical edition of Cesare Beccaria (which appeared both in Italian and French) may serve as an example.<sup>3</sup> In the Russian field there is the splendid Italian edition of Alexander Radishchev's celebrated *Journey from Petersburg to Moscow* (1790), which just appeared in Italian.<sup>4</sup> Besides copious an-

<sup>2</sup> This volume of nearly 750 pages was adumbrated by numerous articles and is solidly based on the texts and the introductions contained in the huge volumes *Illuministi Italiani*, which Venturi published in the early sixties in the enormous *collana* called *La letteratura Italiana*.

<sup>3</sup> Cesare Beccaria, *Dei delitti e delle pene* [1764], ed. Franco Venturi (Turin, 1965). Venturi had previously published and introduced this text of Beccaria's along with other selections and letters from his pen in *Illuministi Italiani*, 3: 2-211. But the separate edition of Beccaria's classic is an entirely new work and a gem of editorial art. Preceded by a new introduction, the text is followed by nearly 600 pages of annotated letters and documents regarding the origins of Beccaria's book and the reactions to it in the following decade all over Europe from Spain to Russia.

<sup>4</sup> Alexandr N. Radiščev, *Viaggio da Pietroburgo a Mosca*, ed. Franco Venturi (Bari, 1972). In editing this book in the midst of putting the new edition of *Russian Populism* through Italian and French presses Venturi enjoyed the cooperation of his wife, Signora Gigliola Venturi, who has made a name for herself as a talented translator of Russian prose and poetry. It might be added that the same year, 1972, also saw the appearance of Venturi's George Macaulay Trevelyan Lectures, *Utopia e riforma nell' illuminismo*, as well as the publication of a large volume of essays on the *settecento* entitled *Europe des lumières, Recherches sur le 18<sup>e</sup> siècle*. As this piece goes to press, Venturi has again surprised and delighted his readers by his enormous contribution (500 pages) to volume 3 of *Storia d'Italia, Dal primo Settecento all'Unità* (Turin, 1973). Venturi's study is called "L'Italia fuori d'Italia." He reviews in it a perfectly staggering amount of the period's literature on Italy in English, French, German, and Russian.

notations the edition contains a long and illuminating introduction from Venturi's pen, based not only on the study of the whole existing literature—beyond which Venturi, as is his wont, pushes on to a scrutiny of primary materials, including the little Masonic reviews that were appearing in Russia in the last quarters of the eighteenth century—but also on his incomparable knowledge of the *settecento*, which makes it possible for him to treat the Western roots of, and the foreign influences upon, this “first Russian intellectual” with a sureness of touch unequaled in previous writings on Radishchev, many of which are marred by Soviet chauvinism and dogmatism. The long years of Venturi's editorship of the *Rivista Storica Italiana*, surely one of the most outstanding scholarly historical periodicals, have been characterized not merely by wealth of ideas and organizational ability but, as is always true of the man, by sheer labor; in a recent issue of the *Rivista* (1972, no. 2) no less than one-third of the bulky number represents either original writings of Venturi's or translations done by him with unwavering attention to every detail down to the last footnote.

The introduction to the new edition of *Populismo russo* contains a survey of the literature on the subject, which appeared since the completion of the first edition. In Soviet Russia after Stalin's death it became possible to return to historical problems, the treatment of which had been stifled or simply forbidden during the lifetime of the despot. In the course of the two last decades Western historians have manifested a keen interest in Russian populism. Some of it was stimulated by Venturi's book, which was extensively reviewed both in the East and the West. Nothing of this large body of literature seems to have escaped Venturi's attention, and he applies to it his ability to summarize in a few paragraphs the crucial aspects of complex writings.<sup>5</sup> The Introduction thus contains a review of reviews of his book, a task to which Venturi applies the combination of firmness and grace that he revealed in his celebrated debate with the Soviet historian, N. M. Druzhinin.<sup>6</sup>

My own review of Venturi's book is also treated in the pages of the Introduction. In that review I expressed my admiration for the Herculean

<sup>5</sup> His summary in the Introduction of the momentous and very difficult magnum opus of Michael Confino, *Les systèmes agraires et progrès agricole* (Mouton, 1969)—the review of which has cost me months of study—bears witness to this extraordinary talent. See *Populismo russo*, 1: xlviii–lii; pp. 52–55 of the French edition henceforth F.E.).

<sup>6</sup> The debate included the following exchange: 1) Venturi, “L'autobiografia di uno storico Sovietico,” *Rivista Storica Italiana*, 74 (1962): 146–52; 2) Druzhinin, “Lettera aperta allo storico Italiano Franco Venturi,” 3) Venturi, “Risposta all'Accademico N. M. Druzhinin,” *Rivista Storica Italiana*, 75 (1963): 846–62; 4) Venturi, “Chiusura di un dibattito,” *ibid.*, 76 (1964): 1070–71; and 5) “Risposta dell'Accademico N. M. Druzhinin,” *ibid.*, 1072–85. Of the preceding items the following were also published in Soviet Russia: item 2: Druzhinin, “Otkrytoe pis'mo ital'yanskomu istoriku Franko Venturi,” *Istoriya S.S.S.R.*, 7 (1963): no. 4, pp. 182–83; item 3: Venturi, “Pis'mo akademiku N. M. Druzhininu,” and item 5: Druzhinin, “Otvét Franko Venturi,” *Istoriya S.S.S.R.*, 8 (1964): no. 5, pp. 189–203.

labor that in over three years of research in Moscow, in the libraries devastated and mutilated by Stalinist vandalism,<sup>7</sup> produced a study that, as I wrote then, "in its breadth and comprehensiveness is without precedent in the literature of the subject in any language." This judgment still holds today. Despite everything that has been written in twenty years, the absence of a precedent is still matched by the absence of any even remotely comparable "postcedent." But while I was able to recognize the great merits of the book, I also felt that some broad interpretation of Venturi's subject matter was in order. If this was criticism of the book, it was at best an oblique one. In those days I was in the process of developing my general approach to the industrial development of Europe by trying to understand the peculiarities of that development in the individual countries in terms of the level of their economic backwardness. In studying Venturi's book I was struck by how much in it—implicitly, and yet irresistibly—led to the insight that comprehension of the phenomenon of Russian populism required relating it to the economic and political backwardness of the country. The book seemed to me to merge with my own thinking; in fact the very selection and arrangement of the material as well as the distribution of emphasis by Venturi suggested that, even though he preferred not to indulge in explicit generalizations, his ideas and his approach were very close to my own. It is only now, after having read the Introduction and reread the book, that I have come to understand that I was, if not altogether wrong, certainly far from being fully right in this belief and that there is something quite fundamental about Venturi's approach that I failed to grasp.

But before I approach this crucial problem a related counter-criticism of my views by Venturi should be dealt with. I believed, and still believe, that it was the economic backwardness of Russia, its abysmal inferiority in comparison with the West, that forced the Russian intelligentsia to concern itself continually with the future destinies of the country. As I said in my review, the populists, in dealing with the problem, "clearly saw the advantages inherent in Russia's being a late-comer upon the modern historical scene" and "the possibility of adopting the results of foreign experience without incurring the heavy cost of experimentation." But they did so only in order to abandon the argument by an almost imperceptible twist and to raise the paradoxical claim that the preservation of the *old*—of the field commune (*obshchina*) and the workers' cooperatives (*artel'*)—rather than the easy adoption of the *new* constituted the "advantages of backwardness." I regarded this course as

<sup>7</sup> It is only now that Venturi describes in the Introduction the grim physical conditions of research work in Moscow between 1947 and 1950. The difficulties of the intellectual readjustment cannot be overestimated. He went to Moscow to a new environment and an entirely new subject after publishing the first Italian edition of his book on the origins of the *Encyclopédie*, a second, improved and somewhat enriched, edition of which appeared in 1963. *Le origini dell'Enciclopedia* (Turin).

a "tragic surrender of realism to utopia" and saw therein one of the main reasons for the decline of populism in the last decades of the century in its contest with the specific Russian brand of Marxism. Speaking of realism and utopia in this connection, of course, must not be conceived in terms of the dogmatic dichotomy between the so-called utopian socialism and the self-styled Marxian scientific socialism.<sup>8</sup> The heart of the problem was an eminently empirical one. For the denial of the possibility of successful industrialization in Russia at a time when industrial development began to proceed at a very rapid rate was too conspicuously at variance with the facts.

I argued furthermore that it was also the political backwardness of the country which, along with economic backwardness, shaped the ideas and the actions of the populists. The existence of autocracy was bound to radicalize minds whose standards of political normalcy, whatever their rootedness in the native soil, invariably came from the West. Radicalism was the natural answer of an intelligentsia that was barred by absolutism from normal professional pursuits and confined to the area of pure thought, within which, despite the rigors of censorship, it was able to move with a degree of freedom that appears quite unbelievable by the standards of a modern totalitarian dictatorship. Hence populist thought wavered uneasily back and forth between radical anarchism and the equally radical apotheosis of an omnipotent Jacobin state. And when it came to action, any method of struggle seemed justified—from forged imperial manifestoes to murderous conspiracies against a government whose troops fired upon the peasants, flogged whole villages, and had jails, exile to forced labor in Siberia, and gallows in store for those who opposed it. Hence the complex and striking features of populists in whose minds and souls love for the people, devotion to the cause, heroism, and the spirit of sacrifice could be conjoined in some cases with the shattering dictum of a Dostoevski hero: *Vsyo pozvoleno* (Everything is permitted). And hence the unrelenting man hunt against the emperor, which ended with his assassination (March 1, 1881, Julian calendar) and the death on the gallows of the populist terrorists—the high drama with which Venturi still concludes his book.<sup>9</sup>

<sup>8</sup> It should be noted that, when in the late 1870s Russian socialists spoke of "scientific socialism," the reference was not at all specifically to Marxism but to the "entire Western corpus of socialist doctrine." See Abraham Asher, *Pavel Axelrod and the Development of Menshevism* (Cambridge, Mass., 1972), 48. Furthermore, it is a curiosity of Russian intellectual history that in that contest between the Marxists and the populists the latter, too, used Marx's analytical tenets just as much as the so-called Marxists and in fact in some respects were closer to Marxian ideas, as for instance in the insistence on the importance of a foreign market for capitalist development. Lenin vehemently denied the point with reference to Marxian schemas in volume 2 of *Das Kapital*, with the result that when, during World War I, he formulated in a popular pamphlet his views of imperialism, he had to crib his ideas from Hobson and Hilferding rather than from Marx and to spread a veil of silence over his previous theoretical position.

<sup>9</sup> There is little doubt that the history of Soviet dictatorship and the massive crimes committed by Stalin and his henchmen make it a good deal more difficult to see the horrors of Rus-

In his Introduction Venturi summarizes these views of mine with his usual skill and adds: "After having read those pages, the problem of populism is no longer what it used to be either for me or, I believe, for anyone else. Gerschenkron's interpretations remained a firm point in the discussions on the subject in the last fifteen years." This graceful acknowledgment must not be taken, however, to connote his readiness to accept my point of view without reservations. Rather he uses it as an occasion to formulate a general position regarding the very function in the historical process of ideas and political ideals. And he goes on to couch some propositions in the characteristically modest guise of questions:

An idea that appears to look backward in time, remolds itself on the past, seems to prefer what has been, and to eschew what will be—does really such an idea, whose function is destined to be negative, constitute a utopian retarding factor in economic and social development? Or does it not rather, at least at times, represent an act of *reculer pour mieux sauter*, that is to say, a fruitful attempt to preserve the most precious aspects of the past in order to transmit them to the future? History is not made just by looking forward but, I should say, by looking both forward and backward. Socialism is the idea of community and equality regarding material goods and of an economy based on solidarity. Is not socialism [so conceived] a legacy of the past that has been preserved by being transformed into an ideal for the future? And is it not true that socialism and communism, and not only populism, are ideas that in their origins were deliberately opposed to economic development?

In support of the oxymorons of these "assertive questions" Venturi quotes the title page of the "old and fundamental" book by Buonarrotti on the Babeuf conspiracy, a book through which, as he says, the communistic ideas of the eighteenth century were transmitted to the following century; and he regards the very motto of the book as "the most explicit declaration possible against economic development." The populists' idea of the field commune, their revolutionary will, and agrarian equality were things of the same order. According to Venturi these resemblances constitute "another reason for which populism should be seen as a page in the history of European socialism."<sup>10</sup>

sian autocracy in the same light in which they were seen by the populist—and not only populist—intelligentsia in nineteenth-century Russia. In those days even the young Miliukov, who was to become a great historian and the leader of the Liberal party (*Kadety*), did not suppress a positive reaction to the regicide on the very day of the event. See the memoirs of P. N. Miliukov, *Vospominaniya* (New York, 1955), 1: 101. What if not the backwardness of the country was responsible for such attitudes, which so often were quite out of character for the individuals? A strong mental effort is necessary in order to place oneself within the climate of the time and to reject the obtrusive but fallacious thought that a government that did *not* exterminate millions and millions of its subjects was after all not so bad. One still cannot read Venturi's terse, accurate, and beautifully written account of the drama without high excitement, and still my feelings are no longer those with which I first learned the facts of the matter more than half a century ago. The knowledge that much more ruthless and vicious tyrants escaped the deserved vengeance intrudes perhaps irrelevantly but irresistibly and affects the sentiments of the reader.

<sup>10</sup> *Populismo russo*, 1: xxi-xxiii (F. E., 1: 27-29).

This spirited defense of populism goes far beyond the criticism of my approach. It is closely connected with Venturi's fundamental position as an intellectual historian, and more should be said about that later. At this point one may merely wonder whether Venturi's generalization does not go too far. In this connection I had better not mention Saint-Simon—the "utopian socialist"—who probably contributed more to the doctrines and the practice of economic development than any other single figure in the course of the nineteenth century. For Venturi in all likelihood would be quick to retort by casting doubt on both the adjective and the noun in the usual attributes applied to Saint-Simon. More pertinent would seem the fact that Venturi's emphasis appears to read out of socialism the basic acceptance of technological progress and economic development by Marx and the Marx-inspired socialist labor movements of the nineteenth century.<sup>11</sup> It is not difficult for me to accept Venturi's further somewhat implicitly stated opinion that, if the reconciliation between Russian intelligentsia and industrial development was carried out by Russian Marxism, the fact that such reconciliation could only occur under the auspices of a socialist ideology was, among other things, also the result of the strong intellectual tradition preestablished by at least a generation of populist thought.

On the other hand, I am more dubious when faced by Venturi's concluding—still interrogative—criticism of my views:

As far as concerns the internal history of populism in advancing gradually into the sixties and the seventies of the century, is it really possible to observe, as Gerschekron would, a step backward, an outright reversal, an occlusion of the pre-

<sup>11</sup> I am not referring in this context to the history of Soviet Russia where Bolshevism has turned out in its practice to be the most enraged "economic developer" on historical record. For I interpret the policies of superindustrialization in the Soviet Union as flowing not from the theory or ideology of Marxism but from the needs of the dictatorship and the mechanics involved in the exercise of dictatorial power. With regard to the latter something might be said about the curious absorption by Bolshevism of some populist ideas. But that absorption was a highly selective one, and the most attractive traits of populism were certainly jettisoned in the process, so that not much could be made with regard to the problem at issue from a tenuous ideological connection that at best refers to the struggle for power rather than to the use made thereof in the postrevolutionary policies. Nevertheless, when everything is said and done, Venturi does have a point. He might have referred in corroboration of his view, among other things, to John Stuart Mill's extraordinarily friendly attitude toward the stationary state, which Mill's predecessors had regarded perhaps as inescapable but as very deplorable. There is little doubt that Mill's position was greatly influenced by the climate of contemporaneous socialist thought. On the other hand, however, it was from the same thought that there derived the assertion, repeated time and again in antisocialist literature, that under socialism the rate of investment in national income was bound to be fatally reduced. Much effort was spent by socialist writers in trying to controvert this assertion. Thus the problem is indeed a very complex one. It took generations of technological progress and enormous increases in standards of living in the industrialized countries for some small but vocal factions of the socialist thought of our days to return to the negative views on economic development, with the result that many a college student nowadays finds great fascination in reading about "techneclastic" episodes in the history of the labor movement, as they are detailed, for instance, by E. P. Thompson in his *The Making of the English Working Class* (London, 1965). But a "retropolation" from the specific conditions of the present to the whole body of socialist thought would be a rather unhistorical operation, and Venturi, I am sure, would be the first to scorn it.



viously possessed understanding of the advantages of backwardness, a relapse into a pure and simple worshipping of the past? Is *Narodnaya Volya* really in this respect a retrogression compared to Herzen's thought? Were not the *narodovol'tsy* the most conscious and most lucid judges of the Russian economic structure and of the fundamental role that the state and its measures always had played in the development of that structure? The debates of those years seem to testify to progress and not to retrogression. All these are things that Gerschenkron knows better than anyone else, but it should not be entirely useless to recall them in discussing with him the interpretation of Russian populism.<sup>12</sup>

Being acutely conscious of the limitations upon, and the manifold lacunae in, my knowledge of the subject, I cannot accept Venturi's complimentary remark. At the same time, however, I am also unable to accept his criticism as wholeheartedly as he would wish me to do. What certainly is correct is the fact that the populists toward the end of the debates of the seventies, quite apart from their contemporaneous opponents, understood the nature of the Russian state much more penetratingly than those Soviet scholars who still produce and reproduce in monotonous reiteration the elements of the basic Marxian theory of state. By contrast the populists said clearly that while "in Europe the state was a creation of the bourgeoisie, with us on the contrary bourgeoisie is being created by the state."<sup>13</sup> It is also true that the populists realized that they were witnessing "an extraordinarily important historical process—the process of the formation of the *tiers état*";<sup>14</sup> they refused to accept the inevitability of the process and to acquiesce in the impossibility of mitigating the negative aspects of the coming order, of changing it, or of limiting the duration of its dominance. For the state is not something "inert, impotent, and deprived of will."<sup>15</sup> This hope in the salutary intervention of the state was conjoined with the rather unelaborated, almost offhand assertion that industrial progress was a "necessity" for the state because of the growth of population and the pressure of international relations, which presumably meant the military needs.<sup>16</sup> It is also true that some changes in views took place over a very short time in the late seventies. In 1878 it was still asserted that in the West "history had placed the factory question in the center," while in Russia "that question was not advanced at all and was replaced by the agrarian question." Two or three years later this belief was somewhat weakened by the recognition that possibly not the villages but the cities, "because of higher development and greater mobility of city dwellers," may be "the first to raise the signal of rebellion."<sup>17</sup> All this can be readily admitted. And

<sup>12</sup> *Populismo russo*, 1: xxiv–xxv (F. E., 1: 30).

<sup>13</sup> See *Narodnicheskaya ekonomicheskaya literatura* (Moscow, 1958), 379; see also 377.

<sup>14</sup> *Ibid.*, 355.

<sup>15</sup> *Ibid.*, 356.

<sup>16</sup> *Ibid.*

<sup>17</sup> *Ibid.*, 323, 395.

yet as one rereads the relevant writings of the period, including the programs of the revolutionary parties then formed, certain things appear to stand out. They speak indeed of the revolution that will transfer all power to a constituent assembly in which the revolutionary party will stand for a democratically elected parliament, a complete bill of rights, land ownership by the people, and transfer of mills and factories to the workers. Thus the state is indeed accepted as the force to carry out the great social transformation, and the former anarchistic tendencies appear reduced to the well-tempered stress on decentralization and the autonomy of the communes of peasants and workers. But apart from the rather noncommittal reference to industrial progress just mentioned and apart from very occasional references to the benefits of introduction of labor-saving machinery, including technological progress on peasant farms, I am unable to find in this literature any reference to the chances of rapid increases of production and productivity—chances inherent precisely in the “advantages of backwardness” that earlier were understood and well-expressed, although immediately cast aside, both by Herzen and Chernyshevskii. Nor do I find any traces of real interest in, and understanding for, economic development, or any expectation that the state, be it the prerevolutionary or the postrevolutionary state, would or should promote economic development. The main concern of the state should be with the vast constitutional reform and with the creation of the collective structure of the economy. The interest in increasing output is hardly visible. Thus Venturi’s often-repeated and correct stress on the populists’ comprehension of the nature and importance of state power is rather irrelevant in the context of my approach to the problem of populism. In fact his reproach is not quite consistent with his previously stated belief in the basic antagonism of socialism to economic development. At any rate, on an empirical rather than ideological level the fatal underestimation by populist writers of the actual possibility and the promise of economic development in Russia of the eighties and nineties still remains an indubitable historical fact.

But I have come to realize that there is much more to the problem. For it seems to me now that any assessment of populism in the light of confrontation of populist thought with Russian economic and social history, however correct it may be in itself, ignores the impulses that caused Venturi to compose what he once called his two *troppo grossi volumi* as well as his basic purpose. He had set out to write a history of a great movement that excited both his admiration<sup>18</sup> and his intellectual curiosity by describing it such as it “really had been,” that is to say, by cleans-

<sup>18</sup> An admiration that did not make him close his eyes to the “absurdities” to which “some emotionally and intellectually less stable representatives of the movement were carried by the spirit of intransigence,” which Venturi sees as a distinct general characteristic of Russian history. *Populismo russo*, 1: xv (F. E., 1: 22).

ing it of the thick ideological crust and the manifold distortions under which it had been buried by much of the Soviet literature on the subject. It is only now in the Introduction to the new edition that the author's motivations and purpose are stated explicitly, and things become clear that could not be readily perceived by the reader of the first edition. This means that Venturi's discussion in the Introduction of the relevant Soviet writings, both old and new, must be seen as a crucial guide to the understanding of the book.

IN HIS DEBATE with Venturi, Druzhinin tried to lecture Venturi on the importance for the historian of having a "general conception of the world . . . a coherent view of the evolution of mankind." Without such a conception, Druzhinin argued, the historian can at best produce a talented narrative, and his work, reduced to a "naked description," loses all "cognitive value." Replying to the charge with dignity and courtesy, Venturi referred to the enormous costs of the adherence in Russia to a materialistic conception of history, conjoined, as it was, with Soviet nationalism and with the enforced neglect and ignorance of the developments outside the boundaries of the Soviet Union by the Soviet orthodoxy.<sup>19</sup>

As one rereads that debate and is illumined by the Introduction one realizes that nothing is farther from Venturi's mind than taking a general position on the role of generalization in history. Venturi is too considerable a historian not to know that according to Goethe's profound word "everything factual is already a theory."<sup>20</sup> For to the extent that a problem of "facts versus theory" exists to Venturi, it is altogether specific to the present case. The two *grossi* (but by no means *troppo grossi*) *volumi* indeed contain a rejection of theory, but only in a very specific sense. The work reflects an aversion, if not revulsion, from what the Soviet version of dogmatic Marxism, guided by Stalin's illiterate literary ambitions and his orders, did to the treatment of a long, fascinating period of Russian intellectual and social history. During the years 1956-64 there were episodes or moments in Russia when it was as though scales had fallen from the eyes of Soviet scholars, and clear and sharp language was used in castigating the sins against historical scholarship that had been committed. At a conference, arranged by the Soviet Academy of Sciences in January 1964, F. B. Konstantinov, an Academician, expressed himself as follows on what the "cult of Stalin" had meant for "the science of history":

In this connection it is proper to speak of the main disabilities of certain historical works, which during the period of the cult of personality were written by Stalin himself and were inspired by him.

<sup>19</sup> See Venturi, "Risposta all'Accademico N. M. Druzhinin," 851, 861.

<sup>20</sup> Goethe, *Maximen und Reflexionen*, *Schriften zur Naturwissenschaft*, pt. 1 of *Sämtliche Werke*, Jubilaeumsausgabe, 39: 72.

Above all it was subjectivism—an arbitrary attitude to, and interpretation of, facts; the absence of historical veracity. If one is to talk about the demolition of confidence in our historical researches, the cause thereof lies in moving away from historical truth, in the subjective attitude to historical facts, in the anti-historical approach to phenomena. But what matters is not merely those researches in themselves, but the principles that were imposed upon scholars.

Konstantinov continued immediately with a concrete illustration of his indictment, an illustration that bears directly on the subject of Venturi's book. He said:

Let us take *the facts known to all* regarding the attitude to the heroic populists of the seventies. Let us recall how they were appraised by Marx, for instance in [the second Russian edition of] the preface to *The Communist Manifesto*, and by Lenin, both of whom very deservedly assigned to the populists a great role in the history of the revolutionary movement in Russia. And let us compare the evaluation of their role and participation in that movement given in [Stalin's] *Short Course*. It was such a misdirection of minds, such a misleading of views and appraisals, such a perverse interpretation of history that it defies imagination. That was an open revision of Marx's and Lenin's appraisals.<sup>21</sup>

"The facts known to all!" If Konstantinov could assert general knowledge of "the facts," the reason undoubtedly was that he spoke more than a decade after the appearance of Venturi's book, which in the interval had been so extensively reviewed in Russia. Even so, his reference to "all" would have been somewhat more convincing if Venturi's book had been translated into Russian—something that neither the favorable reviews nor Khrushchev's thaw had been able to induce.<sup>22</sup>

Konstantinov would have done well to refer to Venturi. Without such a reference it is quite unclear in what way the general knowledge of facts could have emerged from the perversions and prevarications that he described so angrily and so justly. Even so, the foregoing quotation described well the intellectual desert that Venturi found in Russia in those last

<sup>21</sup> Academy of Sciences of the USSR, *Istoriya i sotsiologiya* (History and Sociology) (Moscow, 1964), 91–92. My italics.

<sup>22</sup> It is worth quoting; however, what the previously mentioned Druzhinin said about the impact of Venturi's book in Soviet Russia. Speaking at a meeting of Soviet and Italian historians that took place in Moscow in October 1964, Druzhinin expressed himself as follows: "I should like also to pause here to say something about the significance of the works of our colleague Venturi. . . . Naturally, we are not fully in agreement with Venturi regarding the evaluation of populism and the characteristics of its origins. We start from different methodological principles and scrutinize differently the individual theories of the sixties and the seventies. But this cannot in the least obscure to us the great virtues of Professor Venturi's work on populism. This book had a fate of its own. Perhaps Professor Venturi does not know what I should like to stress: in the period when the cult of personality was being overcome, when the interest in the history of populism revived with us, when young people began studying that phenomenon, Venturi's book attracted enormous attention. For it contained a plethora of facts and was most interesting in its way of posing problems [*problematika*] in comparison with those antiquated and general surveys of the history of populism that had been published before the revolution and in the beginning of the Soviet period. This is indeed a broad canvas that had great historiographic importance." *Problemy sovetsko-ital'yanskoy istoriografii* (Problems of Italo-Soviet Historiography) (Moscow, 1966), 338.

years of the fifth decade of this century. It was that situation which aroused Venturi, the historian. The remedy for him was not to go back to the incontrovertible judgments of Marx and Lenin, the medicine suggested by Konstantinov, which reveals how deeply ingrained is the search for authorities even in a mind that has struggled valiantly to liberate itself from the authoritarian incubus. Venturi knows, of course, well what Marx said about the populists, and, the perfect editor that he is, he cannot forbear reproaching the Soviet editors for publishing Marx's correspondence with the Russian revolutionaries in Russian translation, only without the German, French, and English originals, as well as for publishing only one of the drafts of Marx's famous letter to Vera Zasulich.<sup>23</sup> And as far as Lenin was concerned, Venturi takes some pride in the fact that in the first edition of *Populismo russo* Lenin's name appears just once—in expressing the author's gratitude for assistance to the Lenin Library in Moscow. For the first thing to do in order to understand the populists, Venturi says, was "to leave Lenin alone." In fact Lenin's judgment of the populists is a theme that Venturi regards as a worthwhile but special subject. As the Introduction shows, Venturi is much more interested in the influence of populist thought upon Lenin.<sup>24</sup> Even so, for Venturi an essay in Marxology or Leninology could never be the answer to the problem. Rather the task was an immense cleansing job. What was needed was to study the record and to present it in detailed narration, in a display of texts and documents, as completely and as faithfully as possible and equipped with the full apparatus of references, and in the process to reveal the propelling force of ideas and the continuity of intellectual history as it impinged upon the Russian revolutionary movement, which for being "Russian socialism" was no less an integral part of European socialism. The task then was, in Venturi's words, "to oppose facts to theories, method to metaphysics, research to faith, the individual to the scheme,"<sup>25</sup> and, as Venturi may well have added, to oppose impartiality to bias, truth to lies, and, last but not least,

<sup>23</sup> *Populismo russo*, 1: xli (F. E., 1: 45).

<sup>24</sup> *Ibid.*, 1: ix (F. E., 1: 17). In Soviet Russia a serious discussion of the problem was impossible for decades beyond statements regarding Lenin's admiration for Chernyshevskii. Even in this respect it was left to an emigré writer to record Lenin's admission in a private conversation of the crucial nature of Chernyshevskii's impact upon young Lenin: he had "plowed me deeply all over [*vsego perepakhal*]" was Lenin's reference to Chernyshevskii. See Nikolay Valentinov, *Vstrechy s Leninyim* (Encounters with Lenin) (New York, 1953), 103. The phrase appears correctly but colorlessly translated in Nikolay Valentinov (N. V. Volsky) *Encounters with Lenin*, tr. Paul Rosta and Brian Pearce (London, 1968), 64. It is only now that it is beginning to be realized in the Soviet Union that there was much more to Lenin's positive relation to the populists than the influence of Chernyshevskii, and a Soviet historian is able to trace Lenin's very gradual moving away from Plekhanov's traditional "dogmatic rejection of the populist perspective" and to emphasize the importance of the populist programs for the formation of Lenin's basic views. See M. Ya. Gefter, "Stranitsa iz istorii marksizma nachala XX veka" (A Page from the History of Marxism at the Beginning of the 20th Century), in *Istoricheskaya nauka i nekotorye problemy sovremennoosti* (The Historical Science and Some Problems of Our Time) (Moscow, 1969), 24-25, 34.

<sup>25</sup> Venturi, "Risposta all'Academico N. M. Druzhinin," 860.

objectivity to *partiynost'* (i.e., loyalty to the party as the all-dominating and all-overriding principle in scholarship and art), which was and still is Lenin's unholy legacy and which has caused so much perversion of both scholarship and art.<sup>26</sup> In these objectives and rejections lie the intellectual roots of Venturi's book, and they are indispensable for comprehending his purpose and for appraising and appreciating the result. In a true study of Russian populism empiricism was indeed what was called for. But the result stands high above crude or simple empiricism. For one thing, in scratching away the thick layers of dirt and revealing the huge canvas that lies hidden beneath them in all its multicolored glory, Venturi is intensely conscious of the unity of populism, that is to say, of the historical continuity that pervades and informs the whole painting. This relates, among other things, to the very concept of populism. Some years ago Professor Richard Pipes stressed the fact that the concepts of populism and populists entered the vocabulary only in the seventies in connection with the practical action of "going to the people." In the course of this movement the field commune and the workers' cooperative (the *obshchina* and the *artel'*) were encountered "unbookishly" in the real life of the peasants in the villages. It was only later in the following great debates that in Marxian writings the terms lost their specificity and were broadened far beyond their semantic origin.<sup>27</sup> Venturi does not deny—in fact he praises—the technical correctness of Pipes's findings. But when Pipes, drawing a large inference from those findings, claims that the widespread broad connotation of the term "populism" "has no historical justification," Venturi demurs, because the inference neglects the basic historical significance of the continuity in the history of ideas—the fact, that is, of the strong influence of the pre-

<sup>26</sup> As late as 1963 I. I. Mints, an Academician, still claimed in a scholarly discussion that "the principle of party loyalty [*partiynost'*] of scholarship" is the most important "principle of methodology of history." See Academy of Sciences of the USSR, *Istoriya i sotsiologiya*, 66. And at the same meeting the author of the main paper, P. N. Fedoseev, also an Academician, did not hesitate to express himself as follows: "What is the materialistic conception of history? According to our view, this is a party-loyal [*partiynoe*] conception of historical processes from the position of the working class, [that is] of the Marxist-Leninist party, and in our own conditions from the positions of the whole Soviet people, because the views of the party are shared by all Soviet men and women, by the whole Soviet people." *Ibid.*, 328. In the light of such a statement it is not surprising that the same author in his paper on "Methodological Problems of History" (written together with Yu. P. Fratsev, also an Academician) was able to say: "In determining the subject matter of historical science the guiding role belongs to the propositions of the Program of the Communist Party of the Soviet Union." In *ibid.*, 11. Just imagine Marc Bloch searching a political party program for authoritative guidance in his discussion of what *le métier d'historien* is or should be. And it is in the same light that one must read the statement by the same authors: "Nowadays, a Marxist historian departs from the propositions of historical materialism [regarding them] not as hypotheses, but as proven scientific propositions." *Ibid.*, 24. All this was being said at a meeting of scholars that forgathered in Moscow a decade after the death of Stalin and at which many a bitter word was spoken about what "the cult of personality" had done to the historical scholarship and the teaching of history in Soviet Russia. It is not difficult to imagine, proceeding *a minori ad maius*, what the situation was that Venturi found in Moscow in the years 1947–50.

<sup>27</sup> Richard Pipes, "Narodnichestvo [Populism]: A Semantic Inquiry," *Slavic Review*, 23 (1964): 441–58.

ceding intellectual development as contained in the writings of Herzen and Chernyshevskii in the fifties and the sixties upon the minds of those who "went to the people" in the seventies.<sup>28</sup> Venturi can take this position and thereby vindicate his own broad use of the concepts because from his painstaking empirical studies he has been able to draw an important generalization regarding the interdependence of thought and action in the development of what he so rightly subsumes under the general name of populism.<sup>29</sup>

It is certainly correct to say that Venturi is primarily interested in the history of ideas. But it would be quite wrong to say that he lacks interest in economic and social history. His preoccupation in the Introduction with the researches in agrarian economic history by Michael Confino testifies to his concerns in this direction. And so does his praise for the recent work (a series of books) of an able Soviet historian, P. A. Zayonchkovskii, who *inter alia* subjected the traditionally accepted statistics of peasant unrest to a searching and destructive criticism and in this connection also threw new light on the equally traditionally accepted thesis regarding the alleged "revolutionary situation" in Russia between 1859 and 1861; it was the same author who, destroying the received opinions on the "second revolutionary situation" in the late seventies, has also been able to demonstrate the extraordinary efficacy of the revolutionary populists and their impact upon the autocratic government.<sup>30</sup> In the same context it ought to be said that Venturi does not see the Soviet perversions of the history of populism as flowing exclusively from ideological dogmatism. He knows well that the exigencies of dictatorial politics—Stalin's fear of bombs and pistols—were at least as much responsible, and he mentions both Stalin's and Zhdanov's admission that concern with the populists might encourage terroristic activities against the Soviet government as well as difficulties in dealing with populism after what had been done in the collectivization to *narod*, to the people.<sup>31</sup>

Thus there is enormously more than empiricism to Venturi's study of Russian populism. The thought of the web of history is at all times strong upon him. It is not for nothing that he speaks of Stalin's violent cut through that web, and he always remains conscious that the object of his study was to lay bare *the roots of revolution*—the very appropriate title that his book received in the English translation.<sup>32</sup> It is in-

<sup>28</sup> *Populismo russo*, 1: xlv (F.E., 1: 49).

<sup>29</sup> One way of illustrating the point is to compare Nezhdanov, Turgenev's populist hero of the seventies, in his novel *Virgin Soil*, who is ready "to go to the people" and who first appears carrying books borrowed from the library, to Tolstoy's Pierre Bezukhov in *War and Peace*, who for a short time in 1812 becomes a disciple of the peasant Karataev and as such indeed a populist *ante litteram*, or rather *sine litteris*—something that surely cannot be said of the populists of the seventies.

<sup>30</sup> *Populismo russo*, 1: xlviii–lii, lxvii, xci (F.E., 1: 52–55, 70, 94).

<sup>31</sup> *Ibid.*, 1: xi (F.E., 1: 19).

<sup>32</sup> *Ibid.*

deed impossible to brush aside as a mere empiricist an author who in a dozen lines is able to present a compressed and profound explanation of the intellectual anatomy of Russian nihilism of the sixties.<sup>33</sup>

In dealing with the Soviet literature on populism Venturi is eminently fair. He lists the cases of serious historical scholarship and of debates among Soviet historians before the great Stalinist freeze. He welcomes with joy any sign of revival of historical scholarship after 1956 and is generous in his praise of what he calls a whole library on the subject that has been published within the last fifteen years, including particularly the large thirty-volume edition of Herzen's collected works as well as other significant reprints. But Venturi also sees clearly the other side. To his mind the deaths of Nicholas I and Stalin, the disappearance of the two despots who were the authors of tyranny and repression, are historical events that, if not comparable, are still worth comparison. It is a matter of deep regret to Venturi that the death of Stalin has not led to a clear "break, to a profound renewal" but merely to a "thaw" in the course of which much "old dirt and mud has been carried along" with the slowly melting surfaces.<sup>34</sup> As Venturi says:

The rediscovered men and facts press against the preestablished schemes, deforming and distorting them, so much that one would expect to see those schemes demolished at length. But the expectation may not be fulfilled. Is the monotonous repetition of ancient formulas in the prefaces and the conclusions of new research no more than an ideological ritual without substance?<sup>35</sup>

The ingrained habit of what in Soviet Russia is called *tsitatichnestvo* ("quotationism" in a literal translation) seems ineradicable.<sup>36</sup> The object of the "cult" has been replaced, and the works of Lenin perform the services previously performed by the works of Stalin. In one case even the ever-calm Venturi loses his equanimity. When a Soviet historian, N. N. Novikova, has meaningless recourse to a quotation from Lenin, Venturi breaks out: "And to think that she has done notable research even in private archives (which is a rare case in Soviet Russia)! But nothing stands, everything collapses before a general phrase, a journalistic mention, a simple, occasional illustration penned by Lenin."<sup>37</sup> And Venturi speaks of a "veritable obsession with Lenin."<sup>38</sup> The phrase could not be more precisely chosen. Serious scholars still do not think it beneath their dignity to support results of their thought and effort by quoting a

<sup>33</sup> *Ibid.*, 1: lxxvi (F.E., 1: 79).

<sup>34</sup> *Ibid.*, 1: xxvi (F.E., 1: 32).

<sup>35</sup> *Ibid.*, 1: lxxi (F.E., 1: 74).

<sup>36</sup> Also the term "citatomania" is used. At the previously mentioned meeting the editor in chief of the main Soviet historical journal, *Voprosy istorii*, spoke sarcastically of the "faith in the infinite power of the quotation." Academy of Sciences of the USSR, *Istoriya i sotsiologiya*, 1955.

<sup>37</sup> *Ma nulla vale e tutto crolla* . . . begins Venturi's outburst. *Populismo russo*, 1: lxx (F.E., 1: 73).

<sup>38</sup> *Una vera e propria ossessione leniniana. Ibid.*



dependent clause from Lenin's articles or speeches on historical subjects on which Lenin is known never to have performed any research.<sup>39</sup> On the other hand, the Soviet scholar is still to be discovered who would find anything, however trifling, to disagree with, however mildly, in the forty-five volumes (not counting the letters) and some 25,000 pages of the last edition of Lenin's complete works.<sup>40</sup> As long as this habit of adoration, whose roots reach back beyond the Stalinist epoch into the centuries of the history of the Byzantine Church, is not broken, one cannot expect to receive from Russia any historical work that in its scholarly purpose, objectivity, and the sense of the historian's duty and dignity would equal Venturi's study on Russian populism.

Venturi is not without optimism in this respect. He notes, as did some of the reviewers of his book (Sir Isaiah Berlin among them), that his book has found favorable reception both in the West and the East, and this, he says, precisely in the years of cold war. Has history then the power to override ideological and political conflicts? Venturi asks the question and says:

Despite everything the author of this book was and is convinced that an affirmative reply to this question was to be given twenty years ago and is to be given now. To be sure, the limits to Clio's capacity to clarify are obvious, but this does not mean that her labors are ineffective. Without being the remedy for all the evils, the historian's work still has served and will serve to arouse energies, to recreate the will of truth that can transcend any ideological and political barriers.<sup>41</sup>

One cannot read without being moved this article of faith of a great historian, for whom service of Clio means above all service of Truth, means being *veritatis cultor, fraudis inimicus*. And yet there is enough said in the Introduction to temper Venturi's optimism with regard to the future of the very subject of his book. There is too much in populism, as there is in the general history of socialism, that must go against the grain of the dictatorial system. And that is above all the problem of lib-

<sup>39</sup> For some striking, fairly recent examples, see Alexander Gerschenkron, "Soviet Marxism and Absolutism," *Slavic Review*, 30 (1971): 859-60.

<sup>40</sup> A Soviet historian has elaborated his view of the proper contents of Soviet works about Lenin: "If a scholarly study is devoted to the subject: 'Lenin about this or that historical phenomenon,' then the author obviously must embark upon research of the following questions: 'Why at a given time and in what connection Lenin turned to this phenomenon; what events in current social life had induced him to do so; what was the extent of literature and sources of the subject that was known to him; against whom did Lenin argue and why; what wrong propositions did he refute; what was the new that Lenin said compared to his predecessors as well as compared with his previous works; what was at the time the political significance of Lenin's pronouncements; what was—and what still is—the importance of these thoughts of Lenin's for the further development of our science and social practice' and so forth." Academy of Sciences of the USSR, *Istoriya i sotsiologiya*, 292. All this would be much better than the usual bouquets of quotations. In particular, the suggestion that Lenin's knowledge of sources may have been inadequate is certainly unusual, but one must still note the complete absence of any intimation that Lenin's views might have been all wrong and be subject to criticism.

<sup>41</sup> *Populismo russo*, 1: xxxiii-xxxiv (F.E., 1: 38-39).

erty. It is a great paradox—or perhaps a pseudodox—of Soviet intellectual history that Herzen has been traditionally accepted as “the great revolutionary democrat,” as goes the official designation repeated millions of times. It is indeed possible to honor Herzen by publishing the previously mentioned great edition of his works. But what is extremely difficult, if not impossible, is to study and to interpret Herzen’s work as it really was. In Venturi’s words in the Introduction:

The man of genius, Alexandr Ivanovich Herzen, organically resists any attempt to be forced into an academic party-dictated classification. Whosoever opens [the volumes of] the new accurate edition of his complete works cannot help but finding pallid and inane the explanations, so often repeated in Soviet literature about Herzen’s “liberal illusions.” Herzen is the man least suitable to become an icon or a portrait [in the gallery] of “socialist realism.” He raises continually, on every page, the problem of the relation between liberty and revolution.<sup>42</sup>

Effective and truthful study of Herzen as the great apostle of liberty requires, therefore, if not the solution of that problem, at least the liberation of the historians from the trammels of enforced schematisms and from the need to protect themselves by running fearfully behind the shelter of the irrelevant pronouncements of an allegedly omniscient demigod.

And there is still another and connected problem. In the concluding passage of his Introduction Venturi says that in Soviet Russia “now the sights are often directed to the experience of revolutionary populism because it is seen as a democratic experience, revealing the relationship between the masses of the people and the intelligentsia.” And he continues:

The inescapable problem, the point toward which this renewed interest leads is always the same: The confrontation between history and Marxism. . . . Personally, I am convinced that there is only one way out of this Marxian bind [*strettoia*]: to understand that two centuries of socialist thought and movement in the whole of Europe are something much too variegated and much too rich to be monopolized by just one current thereof . . . and that any attempt to establish within the ambit of socialism one branch described as scientific, regarded as authentic, and opposed to all others described as utopian and fallacious, is not only historically erroneous, but will be conducive to a willful mutilation and distortion of the socialist thought in its entirety.<sup>43</sup>

The future will show whether this confrontation between Marxism and socialism will actually take place in Soviet Russia and, if it does, what the outcome will be. It seems to me that the problem as stated by Venturi is a specific Soviet problem, that is to say, much more a problem of Soviet Marxism, or of what goes under the name of Marxism in Soviet Russia, which is not to deny that in our days many a different and specious

<sup>42</sup> *Ibid.*, 1: lxxiii (F.E., 1: 76).

<sup>43</sup> *Ibid.*, 1: xcvi–xcviii (F.E., 1: 99).

interpretation equally misleadingly sails under the same name in the West. It remains, however, to be seen whether Venturi's exhortation—for this is what it is—will bear scholarly fruit in Russia, or whether the Russian historians in deference to their guardians will remain unmoved by Venturi's forceful language and will prefer to continue admiring his great contribution to the history of socialism from afar.

Thanks to the Introduction it is now possible to read Venturi's work with much more understanding of its meaning and message. It would be, therefore, extremely desirable to let the second edition appear in English, or at least to have the Introduction published as a separate book in an English translation.

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# On Diffusionism and Historicity

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A Review Article by R. C. PADDEN

CYRUS H. GORDON. *Before Columbus: Links between the Old World and Ancient America*. New York: Crown Publishers. 1971. Pp. xi, 15-224. \$6.50.

CARROLL L. RILEY *et al.*, editors. *Man across the Sea: Problems of Pre-Columbian Contacts*. Austin: University of Texas Press. 1971. Pp. xvii, 552. \$12.50.

IT WAS THE INSULARITY of the Western Hemisphere as established by Magellan that posed to Europeans of the sixteenth century the complex problem of American origins. Where did the Indians come from? When? How? What were their cultural derivations? Within the prevailing Augustinian world view the staggering dimensions of these questions could be made to shrink in proportion to movement back in time, for if one pushed the primary question back far enough in Christian reckoning one was bound to arrive at the Deluge and Noah. The answer then became obvious: by the historical assumptions of *Genesis* the newly discovered peoples of the Indies were held to have descended from the seed of Noah. This conclusion simultaneously laid the foundation for a doctrine of diffusionism that, in one guise or another, has been with us ever since.<sup>1</sup>

Modern diffusionists, of course, have long since found it necessary to quit scriptural exegesis if not their theories of interhemispherical diffusion. As part of the process of shucking off scriptural authority in favor of more scientific validation, men of the nineteenth century began to apply evolutionary theory to the study of culture. Broadly stated, this movement led to Adolf Bastian's classic postulation of a psychic unity of mankind wherein similar conditions of culture and environment tend to produce in man similar ideas that might also lead to analogous solutions of common problems. So postulated, by a process of parallelism there

<sup>1</sup> See Lee Eldridge Huddleston, *Origins of the American Indians* (Austin, 1967), for a discussion of and a guide to postdiscovery literature and Margaret T. Hodgen, *Early Anthropology in the Sixteenth and Seventeenth Centuries* (Philadelphia, 1964), especially chs. 6, 7, for a critical analysis of Biblical diffusionism and problems of cultural diversity. Also see Arnold Williams, *The Common Expositor: An Account of the Commentaries on Genesis, 1527-1633* (Chapel Hill, 1948).

was now a possibility of independent invention and isolated reinvention. Yet another omnipresent current of nineteenth-century thought combined with parallelism and evolutionary emphasis to produce a new vision of man held fast in the grip of inexorable Progress, rising from barbarism to civilization in readily perceptible stages. Up from savagery.<sup>2</sup> Unilinear cultural evolutionism, as the first major school in the new discipline of anthropology, thus posed a theoretical alternative to classic diffusionism. And, significantly, behind the postulates of cultural evolutionism lay a wealth of archeological and historical data for testing its theoretical constructs. Moving from the 1880s and Augustus Le Plongeon, that acidic and wrathful champion of Atlantean diffusion, to the 1920s and British anatomist G. Elliott Smith, whose Heliolithic theory of diffusion had all the world's high civilizations diffusing from the Nile valley, one encounters a spectrum of diffusionist theories, most of them leaning in the direction of archeological formulation, but without amassing convincing documentation to support their premises.<sup>3</sup> The loss of scriptural authority and failure to find validation for its tenets within a now-triumphant scientism gave to interhemispherical diffusionism both the status and vulnerability of an idea without a citadel.

More pressure was brought to bear on doctrinaire diffusionism with the coming of the Boas era in the early 1940s. In challenge to the sweeping generalizations of two new movements in anthropology—one of them neodiffusionist, the *Kulturkreis* school, the other racist—and to the rigidity of a now-sclerotic unilinear evolutionism, Franz Boas spearheaded a movement largely aimed at the implementation of historicity in archeology and anthropology. Rejecting the gratuitous formulation of airy theories incorporating grandiose generalizations of human behavior, the reformers demanded the substantiation of theory by hard evidence based upon research and field investigation.<sup>4</sup> One may read the agony of diffusionists in the basic assumptions that underlay most of their theoretical constructs in this period: Man is singularly uninventive; similarity of cultural traits from separated areas is an adequate basis upon which to postulate diffusion; and distance and lack of continuity in distribution are unimportant in comparing culture traits or complexes in separated areas. For the doctrinaire diffusionist it was the uninventiveness of man that ruled out the possibility of independent invention. In postulating interhemispherical diffusion on a basis of cultural similarity,

<sup>2</sup> Carroll L. Riley succinctly traces the rise and development of anthropology in the United States, in "American Historical Anthropology—An Appraisal," in Carroll L. Riley and Walter W. Taylor, eds., *Essays in Honor of Leslie Spier* (Carbondale, 1967), 3–21.

<sup>3</sup> See Robert Wauchope's informative and delightful *Lost Tribes and Sunken Continents: Myth and Method in the Study of American Indians* (Chicago, 1962); G. Elliott Smith, *The Diffusion of Culture* (London, 1933). A brief and devastating intellectual confrontation between Smith and three nondiffusionists was presented in G. Elliott Smith *et al.*, *Culture* (New York, 1927).

<sup>4</sup> Outlined in Riley, "American Historical Anthropology," 11–15.

likeness itself achieved the status of proof. Having thus postulated diffusion it followed that contact must have been made, the mere details of which could be relegated to a status of relative unimportance. How, one wonders parenthetically, could diffusionists have been expected to prove what were, judging by the preponderance of conflicting evidence against which they were held, articles of faith in the best Augustinian sense?

What amounted to salvation came to a whole generation of doctrinaire diffusionists in 1945 when A. L. Kroeber, a most distinguished alumnus of the Boas school, gave the Huxley Memorial Lecture. Laudably forsaking his customary vertical borings, he reported on a recent venture into the macrohistory of culture. He had expanded the ancient idea of *Oikoumenê*, that which the Greeks deemed the habitable world, to include the entire sweep of Old World civilization from Britain to Japan. Within this vast laboratory he traced and compared spatial ranges in successive waves of creativity through historical time; he noted the spread of sculpture of high esthetic value; he outlined shared cultural ideas and material components, like the bred horse, divinity of kingship, metallurgy, eunuchism, chess, divination, alchemy, and so on. Some recurrences were easily fathomed while others defied his means of inquiry. For the latter he postulated contextually analogous diffusion even though he had no direct supporting evidence. He recognized the existence of alternative explanations that involved parallelism or convergence but believed that the contextual suggestions of diffusion stemming from proximity and communication—in the presence of similar traits for which diffusion could be demonstrated—were strong enough to rule out autogenesis. And he found no evidence for the latter at all. Therefore, within the context of the *Oikoumenê* he proposed analogous diffusion as the most probable explanation where he lacked specific documentation. As a nursling of the Boas school Kroeber apparently felt obliged to justify his position and did so by stating a simple truism: as much evidence is needed for the assumption of independent invention as is needed to postulate diffusion. The burden of proof is equal. In the case at hand, on the strength of circumstantial evidence and without evidence to the contrary, he proposed the occurrence of analogous diffusion as a high probability.<sup>5</sup>

As so often happens in human affairs, what Kroeber originally said and meant has been eclipsed by what others have chosen to make of it. In one quantum leap interhemispherical diffusionists problematically joined the New World to Kroeber's *Oikoumenê*, as though his assumption of diffusion on a contextually analogous basis within the Old World *Oikoumenê* could by some miracle of analogical manipulation be made

<sup>5</sup> "The Ancient *Oikoumenê* as a Historic Culture Aggregate," in A. L. Kroeber, *The Nature of Culture* (Chicago, 1952), 379-95.

to prove theories of transoceanic diffusion for which both context and evidence were lacking. Ironically, Kroeber, whose whole methodological thrust rejected theoretical postulation without evidence, was now to be enlisted in its support. This necessitated certain omissions, the most important being Kroeber's studied exclusion of the New World from the *Oikoumenê* for lack of evidence. The conclusion that the New World had no place in the *Oikoumenê* was subsequently shared by Gordon W. Hewes, who, seeing Kroeber's *Oikoumenê* as a dynamic cultural process rather than a historical aggregate, converted it into a civilizational multiplier system.<sup>6</sup> Concentrating on processes, Mr. Hewes's analysis points up some of the kinds of evidence that would be apparent had there been prolonged interhemispherical contact before the Columbian era, but that are consistently lacking.

Fully separated from its contextual origins, Professor Kroeber's truism on the equality of the burden of proof emerged as the *dictum sanc-torum* of anyone who would postulate interhemispherical diffusion. Where in the past diffusionists tended to be intimidated by lack of evidence, some now insisted that if they could not assume contact and diffusion without evidence, then traditionalists could not assume the isolation of the Western Hemisphere and its independent development, because the burden of proof was equal.<sup>7</sup> As was exemplified in a challenge made by a transpacific diffusionist, let the traditionalists prove that Aztec and Maya writing systems were invented without any suggestion from Old World systems. "In a historical question," responded Philip Phillips, "how does one prove that something did not happen otherwise than by pointing to the lack of evidence that it did?"<sup>8</sup>

The crux of the problem is that such a quantum leap in analogy fails to provide evidence of the intercommunication that is at once the engine of diffusion in Kroeber's *Oikoumenê* and the ultimate necessity of any process of transoceanic diffusion. Accordingly, diffusion and autogenesis would appear to be logical alternatives as explanations of the cultural origins of the New World: if there is no evidence of contact and diffusion then one must assume independent development because under such conditions probability lies inversely proportionate between them. The burden of ultimate proof remains equal, but interim assumptions of probability manifestly do not.

<sup>6</sup> Gordon W. Hewes, "The Ecumene as a Civilizational Multiplier System," in *Kroeber Anthropological Society Papers*, no. 25 (Berkeley, 1961), 73-109. This work has meaning for historians of culture.

<sup>7</sup> The equation is inherent in John L. Sorenson, "The Significance of an Apparent Relationship between the Ancient Near East and Mesoamerica," in Riley, *Man across the Sea*, 219-41, and Thor Heyerdahl, "An Introduction to Discussion of Transoceanic Contacts," in *Proceedings, XXXVII International Congress of Americanists* (Buenos Aires, 1968) 4: 67-88.

<sup>8</sup> Philip Phillips, "The Role of Transpacific Contacts in the Development of New World Pre-Columbian Civilizations," in *Handbook of Middle American Indians* (Austin, 1966), 4: 311-13. Cf. Robert Heine-Geldern, "The Problems of Transpacific Influences in Mesoamerica," *ibid.*, 277-95.

THE MOST RECENT enterprise in interhemispherical diffusionism is that undertaken by Cyrus H. Gordon in *Before Columbus: Links between the Old World and Ancient America*. According to Professor Gordon a highly sophisticated civilization of sea lords ruled and roamed the earth's seas in regular voyages of exploration and trade throughout the Bronze Age (ca. 3000–1200 B.C.). By their mastery of mathematical sciences and their cultural attainments they are held to have been the authors of all subsequent high civilizations. The Western Hemisphere was intimately known to them and under their aegis became the scene of a global intermingling of peoples and cultures. Corollary processes of diffusion created a number of brilliant American civilizations that, in their later phases, are known to us as Maya, Aztec, and Inca. Mr. Gordon concludes: "The breathtaking achievements of the Mesoamericans could not be, and were not, the works of savages who lifted themselves up by their bootstraps. Instead they are the culminations of mingled strands of civilization brought to these shores by a variety of talented people from Europe, Africa, and Asia." It seems essential that the author of so startling a thesis should be permitted to explain how it is developed and sustained:

Our method is straightforward. We shall follow the primary sources to their simple and direct logical conclusions, unhampered by opinion that runs counter to the primary sources. For this reason we will not be concerned with any Gallup Poll type of approach; 49 percent support does not make anything necessarily wrong; nor does 51 percent support make it necessarily right. . . . Nor are we committed to any school of thought such as "diffusionism" on the one hand or "independent inventionism" on the other.<sup>9</sup>

In the face of Professor Gordon's thesis, the latter disclaimer seems inexplicable.

As a proponent of cultural unitarianism Gordon believes that the real world-history of mankind has yet to be written, which suggests a philosophical ground commonly held by diffusionists: the universality of world history, as established by German historiography of the nineteenth century, was deformed by American pragmatism and its applied evolutionism and parallelism. But diffusionism is to revive universality through its historical unification of Europe, Asia, Africa, and Mesoamerica in the pre-Columbian era.<sup>10</sup> The affinity of such a view to Kroeber's *Oikoumenê* is obvious, and one finds Professor Gordon employing it simply as "ecumene" but without reference to those elements embarrassing to his thesis.

For a solution to the problematical lack of intercommunication and transport between the Old and New Worlds, the author avails himself

<sup>9</sup> Gordon, *Before Columbus*, 30, 16.

<sup>10</sup> See, for example, Wilhelm Koppers, "Das Problem der Universalgeschichte im Lichte von Ethnologie und Prä-historie," *Anthropos*, 52 (1957): 369–89.



of Charles H. Hapgood's *Maps of the Ancient Sea Kings*.<sup>11</sup> Working from the 1513 map of the Turkish admiral Piri Reís, Mr. Hapgood theorizes that the makers of the source maps—supposedly Bronze Age—from which the Piri Reís map was drawn had mastered the spherical trigonometry of map projection. If his analysis and extrapolations should be proved correct and if it could be established that the source maps were adequately ancient, then there would exist circumstantial evidence of a preclassical science and civilization that dwarfed every other until the nineteenth century. Mr. Hapgood's theories and some of their implications are incorporated in the Gordon thesis, not for consideration as theory but as a basis for assuming interhemispherical diffusion spanning the Bronze Age under the aegis of a preclassical thalassocracy.

There is an immediate problem: the chronologies of Mesoamerica's varied cultural precipitations, as they are presently understood through historical and archeological evidence, do not coincide with the Hapgood chronology and therein offer Mr. Gordon's thesis formidable contradiction. Moreover, if one is to predicate diffusion it is incumbent upon one to nucleate the evidence, to demonstrate the spatial and chronological overlaps between donor and recipient cultures within which specific traits of culture and their developmental chronologies are to be established. Professor Gordon makes no attempt to reconcile the chronological discrepancies, nor does he provide any sort of datum chronology. Instead he questions the validity of the evidence against his thesis by arguing in piecemeal fashion that archeology tends to confirm whatever is traditional. This is further explained as a result of overspecialization, which creates hyperskepticism, so that archeologists and historical traditionalists are prone to be hyperskeptical and negative toward new ideas.<sup>12</sup> As evidence he cites Heinrich Schliemann's dramatic unearthing of Troy, so long believed by archeologists to be nothing more than a figment of Homeric myth. This theme is sufficiently recurrent to suggest that Professor Gordon's ultimate purpose is to play Schliemann to Mesoamerican myth.

Having assumed the means of transoceanic diffusion in the Bronze Age, the author suggests a series of interhemispherical "links" that is held out as proof of his thesis. The master link in the chain, one forming a leitmotiv by its reinforcement of unrelated conclusions, is based on the imposing figure of Quetzalcóatl, the Plumed Serpent, whom the author invests with a specific myth. It is Professor Gordon's contention that Aztec, Maya, and Inca civilizations all shared an identical tradition wherein the civilized arts were originally brought to them by a bearded white man who came from the east over the Atlantic by boat. To the Aztecs he was Quetzalcóatl; the Maya called him Kukulcan; among the Inca he was known as Uiracocha. This Plumed Serpent myth, the author

<sup>11</sup> Charles H. Hapgood, *Maps of the Ancient Sea Kings* (Philadelphia, 1966).

<sup>12</sup> Gordon, *Before Columbus*, 170, 121, 79.

insists, was a consistent and persistent American tradition. On the other side of the Atlantic Professor Gordon points to a pediment from a temple on the Athenian Acropolis that portrays several bearded man-serpents, presumably out of Greek mythology, in feather-trimmed garb, and argues that the legends of Quetzalcóatl, like Greek legends, reflect "historic movements." Now the ghost of Schliemann appears via the analogical implication that overspecialized archeologists and ethnohistorians have missed the real significance of Quetzalcóatl, which is presently to be disclosed by an amateur armed with new ideas. Mr. Gordon forthwith identifies the Plumed Serpent as a meandering Mediterranean and postulates the link thus:

This embodies the essential traits—at two levels—of the American iconography. First, we are dealing with a bearded white man from the Mediterranean; second, he is at the same time a feathered serpent. There are too many details involved to be attributed to accident. The diffusion of ideas from the Mediterranean to Mesoamerica explains the facts more reasonably than a psychological approach implying that it is so natural for men to conceive of bearded white men who are at the same time feathered serpents, that the same combination naturally developed independently at ends of the earth in isolation.<sup>13</sup>

The validity of this link bears upon the historicity of Professor Gordon's entire thesis. Leaving aside his gratuitous assumption of transport and the unresolved chronological contradictions, one must raise a crucial question: Was there, in fact, such a persistent and consistent American Plumed Serpent tradition? Since the sixteenth century, dilettantes, whose writings on this theme usually gravitate to the "esoteric-occult-curiosa" stalls of used-bookshops, have suggested this refrain in part if not in whole.<sup>14</sup> But the primary sources deny it. It all depends upon whom one chooses to believe. Mesoamerican primary sources represent Quetzalcóatl as an integral part of the cultural inheritance received by the Aztecs from the Toltec past. As such Quetzalcóatl was held to be the inventor of civilization and its arts, not a mere introducer of them. And in both Toltec cosmogony and Aztec tradition he was invariably held to be native-born. The *Leyenda de los soles* treats him as a local creator deity, while the *Anales de Cuauhtitlán* register his birth in Tollan as a result of parthenogenesis.<sup>15</sup> There are many other traditions, but virtually all agree on his native origin. The historical Quetzalcóatl appears to have been a son, or possibly grandson, of the conqueror Mixcoatl. Later in life, as a priest-king of Tula, he lost a struggle for power and was forced out

<sup>13</sup> Gordon, *Before Columbus*, 170, 52-53.

<sup>14</sup> Wauchope, *Lost Tribes*, 6, *passim*. By including the Andean world Professor Gordon stretches Mesoamerica out of meaningful shape. Mesoamerica is commonly understood to be comprised of the area between Mexico's Pánuco and Lerma River basins in the north and central Honduras in the south, or that area which is delineated by common calendrical and agricultural adaptations.

<sup>15</sup> *Codice Chimalpopoca* (Mexico, 1945), 120-21, *passim*; 6-8. Also see *Florentine Codex*, ed. and tr. A. J. O. Anderson and C. E. Dibble, 1 (Santa Fé, 1950): ch. 5.

of the country, apparently in 987 A.D.<sup>16</sup> Maya sources, which provide exact dating, pick him up at this point. He and his partisans gathered an army and invaded the Maya world; after a violent conquest he established his capital on the older one at Chichen Itzá. There he imposed the feathered serpent cult, for which there is archeological evidence along with other Toltec cultural imprints. He thus became known in Maya tradition as Kukulcan, no culture hero from the east, as Professor Gordon imagines, but a brutal military conqueror from the west. The celebrated Mayan prophets, to whom we shall return, never identified Kukulcan or Quetzalcóatl with their prophecies, nor is there any known primary source that connects Kukulcan with the much earlier people of Itzá who, according to obscure legend, came from the northeast by sea. "Kukulcan," concludes J. Eric S. Thompson, "was but a flash in the Maya pan."<sup>17</sup> Gordon is equally uninformed when he claims that the Inca cherished the same tradition. Like the Tolteca, they conceived a local creator-deity, a culture hero of the Andean world who invented the civilized arts. He went nameless but had many titles, one of which, *Wiraqoca*, was hispanized into "Viracocha." He bore no relationship to bearded white men, benevolent strangers, or Plumed Serpent cult in fancy or in fact.<sup>18</sup> There is not and never was a generalized indigenous American Plumed Serpent myth. Nor is there any evidence that the Greek serpent-men were "Plumed Serpents" in the Mesoamerican sense, or vice versa. Considering the universality of the serpent as symbol in mankind's mythic creations, the comparison is narrowly and naively contrived.

As Schliemann's ghost is laid, two relevant points obtrude: Heinrich Schliemann, however much an amateur, knew his Homer. Professor Gordon did not know the primary Mesoamerican sources. The second point may follow from the first. The myth of Troy had an objective reality in Homer, but the Plumed Serpent myth, as propounded by Mr. Gordon, has none.

Another link is proposed in a series of parallels that the author perceives between Mediterranean traditions and the *Popol Vuh*, the sacred

<sup>16</sup> On Quetzalcóatl as both man and idea, see Miguel León Portilla, *La filosofía Náhuatl* (Mexico, 1959); Laurette Sejourne, *El universo de Quetzalcóatl* (Mexico, 1962); B. C. Hedrick, "Quetzalcóatl: European or Indigene?" in Riley, *Man across the Sea*, 255-65; Paul Kirchhoff, "Quetzalcóatl, Huemac, y el fin de Tula," *Cuadernos Americanos*, 84 (1955): 164-96; and Angel María Garibay Kintana, *Historia de la literatura Náhuatl* (Mexico, 1953). Quetzalcóatl is the great enigmatic figure of the Western Hemisphere. Since the sixteenth century interhemispherical diffusionists, largely following Spanish missionaries who had their own reasons for considering Quetzalcóatl non-American, have made him out to be a Jewish St. Thomas the Apostle, a Roman, an Icelandic missionary, a Carthaginian, an Irish saint, an East Indian, and in the present case, a misplaced Mediterranean. For details, see Manuel Orozco y Berra, *Historia antigua y de la conquista de México* (Mexico, 1960), 1, ch. 5, and Vicente Riva Palacio, *México a través de los siglos* (Mexico, 1940), 1, bk. 3, ch. 2.

<sup>17</sup> J. Eric S. Thompson, *The Rise and Fall of Maya Civilization* (Norman, 1966), 265.

<sup>18</sup> For details, see John H. Rowe, "Inca Culture at the Time of the Spanish Conquest," in *Handbook of South American Indians*, 2 (Washington, 1946): 293 n. 28.

book of the ancient Maya. Ostensibly these apparent cognates are held to indicate pre-Columbian contact. For the most part Professor Gordon merely elaborates what we already know, that mythologies the world over show striking similarities. Like others who have cited the *Popol Vuh* and similar materials, Gordon's lack of familiarity with it permitted a misapprehension that he was in direct contact with the ancient Indian mind and its ideas and could therefore draw straight comparative lines between Old and New World texts. What he did not understand is that ancient Indian traditions were oral long before they were written and that they lost something—perhaps a great deal—when they were subjected to the limitations of the Roman alphabet and the impositions of a foreign syntax. Even though the Indian(s) who first translated and edited the *Popol Vuh* remain anonymous, we know from critical analysis of other ancient Mesoamerican texts and commentaries on them that the only Indians capable of such labors were those who had been trained to it by Mendicant mentors. In the course of their education they were invariably Christianized and Latinized and could not escape the refractive power of such a lens. One must therefore question whether the ideas and traditions taken from antiquity were being reported as they actually existed or whether the moral values and intellectual discriminations of a foreign culture were operating as a mesh through which ancient ideas were being forced and thus inadvertently distorted. It is entirely possible, even probable, that Professor Gordon was correlating the work of an Indian editor who was himself closer intellectually and religiously to some of the European texts with which his work is being compared than he was to the ancient Indian traditions.<sup>19</sup> Comparatively speaking, the works of Mayan prophecy, like the books of Chilam Balam and Nahau Pech, provide suggestions of Christian tenets and moral distinctions having been read in. Quite aside from these hazards, the great Chilam Balam of Chumayel first made his celebrated prophecy of Spanish arrival and the new religion when the Spaniards were already swarming over the Caribbean.<sup>20</sup> They were using the Carib tongue, which the Maya knew, and when one considers the incredible amount of religious propaganda the Spaniards employed, the clairvoyant powers of the seer are diminished even as the erstwhile religious cognates he expressed are rendered suspect. These are not reliable sources upon which to predicate pre-Columbian contact.

Many of the friars who taught Latinity to the Indians themselves wrote of the Indian past, and their works comprise an invaluable body of sources. But it must be kept in mind that, with few exceptions, they

<sup>19</sup> There is yet a further question about the editing of the first Indian edition carried out by the cleric who discovered the manuscript. Some aspects of this problem are discussed more fully in R. C. Padden, *The Hummingbird and the Hawk: Conquest and Sovereignty in the Valley of Mexico, 1503-1541* (Columbus, 1967), ix-xvi.

<sup>20</sup> See Ralph L. Roys, *The Book of Chilam Balam of Chumayel* (Norman, 1967).

labored under the historical assumptions of *Genesis* and on that account postulated diffusion even if it required distortion of the evidence. Witness the otherwise trustworthy Fray Diego Durán in action: his source was an ancient Indian manuscript that told of tribal migration to Mexico from the north. Pictographs plainly showed a volcanic eruption with an accompanying earthquake; the air was filled with volcanic ash and a hapless Indian had fallen into a gaping seam as the earth opened beneath his feet. Durán interpreted the scene as an Indian version of the *Book of Numbers* wherein the earth swallows up Korah, Dathan, and Abiram. The ash was identified as the manna with which God sustained the Hebrews in the desert.<sup>21</sup> On the basis of such interpretation latter-day diffusionists could contend the Indians' pre-Columbian knowledge of the Bible.

Professor Gordon considers Mesoamerican sculpture to be a picture gallery that unmistakably shows the racial origins of its original subjects. Among many examples he includes a striking photograph of what is described as a Postclassical Mixtec head from Oaxaca: "The black color and the features, such as thick lips, leave no doubt in anyone's mind that the artist has portrayed a Negro."<sup>22</sup> The presence of a Negro in this time and place constitutes another link in Professor Gordon's chain. But at what cost? He has reduced analytical criticism, without which there can be neither artistic nor archeological determination, to a state of simplistic absurdity. Apparently, what you are looking at is nothing more than whatever it appears to be. On that basis, Preclassical figurines, with their typically too-large heads, puffy faces and mongoloid eyes, would necessarily represent a society of hydrocephalics. The diffusionist whom Gordon follows here has elsewhere described this technique as a science of "faciology."<sup>23</sup> An alternative explanation of the head, if it is Mixtec and Postclassical, suggests that the thick, downturned lips exemplify a widely used man-tiger stylistic convention based upon a legendary transformation of beast into man that was present in Mesoamerica from early Monte Albán times. The black color, if not inherent in the stone itself and therefore incidental, could represent what was later known as the black *ulli*, a staining pigment commonly applied to the bodies of priests and gods and having nothing at all to do with an attempt at a realistic portrayal of skin pigmentation.

It would seem profitless to discuss in detail the rest of Professor Gordon's links. He devotes an entire chapter to the evidence of language without producing any linguistic evidence. Competent linguists simply do not compare isolated words of unrelated languages in order to dem-

<sup>21</sup> Fray Diego Durán, *Historia de las indias de Nueva España y islas de Tierra Firme* (Mexico, 1951), bk. 1, ch. 1.

<sup>22</sup> Gordon, *Before Columbus*, 22.

<sup>23</sup> Alexander von Wuthenau, "Representations of White and Negro People in Precolumbian Art," *Proceedings, XXXVI International Congress of Americanists* (Seville, 1964), 1: 109-10.

onstrate historical relationships. No one has yet established a continuity of linguistic families between the hemispheres in the pre-Columbian period. His chapter on ancient Greek authors is a rehash of previous speculations, contributing nothing of evidential value to his thesis. Materially, he offers a number of freak finds as proof of contact: an excavated Roman head, Hebrew and Roman coins, and inscribed stones and wall inscriptions. There has been a reluctance to accept such evidence. But even if it should be genuine it could prove nothing beyond a specific episode of pre-Columbian contact, and there is nothing earthshaking in that. Non-diffusionists are increasingly willing to concede that from time to time there could have been accidental landfalls and shipwrecks that resulted in contact. What is lacking is any apparent connection between these freak finds and Mesoamerican cultural development. One specialist in this area concludes that: "Items like these have so often proved elusive, unreliable, faked, or with such other disabilities as evidence that they must be ignored for practical purposes."<sup>24</sup>

One searches in vain for a coherent methodology in this work, for some systematic formulation of analytical criteria and evidence of its application. On a basis of superficial similarities Professor Gordon learnedly proposes "genetic relationships" between megalithic structures from Malta to Stonehenge, as borne out by "interlocking major features and details." "Genetic" is an unfortunate qualifier here; the interlocking major features and details are as follows: Malta and Stonehenge have horseshoe formations, and both are oriented toward celestial phenomena; a megalith at Stonehenge has carved on it a "distinctly Mycenaean" dagger; spiral figures carved on some of the monuments resemble the spiral form of the zodiac as represented on Egyptian remains; two such spirals carved on a Maltese temple are to be compared with two spirals on the Phaistos Disc from Crete, one other detail of which, in yet another context, is compared with a detail of an Aztec glyph and held as proof of pre-Columbian contact. Competent analysis requires that the possibility of convergence as a factor be examined. We know that the developmental processes of architectural traditions tend to produce structures of remarkable likeness in cultures widely separated in time and space. It is probable that structural analysis of these megaliths would demonstrate quite different derivations of these superficially similar monuments. But in lieu of analytical sufficiency it is "faciology" over and over again.<sup>25</sup>

Although Professor Gordon began with the declared intent to follow the primary sources to their logical conclusions, contrary opinion notwithstanding, he appears to have done precisely the opposite. Mesoamerican primary sources, both historical and archeological, argue persuasively that it was the American Indians themselves who created their brilliant

<sup>24</sup> Sorenson, "Significance of an Apparent Relationship," 223.

<sup>25</sup> Gordon, *Before Columbus*, 80, 93.

civilizations. Mr. Gordon rejects those sources and on a basis of diffusionist opinion proclaims the Mesoamericans to have been wretched dummies incapable of any such creation or development by themselves. From that point it is all downhill. The drawback is that his thesis, by its estrangement from Mesoamerican primary sources, is fatally devoid of historicity.

If *Before Columbus* is stereotypical of the many diffusionist arguments that have gone before it, *Man across the Sea* is not. Quite the contrary, it suggests the end of traditional transoceanic diffusionist postulation, especially as epitomized in the former work. As its title suggests, *Man across the Sea* is concerned with problems of pre-Columbian contact between the hemispheres and had its origin in a symposium on that theme held by the Society for American Archaeology in May 1968. Most of the papers presented are included in the present volume, together with others already published by the Southern Illinois University Museum in 1969. The volume is divided into three sections on theory, transoceanic contacts, and cultural geography respectively, with editorial commentaries.

Generally considered this book is not likely to be cheered by doctrinaire interhemispherical diffusionists. In spite of exhaustive research in ax and adz hafting, watercraft and sailing, ceramics, and intensive analysis of plant and animal evidence, there is still no proof, no hard evidence on which to predicate pre-Columbian contact and diffusion from the Old World to the New or vice versa. In fundamental theory the editors and commentators appear to agree that man is inventive after all. For some time now historians of culture change have demonstrated that it is invalid to assume the mutual exclusivity of diffusion and independent invention in human cultural development; they are differing aspects of a larger and more complex process of culture change, which neither can in itself explain. The abundant historical evidence of Spanish entry into the New World shows the adaptive process combining both evolutionary and diffusive mechanisms within the more general processes of transculturation.<sup>26</sup> The editors of *Man across the Sea* appear to agree with the historical argument in denying that diffusion and evolution as cultural processes are mutually exclusive, since the same integrative mechanisms seem to function in either case.

Methodological criticism is more pointed. David H. Kelley, writing on "Diffusion: Evidence and Process," warns that it is not possible to postulate diffusion credibly by mere compilation and comparison of artifacts or traits or by equally superficial comparisons of function. The in-

<sup>26</sup> See R. C. Padden, "Cultural Change and Military Resistance in Araucanian Chile, 1550-1730," *Southwestern Journal of Anthropology*, 13 (1957): 103-21; Padden, *The Hummingbird and the Hawk*; and Charles Gibson, *The Aztecs under Spanish Rule* (Stanford, 1964).

tegrative process can so change the diffused form that it is rendered unrecognizable; the trait is too much altered in the process and is therefore lost as evidence. "The criterion of functional equivalence between compared systems or artifacts as a test for historical relationships," he concludes, "seems almost completely invalid."<sup>27</sup>

Jon Muller takes a brilliantly critical look at "Style and Culture Contact," arguing that comparisons based on form alone—outward shapes and visible characteristics—are at best questionable. Such similarities, whether of massive structure or decorative detail, are often shown to be superficial by structural analysis, which entails thorough investigation of the cultural and ecological conditions surrounding the objects being compared. He seems to be saying that the cultural history of the objects being compared shows their chains of derivation, which, if different, nullify the basis of comparison. One significant upshot of this thesis is that diffusionists may no longer argue contact and diffusion on the ground that the compared forms are too complex to have arisen in two places independently without first having applied structural analysis to them. If one does not know how and why compared forms came into being, one cannot know that they are of the same derivation and hence comparable. These points, along with Mr. Muller's keen analysis of stylistic criticism, have profound implications for future stylistic comparisons.

Though the need for establishing genuine equivalence between compared traits is sharply drawn in this volume, the need for equivalence between "problem area" designation and the actual distribution of compared traits is not. There is no such penetrating examination of the diffusionist's arbitrary delimitation of problem areas within which he postulates diffusion from point A to point B. In spite of the papers on methodology and theory, otherwise informative and useful, the reader is left to wonder why diffusionists so often fail to consider the distribution of the traits they are comparing beyond their immediate "problem" areas. If, for example, the traits described as common to both Japan and Mexico and thus held to be indicative of diffusion are also to be found in Morocco and the Levant and wherever, by what definition is there a transpacific problem area? And does not the existence of the trait outside the problem area invalidate the premise of diffusion within it? These questions were raised by John H. Rowe several years ago, but in spite of responses by diffusionists they have not yet been satisfactorily answered.<sup>28</sup>

On the positive side *Man across the Sea* indicates that in recent years

<sup>27</sup> David H. Kelley, "Diffusion, Evidence and Process," in Riley, *Man across the Sea*, 61-62. Mr. Kelley points up the urgent need for more sophisticated analysis of the processes of culture change, a position with which historians readily agree. But at the same time one can but wonder why so many of those who postulate diffusion seem reluctant to take into account H. G. Barnett, *Innovation: The Basis of Cultural Change* (New York, 1953), and Margaret T. Hodgen, *Change and History* (New York, 1952).

<sup>28</sup> John H. Rowe, "Diffusionism and Archaeology," *American Antiquity*, 31 (1965-66): 334-37; Stephen C. Jett and George F. Carter, "A Comment on Rowe's 'Diffusionism and Archaeology,'" *ibid.* (1966): 867-70; Heyerdahl, "An Introduction to Discussion of Transoceanic Contacts."



there has been a shifting of gears and directions and that some scholars who continue to be intrigued by the idea of transoceanic diffusion have abandoned the taxonomical-comparative approach of the old scientism and are reaching for the new. As Carl O. Sauer cogently noted twenty years ago, the heart of the transoceanic contact and diffusion problem is that one is dealing in the present with a past that does not come again and cannot be verified experimentally.<sup>29</sup> That obstacle was more intimidating to his generation than to ours; however much scholars in disciplines outside the confines of pure science sought to emulate scientific method they could not achieve the certainty of predictability that was so essential to scientific method itself and to the validation of proof as scientific. There were inescapable consequences, one of which was a foreshortening of theoretical vision with an attending misapprehension of potentialities. Looking at the future from within the discipline of sociology a quarter century ago, for example, who could foresee its present dimensions? Since the splitting of the atom, however, science has seized upon probability as avidly as it had predictability in earlier times, and, while predictability continues to be a useful element of scientific method, calculated probability has arisen as an empirical alternative to certainty. It is this that has freed the social sciences and humanities from their former bondage; and in the long run it may well prove to have been this, rather than the unleashing of atomic fire, that labels the event Promethean.

George F. Carter, summing up his researches on the question of "Pre-Columbian Chickens in America," provides *Man across the Sea* with a fair example of how the new scientism is changing the search for evidence and perhaps the nature of the diffusion problem itself. The chicken debate has been going on since the sixteenth century. Spaniards could and could not remember—but could never agree—on whether or not the chicken was already present when they overran the Indies.<sup>30</sup> Professor Carter approaches the problem in multiple ways:<sup>31</sup> he delves deeply into zoological and genetic data; he exhausts the linguistic aspect; he compares the sociocultural complexes that surround the bird wherever it is found, especially in America, Polynesia, and Asia. His accumulated evidence establishes a possibility of pre-Columbian introduction. Next he painstakingly constructs a historical model of the origin and dispersals of the chicken in Europe, fixing precise rates of diffusion. Then

<sup>29</sup> Carl O. Sauer, *Agricultural Origins and Dispersals* (New York, 1952), 1. This brilliant contribution is once again available under the title *Seeds, Spades, Hearths, and Herds* (2d ed.; Cambridge, Mass., 1972).

<sup>30</sup> The basic literature is E. Nordenskiöld, "Deductions Suggested by the Geographical Distribution of Some Post-Columbian Words Used by the Indians of South America," in *Comparative Ethnographical Studies*, no. 5 (Göteborg, 1922), 1-46; Ricardo E. Latcham, *Los animales domésticos de la América pre-columbiana* (Santiago, Chile, 1922); Sauer, *Agricultural Origins and Dispersals*; R. M. Gilmore, "Fauna and Ethnology of South America," *Handbook of South American Indians*, 6 (Washington, 1959): 345-464.

<sup>31</sup> At the risk of oversimplifying his sophisticated methodologies I have taken Mr. Carter's procedures out of order and context in an attempt to conserve space.

he builds a duplicate American model—and finds a diffusion rate that is by comparison so much faster that European introduction of the chicken is rendered incredible. He further summarizes the argument for Spanish and Portuguese introduction of both Asiatic and European chickens after 1492, but the case for pre-Columbian Asiatic origin is more convincing by far. The archeological record, one chicken bone, should settle this question absolutely.

Perhaps the clearest example of calculated probability one can suggest in the context of this paper is Paul Tolstoy's "Culture Parallels between Southeast Asia and Mesoamerica in the Manufacture of Bark Cloth." He theorizes that the highly technical process of bark-cloth making diffused from Southeast Asia to Mesoamerica in the pre-Columbian era. It is his method that is most relevant here.<sup>32</sup> Briefly, Professor Tolstoy identifies and graphs out the obligatory and optional decisions—crossroads at which the bark-cloth maker is faced with a functional or material choice in the course of manufacture. He likewise lays out the actual options that are available to the worker in taking these decisions. High correlations of decisions made in the presence of similar alternatives are held to increase the probability of imitation and to decrease the likelihood of independent invention. His correlations appear to be high enough to pose the ultimate question: how much calculation is enough? On the face of it probability would appear to be adequately calculated when contact and diffusion, even without apparent means, is easier to accept than duplication of the statistics of bark-cloth manufacture merely by chance. Leaving aside the crucial matter of historicity, to which we shall return, there is a reservation to be considered in this context. The value of calculated probability is necessarily modified by the presence or absence of corroborative historical sources; mere probability can be illusory. The given statistics and their correlations on Asia and Mesoamerica, for example, have apparently not been subjected to analytical comparison with those of bark-cloth manufacture over the rest of the world. Until that is accomplished it is difficult to see how the present correlations in themselves can be held indicative of any specific, directional process of diffusion or how the existence of a transpacific diffusion "area" can be assumed.

These approaches to the question of interhemispherical diffusion coincide with the much earlier interests of cultural geographers in the problems of plant migrations.<sup>33</sup> Phytogeographers have long suspected interhemispherical contact. If a cultigen could not possibly have spread

<sup>32</sup> Paul Tolstoy, "Culture Parallels between Southeast Asia and Mesoamerica in the Manufacture of Bark Cloth," in *Transactions of the New York Academy of Sciences*, 2d ser. (1963) 25: 646–62; Tolstoy, "Method in Long Range Comparison," *Proceedings, XXXVI International Congress of Americanists* (Seville, 1964), 1: 69–89.

<sup>33</sup> See, for example, Jacques Barrau, ed., *Plants and the Migrations of Pacific Peoples* (Honolulu, 1963); J. W. Purseglove, *Tropical Crops* (New York, 1968); Saucr, *Agricultural Origins and Dispersals*; and Herbert G. Baker and G. L. Stebbins, eds., *The Genetics of Colonizing Species* (New York, 1965).

by natural means, then human intervention seems the only alternative explanation. The final section of *Man across the Sea* thus includes updated studies of the coconut, the bottle gourd, and the sweet potato and of squash, maize, cottons, and beans. As proofs of diffusion they are either negative or inconclusive, and they generally fail to support the fancies of traditional diffusionists, yet these studies are not without provocative suggestions. The work of Herbert G. Baker, this section's commentator, argues persuasively for the probability of human intervention in the dispersal of the kapok tree in pre-Columbian times.<sup>34</sup> It is worth noting the spread of disciplines represented in this section: two geographers, two geneticists, one biologist, one ethnobotanist, one anthropologist, and three botanists. One finds here methodological and speculative levels more consistently impressive than elsewhere in the volume.

The telling difference between the traditional advocates and the more recent proponents of diffusionism is that the former tended to speculate on the probable validity of gratuitous assumptions,<sup>35</sup> while the latter attempt to calculate probability on a basis of data analysis. The old route of easy abstractions invariably led to gross oversimplification, implying commensurate irrelevance for historical sources. Probability, if adequately calculated, itself becomes a kind of ahistorical evidence, neither documentary nor circumstantial; yet it points up directions in which historical evidence might be researched and developed. But probability never tells us how or who and seldom indicates when. Whatever the disciplines involved and the methodologies employed in the calculation of probability, the problem to which it must be applied, whether that problem be the origins of agriculture or cultural diffusion, is preeminently historical in nature.<sup>36</sup> But historical problems are not often amenable to biological or anthropological solutions. And there is the rub. Most of the methods by which probability is calculated tend to be essentially synchronic. According to hematologists, for example, the blood group genetics of Polynesia suggest the conclusion that modern Polynesians result from a gene pool derived from Indonesia, western Polynesia, and South America.<sup>37</sup> This data possibly has relevance for

<sup>34</sup> Herbert G. Baker, "The Evolution of the Cultivated Kapok Tree: A Probable West African Product," in D. Brokensha, ed., *Ecology and Development in Tropical Africa* (Berkeley, 1965).

<sup>35</sup> An example is provided by Gordon F. Ekholm, "Transpacific Contacts," in Jesse D. Jennings and Edward Norbeck, eds., *Prehistoric Man in the New World* (Chicago, 1964), 498-501.

<sup>36</sup> See Ping-Ti Ho, "The Loess and the Origin of Chinese Agriculture," *AHR*, 75 (1969-70): 1-36. It is not my intention to obscure the meaningful distinctions between historical problems and nonhistorical problems in history. My point is that even though multidisciplinary calculations have brought the question of pre-Columbian chickens down to the matter of one tiny bone, if and when that bone is found the historical question will be primary.

<sup>37</sup> Ruben Lisker, "El origen de los grupos humanos en América: Serología y hematología en general de los amerindios y sus posibles relaciones trans-pacíficas," *Proceedings, XXXVI International Congress of Americanists* (Seville, 1964), 1: 43-51; Roy T. Simmons, "The Blood Group Genetics of Easter Islanders (Pascuense), and other Polynesians," in Thor Heyerdahl and Edwin

postulating migration of the sweet potato out from South America; but, for the historicity upon which any such postulate must eventually depend, it is signally irrelevant. As we have seen, anthropologists commonly amass culture traits for purposes of comparison and, on occasion, predication of diffusion. Although the traits have been taken from widely separated time levels and sources, for analytical and comparative purposes they are assumed to be constant. But in separating the traits from their chronological and spatial identities the entire analytical structure is divested of historical reality and validity. Any probability value that may have been calculated is thereby compromised with respect to the predication of actual historical process. Inescapably, any reconstruction of the past will lose its historicity in direct proportion to the departure of its elements from chronological and spatial sequence.

This is where diffusionists have so often come to grief: they seldom find it possible to accommodate theory to historicity, which, in historical questions, has always been the ultimate test. Some simply refuse, arguing that unsolved problems of nonmatching sequences and distribution—the essence of historicity—are irrelevant before the implications and demands of anthropological theory. Strangely enough, one person who makes that argument pleads in the next breath that “solving the mystery of man’s past” is her central goal.<sup>38</sup> Historians have not rejected diffusionist claims out of obtuseness or fear of new ideas. They have refused to accept the postulation of historical processes and events that are patently lacking in historicity.

Probability, when precisely calculated from sources and data commensurate with historical methodologies, is an invaluable tool for reconstructing the past, especially a past filled with shadows. For those of us who work in ethnohistory calculated probability is indispensable. It seems evident that whatever their disciplinary foci, exponents of diffusionism must eventually come to terms with historicity because it is the *sine qua non* for comprehending the development of human culture and for any credible demonstration of the phenomena accompanying that development. The problems are truly formidable and will not, as in the past, be obviated by simplifying their dimensions to fit inadequate methodologies or to satisfy theoretical limitations.

The editors of *Man across the Sea* believe that the question of what constitutes acceptable evidence is the major problem facing scholars today in any discussion of diffusionism. And one could agree that for diffusionists the problem of evidence is as old as the idea itself. But the diffusion of culture is a historical question ultimately and in the realm of

N. Ferndon, Jr., eds., *Reports of the Norwegian Archaeological Expedition to Easter Island and the East Pacific*, 2 (Stockholm, 1965), 333–43.

<sup>38</sup> Betty J. Meggers, “North and South American Cultural Connections and Convergences,” in Jennings and Norbeck, *Prehistoric Man in the New World*, 511–26.

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## Reviews of Books

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### GENERAL

JACOB VINER. *The Role of Providence in the Social Order: An Essay in Intellectual History*. (Memoirs of the American Philosophical Society, volume 90. Jayne Lectures for 1966.) Philadelphia: the Society. 1972. Pp. viii, 113. \$2.00.

No informed reader can hope to derive unalloyed pleasure from these four brief, elegant lectures delivered at Philadelphia in 1966, given as they were just four years before the author's death. Nor, of course, does the editor regard this volume, lacking in footnotes and index, as a fitting memorial to an outstanding scholar—one of the greatest economists America has produced. Nevertheless, those who have been awaiting Viner's long-promised study of the European background to religious thought and economic society in America, which was announced more than a decade ago, must derive what crumbs of comfort they can. Moreover we may fervently hope that this is one of several posthumous works, a possibility suggested by Professor Fritz Machlup's helpful article, "What was Left on Viner's Desk" (*Journal of Political Economy*, 80 [1972]: 353-64).

Viner's lectures range widely in time, from the Greeks and Romans through the Bible to the nineteenth century, with special emphasis on the seventeenth and eighteenth centuries. The first, "The Cosmic Order in the Service of Man," presents the historical background to the main theme, chiefly from Christian sources, and concludes that optimistic providentialism had its roots in the Enlightenment and the secularization of religious thought, rather than in traditional Christian orthodoxy. In discussing "The Providential Elements in the Com-

merce of Nations" Viner concentrated on the concept of a providential relative abundance of necessities as compared with luxuries, and on the role of trade as a means of promoting the universal brotherhood of man. Lecture three, "The Invisible Hand and Economic Man," deals with secularized ethics and its applications to socio-economic issues, while the final address, on "The Providential Origin of Social Inequality," considers both the defense of providence in creating inequality and the justification of inequality in providential terms.

Although in his opening and closing remarks Viner adopted an austere view of the role and responsibilities of the historian of ideas, his lectures are characteristically urbane, witty, and erudite. While deliberately eschewing technical economic analysis he nevertheless offered illuminating comments on the intellectual background of modern economics. One can, of course, cavil at some of his opaque, if carefully worded, generalizations—for example, his insistence on the unity of "expressed opinion" in British social thought from 1660 to 1776 "with respect to general social policy bearing on class stratification, the rights and duties of the poor, the proper location of political power, the functions and limitations of public alms and private charity" (p. 96). But such reservations are far outweighed by a deep sense of gratitude for this reminder of a splendid scholarly life. And is it not fitting to conclude by urging his Princeton colleagues to mine more treasures from the rich store that Viner bequeathed to us?

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*Household and Family in Past Time: Comparative Studies in the Size and Structure of the Domestic Group over the Last Three Centuries in England, France, Serbia, Japan and Colonial North America, with Further Materials from Western Europe.* Edited, with an analytic introduction on the history of the family, by PETER LASLETT, with the assistance of RICHARD WALL. (A publication of the Cambridge Group for the History of Population and Social Structure.) New York: Cambridge University Press. 1972. Pp. xii, 623. \$37.50.

This important volume will doubtless be discussed for years to come. Earlier studies from the Cambridge Group for the History of Population and Social Structure have already lifted our understanding of the demographic history of England to a new plane. Now Peter Laslett and nearly two dozen other scholars from universities around the world have gathered together masses of new evidence on the nature of the domestic group in England over the past three centuries and have complemented it with comparable material from France, Japan, Serbia, Italy, Belgium, Holland, and North America. The bulk of the book is given to the presentation of this data case by case, with chapters on "Household and Family in Tuscany in 1427," "Size and Structure of Households in a French Village Between 1836 and 1861," "Rhode Island Family Structure: 1875 and 1960," and the like. But there is as well a heroic effort at integration and systematic cross-cultural comparison in the ninety-page introductory essay by Peter Laslett.

I cannot pretend to have fully digested this formidable enterprise. I am not an expert in the techniques of historical demography, have no firsthand acquaintance with the sources utilized by the authors, and lack even a thorough grasp of the abundant literature on the subject. But there may be some value in approaching this volume from the vantage point of a social historian who is more a consumer than a producer of demographic history. What can such an innocent learn from this book about the household and family in past time?

I found myself alternately excited, bored, and puzzled. The boredom came in a few papers that offered statistical data without general informing ideas—useful perhaps to specialists in the field but deadly to the outsider. More than offsetting these in number are some

excellent essays that utilize microscopic data to suggest interesting interconnections between family and society. With characteristic grace and lucidity, John Demos draws out the psychological implications of demographic materials from colonial New England. The anthropologist Robert J. Smith presents fascinating findings on the residential instability of households in eighteenth- and nineteenth-century Japanese cities and links them to characteristics of the local political system. Another anthropologist, E. A. Hammel, provides a penetrating look at the *zadruga*, the classic extended family of the Balkans, warning that the institution was "not a thing but a process" and that it displayed impressive continuity over time despite wide fluctuations in the simple statistical indicators commonly used to measure its vitality. Several papers—those by Klapisch on fifteenth-century Tuscany, van der Woude on seventeenth- and eighteenth-century Holland, Greven on eighteenth-century America, and Hayami and Uchida on Tokugawa Japan—point up interesting variations in family patterns related to social class, urbanization, modes of agriculture, and other factors. It is clear from contributions like these how much the social historian can learn from a closer look at the family.

The backbone of the book, however, is Peter Laslett's introduction, "The History of the Family." This major essay is bold, challenging, and elegantly written. It sharply assesses the current state of the field and offers shrewd comments about such issues as the relationship between societal belief systems concerning the family and actual behavior patterns. But its central core left me dissatisfied and somewhat baffled.

Laslett presents a classification system that can be employed to provide strictly comparable measures of household size and structure over time and space and tries to demonstrate the value of that system in a comparative analysis of five areas—England, France, Serbia, Japan, and colonial America. Two large questions are posed. First, how large was the typical household, defined strictly as people living together under one roof as a "coresident domestic group," across a broad temporal and cultural spectrum? Were families characteristically much larger in preindustrial societies of the

past, and did they shrink in the course of modernization? Second, how was the household structured in these various settings? Were complex, extended, or multiple family groupings once the norm? What circumstances created the simple nuclear family pattern dominant in the advanced societies of our own day?

For England since the late sixteenth century, Laslett's answers are both convincing and startling (though anticipated, of course, in earlier publications from the Cambridge Group). On the basis of data from a sample of one hundred English communities between 1564 and 1821 and subsequent census figures, he demonstrates that the mean household size was relatively small (less than five persons) and astonishingly constant until the very end of the nineteenth century. After that the average size of the English family decreased substantially, but the change came so late that the usual explanations—"urbanization" or "industrialization"—are obviously inadequate. Likewise, the simple nuclear family was not the product of modernization. It was practically universal in Elizabethan England, as much so as in the reign of Elizabeth II more than three centuries later. The only deviation from this pattern, and a quite unexpected one, was during the early phases of the Industrial Revolution when there was an abrupt but temporary rise in the proportion of complex households in both urban and rural areas. These results are thoroughly persuasive, and they demolish much conventional wisdom concerning the corrosive, atomizing, individualizing thrust of historical development.

It is Laslett's effort to set these findings into a broader comparative context that proves troubling. The key cases considered here are Bristol, Rhode Island, in 1689, a Japanese fishing village in 1713, Belgrade in 1733, and a northern French village in 1778, with sporadic references to roughly comparable data from other communities. The chief conclusions drawn from this exercise are that, with respect to family size, the English, Japanese, and French patterns look much the same, while Serbian and American families were significantly larger. As to structure, simple nuclear forms were nearly universal in England, France, and America; more complex families

were the rule in Japan and were a large minority (about a third) of Serbian households.

Just what are we to make of this? What most surprises Laslett, apparently, is that the differences between these societies were not more pronounced. Only Tokugawa Japan stands out as a culture dominated by a complex family tradition, and since it was selected for study precisely because it was expected to exhibit that characteristic, Laslett is impressed that even there households were not unusually large and that not many fewer than half of them were nuclear in form. This brings him to advance "the null hypothesis in the history of the family, which is that the present state of the evidence forces us to assume that its organization was always and invariably nuclear unless the contrary can be proven." It follows from this that "the form of the family" is "not capable of doing all the work which social scientists have seemed to expect it to do." Patterns of family organization will not serve us well as explanatory variables, whether dependent ("industrialization brought the nuclear family") or independent ("the dominance of the nuclear pattern in early-modern England predisposed the society towards industrialization"). A variable, after all, must *vary*. The major attempt at synthesis in a volume aimed at "opening up a new field of enquiry" thus seems to close off more unexplored territory than it opens up. If Laslett is indeed right, further comparative study of the dimensions of family life his classification scheme is designed to measure will be an utter waste of time.

Is he right? Laslett is diffident and tentative, as well he should be. The spectrum of cases analyzed is rather narrow and of limited representativeness. There is a serious methodological question, as the author admits, about the validity of generalizations based upon population listings at a single date. As Hammel notes, families are not things but processes; complex families might embrace only a fraction of a population at any one point and yet be experienced by a majority at certain crucial phases of the life cycle. (For a splendid case study illustrating this point, see Lutz K. Berkner, "The Stem Family and the Developmental Cycle of the Peasant Household: An Eighteenth-Century Austrian Example," *AHR*, 77 [1972]: 398-418.) One wonders, as in all such exercises in

comparison, how much of a difference is enough to make a difference. I myself was struck by the dominance of complex family forms in Japan and by their great importance in Serbia. Both areas seem fundamentally unlike England, France, and North America on this count, and it is not beyond the realm of possibility that this has some bearing upon other distinguishing features of those societies.

On the whole, though, the evidence seems sufficient to raise serious doubt that the simplest quantitative indicators of family patterns in past time—size and degree of complexity—are in themselves potent variables when isolated from other features of the family system. Perhaps this is a very useful thing to know, a valuable reminder that further efforts at comparative analysis of family life will need to be more broadly gauged, more refined, and more subtle. My disappointment with Laslett's synthesis, which will surely be the most widely-read piece in the book, is that it does not take the next step, does not forcefully point out the directions in which progress may be made. Laslett has just returned from an unsettling voyage of discovery, and what is most on his mind is that there is no Northwest Passage. Possibly he is right, but many of the essays in this volume suggest that there are untapped riches beyond the null hypothesis. A preliminary map of the shape of that territory is what we need now.

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*Third International Conference of Economic History, Munich, 1965. Volume 4, Demography and Economy: Papers and Report of Discussion*, edited by D. E. C. EVERSLEY, assisted by JANE S. WILLIAMS. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Congrès et colloques, 10.) Paris: Mouton & Co. 1972. Pp. 268. 48 fr.

The appearance of this collection of papers on demography and economy presented at a conference seven years ago is unlikely to arouse much interest in the field of historical demography, which, like Malthus's unchecked population, has been growing in geometric progression since then. (See the review article by Franklin F. Mendels, "Recent Research in European Historical Demography," *AHR*, 75 [1970]: 1065-73.) The preface to this volume

relates the unhappy publishing history of the venture, and the results are terribly uneven, ranging from arguments without data to data without arguments, with a few substantial articles scattered in between. Fortunately the excellent introduction by Eversley puts the papers into perspective and skillfully extracts the major themes: regional growth patterns, rural industrialization, origins of the labor supply, marriage patterns, fertility, mortality, and migration.

The four papers on Great Britain all cover familiar ground. Wrigley discusses the problems of family reconstitution from the registers of the now-famous parish of Colyton. Drake again makes the important point that marriage ages differed in Ireland and Norway not only according to social class, but also by sex within each class. Farmers married late, but chose young women; cotters married earlier, but chose older wives. Armstrong's paper does little more than review the extensive literature on the relationship between industrialization and population growth in England, but it may be profitably read as a background to Pentland's important article on "Population and Labour Growth in Britain in the Eighteenth Century." Pentland constructs indices of natural increase for seven series of English parish registers and finds that there appear to be roughly twenty-year cycles of population growth throughout the eighteenth century. He argues that these cycles are related to the low proportion of young adults in the population and their response to favorable economic conditions.

There are also four papers on Scandinavia, but Lassen's series of tables illustrating regional population change in Denmark from 1650 to 1960 and Friberg's statistics on population change in a Swedish mining district from 1650 to 1750 consist of little more than presentations of undigested data. Much more useful is Jutikkala's synthesis of a number of recent unpublished studies of migration in Finland during the nineteenth and twentieth centuries. He shows that there was more geographic mobility before the period of modern industrialization in Finland, because of the continual migration of agricultural laborers. One article in the collection that will be of general interest to social historians is Hansen's long summary of his book on the Danish aristocracy from 1470



to 1720. He describes not only the demographic characteristics but also the social composition of this group and makes the interesting case that the decline of the old aristocracy in the seventeenth century can be explained by an occupational shift into military careers, which led to higher mortality and also increased hindrances to marriage.

The rest of the papers are extremely short summaries of previously published or ongoing research dealing with the demographic history of particular nations or regions of Europe. Blaschke (on Saxony) and Deprez (on Flanders) briefly present the results of their well-known work on population growth in two regions of highly developed rural industry. The correlation they find between high regional population density and rural industry in the early modern period is also reported in an interesting piece on Bohemia by Petráňová. Kovacsics discusses the problem of estimating population change in eighteenth-century Hungary from the tax lists and censuses. The group of historians associated with Slicher van Bath summarize the state of their research into the social, demographic, and economic history of various regions of the Netherlands before 1800. Finally, there are brief reports of the four discussion sessions, which add a few scattered ideas and facts. None of the problems discussed in this volume have been solved in the seven years since the papers were first presented, but such an enormous amount of information has been added that it would be unwise to depend on this collection for more than an interesting introduction to some of the questions and for English and French summaries of some research in unfamiliar languages.

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HEINRICH OTTO MEISNER. *Archivalienkunde vom 16. Jahrhundert bis 1918*. Göttingen: Vandenhoeck & Ruprecht. 1969. Pp. 365. DM 28.

HELMUT MATHY. *Die Geschichte des Mainzer Erzkanzlerarchivs 1782-1815: Bestände—Organisation—Verlagerung*. (Recht und Geschichte, number 5.) Wiesbaden: Franz Steiner Verlag GmbH. 1969. Pp. ix, 158. DM 24.

The *Archivalienkunde* by H. O. Meisner is a revised and expanded version of the author's

well-known *Urkunden-und Aktenlehre* (Leipzig, 1950). It is a systematic analysis of the various types of archival materials of the period 1500 to 1918, covering public and private, manuscript, typescript and printed, as well as visual and recorded materials. It is not, however, a general manual on archival science, as it is based overwhelmingly on German, especially Prussian, archives. This is not surprising in view of Meisner's long and distinguished career in German archives administration and training, most recently in the Zentralarchiv in Potsdam. One could characterize this work in general by saying that Meisner has done for German archives what Sir Hilary Jenkinson did for the English with his classic treatise (*A Manual of Archive Administration* [3d ed.; London, 1965]).

The book is divided into two main parts. The first, the more general one, consists of a theoretical and historical discussion of the main categories of archival records. The differences between "Urkunden," "Akten," and "Briefe" are meticulously set forth to a degree of refinement perhaps not always comprehensible to the practitioner in non-German lands. Considerable attention is paid to the history and technique of registry of archival materials and the important role of archives in modern documentation.

The second, more extensive part of the work is devoted to modern diplomatics—more specifically, to the analysis and identification of public documents by the application of three main methods: the systematic, the analytic, and the genetic. The systematic method involves chiefly the utilization of certain style characteristics (for example, use of personal pronouns, rank of sender and receiver) to determine the status of a document. The analytical method consists of an almost microscopic dissection of physical characteristics (such as paper, ink, watermarks, seals) and of internal ones (salutation, titles, dating system, main text, closing formula, and signature). Finally, the genetic method views the document from the point of view of its origin and evolution to its final form. This part of the work, describing the originating process of state documents and the specific administrative agencies and officials involved (again chiefly in Prussia), is no doubt of special interest and help to scholars who wish to work in

German archives (also, I hope, in those located in the German Democratic Republic). The final section consists of a glossary of technical terminology, a field in which Meisner has done pioneering work. There are numerous explanatory notes and a full bibliography.

The little book edited by Helmut Mathy is of particular interest to German archivists and to historians of archival science in general. It consists of a selection of fourteen documents, with a brief introduction by the editor, that shed light on the organizational development, as well as the vicissitudinous career of the Reichsarchiv of the elector of Mainz during the stormy years of the French Revolution. The special importance of this archive derives, of course, from the fact that the archbishop and elector of Mainz held the office of arch-chancellor of the Holy Roman Empire, and his archives were thus the repository of documents that constitute an important source for the whole political history of the Reich (they were incorporated in the Vienna Haus-Hof-und Staatsarchiv during the nineteenth century). The documents presented here contribute within a limited context to our fragmentary knowledge of the Mainz archives, whose full history remains to be written and whose unique contents have been hitherto rather neglected by historians. It contained, for example, original copies of the Golden Bull of 1356, the Augsburg Confession, and the Treaty of Westphalia.

The central document in this book is a lengthy, detailed proposal by the archivist, Johann Baptist Kissel, written in 1781, for the establishment and administration of a separate archives (Reichsarchiv) for these imperial documents, based more or less on the modern provenance principle. The most frequent user, by the way, of the Mainz archives in those days was the famous Swiss historian, Johannes von Müller, to whose home—blissful bygone days!—the desired original documents were dispatched by the obliging archivist.

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ELLIS RIVKIN. *The Shaping of Jewish History: A Radical New Interpretation*. New York: Charles Scribner's Sons. 1971. Pp. xxxi, 256. \$7.95.

Professor Ellis Rivkin of Hebrew Union College here offers a "radical new interpretation" intended to "redesign Jewish—and as a consequence world—history." He claims that Jewish history is unique "in a phenomenal sense." His "operative principle" is "utterly simple: The problems of Jewish history can be understood by means of the unity concept. For most of Jewish history, this concept was the affirmation that God was one and omnipotent. . . . Each successive form of Jewish history represents a solution to problems posed to the idea of unity by changing historical circumstances." Rivkin spells out his interpretation in reference to the standard themes of an Occidental and European conception of the histories of Jews: the Biblical period, Pharisaism and Talmudic Judaism, the medieval West, the rise of capitalism, the emancipation, nationalism, "The Road to Auschwitz: The Disintegration of Nation-State Capitalism," and "The Road from Auschwitz: The Emergence of Global Capitalism." The Jews scarcely appear in these last chapters, which focus upon the cold war.

A book with such a pronounced theological orientation is bound to raise serious problems for the ordinary historian. But the primary one is, does the author exhibit awareness of the intellectual antecedents of his theology? In calling his interpretation "radical" and "new," Rivkin makes one wonder whether he has read Deuteronomy, which posits the same "principle," or any of the subsequent sacred literature of Israel and of Judaism. At the beginning of the modern period in Jewish historiography, a century and a half ago, Immanuel Wolf wrote, "What is this idea that has existed throughout so much of world history? It is the idea of unlimited unity in all. It is contained in the one word YHWH which signifies indeed the living unity of all being in eternity." Wolf fails only to tell us this is "radical" or "new." Rivkin posits such a continuum as "Jewish history" without asking what justifies the treatment as a single continuous entity of the disparate and discrete groups known in various settings as Jews, especially since the traits thought in one place quintessentially Jewish in another in no way were associated with Jews. True, Jewish theology creates the category of "Jewish people," which functions like "church" for Christian theology—and is every bit as abstract and

remote from the data of the churches here and now. But, entirely lacking in theological sophistication and intellectual self-awareness, Rivkin is unable to see the force of these questions.

If he is not much of a theologian, Rivkin fails also to establish a claim to be taken seriously as a historian. This is for simple reasons. First, he passes off as facts a great many questionable or moot propositions. Side-stepping the central issues of historical knowledge, he pays no heed either to problems inhering in sources or, indeed, to whether any sources at all say what he says is so. Second, he resolutely ignores the whole of scholarly literature on each and every point; the book lacks not only footnotes but also a bibliography. The entire apparatus of scholarship is missing. Third, the book is full of fake arguments, for example, "No one really knows, simply because there are no records that tell us how this composite became the Pentateuch. . . . I therefore see no cogent argument against approaching the Pentateuch from a very different angle of vision." I last heard that in junior high school. Fourth, the definition of "Jewish history" omits virtually the whole of the non-European segment, as if it simply never lived. Fifth, Rivkin's grasp of the various histories necessarily included in his account is exceedingly fuzzy, as is understandable. To take one minor instance, he regards the "intellectual world of the Sassanians" as "nonrational and disorderly," and this explicitly is applied to disorder in economic, social, and political terms. But everyone knows the hierarchical and highly orderly theory of Sassanian politics and government; the consequent explanation of the "disorder" of the Babylonian Talmud—which some think very logically put together indeed—is based upon incorrect suppositions. And so it goes. Finally, for a historian the book's florid and verbose style is painful; but I should not condemn theologians to sit through it either. Nearly every page will yield glories such as this: "The downtrodden trade off trust for the psychic reassurance of ideas saturated with fantasies of ultimate liberation. So it was with Christianity, again and again. So it was with Judaism, again and again. So it has been with Marxism, again and again." And so on.

This "radical new interpretation" may have

the redeeming social value of inspiring Reform Jewish Sunday school students, but its pretentiousness, ignorance, and vulgarity of thought and expression hardly commend it for the careful consideration of a less partisan, noncaptive audience.

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PHILIP P. ARGENTI. *The Religious Minorities of Chios: Jews and Roman Catholics*. New York: Cambridge University Press. 1970. Pp. 581. \$22.00.

Exposed to invasions and occupations, it was natural for Chios to acquire minorities. One of them, a small one, was the Jews. They probably settled there first during the great dispersion of the Hellenistic period, but they began to live as a community in the eleventh century, under the Byzantines. The author has related, all through his study, the fate of the Jews of the island to that of the Jewry at large, apparently because of the scanty information at his disposal. It was the Emperor Constantine IX Monomachus who assigned the Jewish community of Chios to the Orthodox monastery, *Nea Moni*, with the obligation that they pay a poll tax, called *kephaletion*. An interesting discussion ensues, expressing various scholarly points of view, on the connection between the Jewish *kephaletion* of the island and that of the Byzantine Empire as a whole. The Byzantine period is the most extensive and the best documented in the book, though in parts it reads like a legal treatise. The lot of the Chios Jews did not improve under Catholic rule, for the Catholic Church pursued a policy of persecution. There were instances, however, when the secular power was not faithful to it. During the Genoese rule of the island (1346–1566), thanks to the support of the *Mahona* (Genoese secular power), the Jewish community prospered, particularly as middlemen for the mastic trade. Under the Ottomans, the Jews fell in the category of *Dimmis* (tolerated infidels) and they were treated like the other non-Moslems. They had to pay *cizya*, a capitation tax. While there has been a confusion between *cizya* and *harac* (land tax)—the latter having changed meanings in the course of time—Argenti is not right in maintaining that "by the Ottoman

period the poll-tax was designated as *kharadj*" (p. 149). The term *cizya* was still used when *harac* was substituting for it. Even Gibb and Bowen, whom the author cites, state that the improper usage of *harac* for *cizya* seems to have been adopted earlier than the nineteenth century, "though not in either annals or official documents" (*Islamic Society and the West*, 1 [1957]: 252). Ottoman rule was not harsh on the Jews and they favored it. It was their pro-Ottoman attitude during the Greek revolt of 1821 that accounted for the wave of anti-Semitism that spread among the Greeks.

The Roman Catholics were the largest minority in Chios. Under the Western powers, they were naturally the most dominant. When the Ottomans occupied the island, on the other hand, they were at a disadvantage, as the pope was outside the sultan's territories, and any contact with the Vatican might be interpreted as political disloyalty. During the period 1566-1695 relations between them and the Orthodox, who were all Greek and constituted the great majority of the population, were surprisingly friendly. There were families composed of Orthodox and Catholic members; in the "mixed churches" there were altars for the two rites; the council of the Chios community was constituted of Orthodox and Catholic *demogerontes* (elders). But these peaceful relations were disturbed at times—the Orthodox resented the proselytizing efforts of the Catholic Church. More frequent were the quarrels among the Catholic religious orders of the island. In 1694, at the time of the war of the Holy League against the Ottomans, Venice occupied Chios. The Catholic population manifested its sympathies and assisted her. In 1695 the Venetians were compelled to leave the island. With them a substantial number of the Catholics departed and settled in Venetian Morea and the Ionian Islands. This reminds one of the "great migration" of the Serbs in 1690, when they followed the retreating Austrian armies, whom they had helped during the same war against the Ottomans, and settled in southern Hungary. After 1695 the number of Catholics of Chios began to dwindle until in the twentieth century it became negligible.

As usual, Argenti's book is well documented. There is even a long section (pp. 373-537) containing whole documents. He is also objective.

But the treatment of the subject is uneven and there is not much coherence—it is fragmented. Yet the study throws considerable light on the two religious minorities of his native island and the broader aspects of Jewry and Catholicism.

STAVRO SKENDI

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O. EDMUND CLUBB. *China & Russia: The "Great Game."* (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1971. Pp. xii, 578. \$12.95.

This work—a reference library packed into one volume—is crammed with events, names, and dates covering some 750 years of Chinese, Russian, and Central Asian history. Clubb's knowledge and research of so broad an area is admirable. Yet while working oneself through more than 550 pages of text and notes—and real work it is—the thought occurs whether it is feasible to cover so many centuries and so vast an area in one volume. I believe not, despite Clubb's heroic effort. Some aspects should have been sacrificed; this book, valuable as it is for historians and political scientists alike, lacks analysis in depth, especially of the modern and the contemporary eras. The task of a historian goes beyond that of a chronicler. He must explain at length the meaning of dates and characters during a specific stage of history in terms of the main topic: first, relations between China and Russia, and later on, Sino-Soviet relations. Clubb sometimes attempts to do so, but whenever he has valuable insights, more often than not he buries them under an avalanche of facts.

No reviewer, try as he may, can be absolutely objective. He can perhaps suppress his predilections but not his experience. On this basis, I cannot agree with some of the author's views. For example, Clubb should have been rougher on Chiang Kai-shek's behavior toward General Stilwell and far rougher on Patrick Hurley and the policy makers whom he duped in 1942. I thought that Barbara Tuchman's book on Stilwell had a more realistic approach. I mention this because the mistakes made by both Chiang and Washington had dire consequences. Hurley's lack of comprehension, in particular, prevented us from an earlier understanding of the Chinese Communists. Whether the men in Peking should be "viewed as communists—but

first as Chinese" needs far more analysis than Clubb has space for. A similar question once was posed with regard to Poland: was Gomulka first a Pole and then a Communist? I submit that Mao and Gomulka are both Nationalist and Communist but that their lifelong dedication to Marxism-Leninism and their revolutionary experience would influence their decision. Nevertheless, if the leaders of a Communist state find that their national interests have to be safeguarded (as happened increasingly during the sixties and early seventies), the founding fathers of communism may color their approaches but no longer decisively direct their policies—at least temporarily. I doubt that Mao's 1957 epigram "the eastwind is prevailing over the westwind" has any meaning left at all. It needed only fifteen years for this slogan to deteriorate and presumably be buried in the graveyard of discarded Communist theses.

Perhaps Clubb's brief epilogue ("Chinese, Russians and Americans") contains the most fruitful pages in terms of a contemporary estimate of world politics. It is evident that Clubb is more familiar with China than with the Soviet Union, but his outline of the great power chess game being played by Washington, Moscow, Peking, Tokyo, and New Delhi is interestingly put. I wonder how he would interpret the American rapprochement with Moscow and Peking, which occurred after he had completed his manuscript.

To recapitulate: the book is a mine of information for researchers and scholars in various fields of study throughout Eurasia. There is an overflow of material on countries, personalities, dates, and politico-military developments. The growth of China and Russia from relatively small boundaries to their present immense size could have been pictured more clearly without the ballast of too many details. The analysis of developments between 1223 and 1970 is too scanty, but perhaps understandable in a work of such scope. Clubb's command of English is as spectacular as the wealth of his research. All in all, this is an indispensable book for libraries, historians, and international affairs specialists.

K. L. LONDON

*Institute for Sino-Soviet Studies,  
George Washington University*

MARK MANCALL. *Russia and China: Their Diplomatic Relations to 1728*. (Harvard East Asian Series 61.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xii, 396. \$12.00.

The book's jacket, foreword, introduction, and earlier reviews all emphasize the relevance of this study to a contemporary understanding of the problems involved in Sino-Soviet and Sino-Western relations. This question of relevance is currently rather fashionable, but is beginning to become tiresome. Needless to say, books that assist in understanding Chinese thought and action are extremely useful in this period of the Brezhnev doctrine and the Nixon détente. What should be stressed as important about this volume, however, is that Professor Mancall has produced a first-class piece of historical writing. It needs no further explanation, justification, or relevance.

This work deals with Sino-Russian contacts and relations from the mid-seventeenth century when Cossack explorers first arrived along the Amur River to the year 1727 when the treaty of Kyakhta was signed. Emphasis naturally is placed on the background negotiations leading to the compromise settlement of problems in the Nerchinsk and Kyakhta treaties, and on the semicommercial, semidiplomatic Russian caravans sent to Peking during the interim period, 1689-1727. The point is clearly made that while Russians regarded their contacts with China primarily as a commercial venture, the Manchu government was mainly concerned with fitting the Russians into what is commonly known as the traditional tribute system by which China dealt with, and controlled, the peoples on its inner-Asian frontiers. Thus from the earliest stages, relations between the two empires were bedeviled by conflicting and mutually incomprehensible East-West, Confucian-Christian thought patterns. That the Russians adapted themselves to China's system of barbarian relations is testimony both to their desire for trade and profits and to the Manchu's position of political and military strength vis-à-vis Russia's position of weakness along the Amur.

In an epilogue, Professor Mancall shows that in the nineteenth century, when new problems arose in Sino-Russian relations after over a century of stability, the situation was not simply a case of Russian strength and aggressiveness ver-

sus Chinese weakness. An entirely new and different European outlook on international relations had developed. No longer could the civilized European adapt to China's world view, but instead China must be forced to adapt to the system of relations between nation-states created in late eighteenth- and early nineteenth-century Europe. China's inability to so adapt brought it to the brink of almost complete disaster.

That this study is based on extensive, multilingual research is indicated by a detailed bibliography and no less than sixty-one pages of footnote references. An appendix of Russian-Chinese treaties, a glossary of Chinese-Manchu terms, and an adequate index absorb another forty pages to complement a somewhat slim text of 276 pages. Three maps are provided, but are of dubious value.

As so often happens when one finishes reading a well-written history book, there is an ambivalent feeling of satisfaction and deprivation. What Professor Mancall has provided is good, but with all the research done, the reader wishes that more had been said. Numerous areas could have been profitably expanded. The pivotal role of the Mongols in influencing both the Russian and Manchu positions at Nerchinsk is but one topic where more information would have been welcomed. Nevertheless, this study will undoubtedly remain as a standard reference for many years to come, and is highly recommended to all students of Russia's tortuous relations with the Middle Kingdom.

JOHN W. STRONG  
Carleton University

S. F. ORESHKOVA. *Russko-turetskie otnosheniia v nachale XVIII v.* [Russian-Turkish Relations at the Beginning of the 18th Century]. (Akademiia Nauk SSSR, Institut Vostokovedeniia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 203.

Svetlana Oreshkova has avoided upsetting Russian national sensitivities and tendencies for hero worship in her book on Peter the Great's near-disaster, the Pruth campaign of 1710-11. Following the Russian triumph over the Swedes at Poltava, Charles XII sought refuge on Turkish territory. His presence there caused the stationing of Russian forces in Poland to

interdict the escape route, encouraged French diplomatic pressures upon Constantinople, and incited the Crimean Tatars in favor of a war of revenge—the chief factors in the Ottoman decision to commence hostilities with Russia. Peter then copied the mistake of Charles XII by launching a small, inadequately supplied army, with himself in it, deep into Turkish Balkan territory, where it was quickly surrounded and defeated. Unlike Charles, however, the tsar could not escape and was forced to sue for peace.

This book concentrates on relating and analyzing the complicated diplomatic relations before, during, and after the campaign up to the signing of a new peace in 1713. On the whole it is a valuable contribution to an understanding of previously neglected aspects of international relations during the War of the Spanish Succession and the Great Northern War. Utilizing most of the available Russian and Turkish published sources, Oreshkova emphasizes the role of the French in creating a "diversion" for the benefit of their defeated Swedish ally. She neglects, on the other hand, a thorough treatment of the border nations, especially Ukrainian Cossacks and Crimean Tatars, who were also involved in the conflict.

Peter the Great is strangely and artificially relegated to the background, while Charles XII and the Turkish officials debate strategy and the Russian generals make the best they can of a bad situation. According to Oreshkova, conflicts between Turkish, Swedish, and Tatar objectives and the precarious circumstances of Ottoman mobilization and supply made it possible for the Russians to come out with better terms than might have been expected. She, perhaps too readily, dismisses the influence of Russian bribes on the Ottoman leaders.

With an emphasis on diplomatic detail, Oreshkova's work is difficult to compare with the only book in English devoted to the subject, B. H. Sumner's *Peter the Great and the Ottoman Empire* (1949). While Sumner emphasizes the personal role of Peter and how he was influenced by background situations in the area, particularly by the problems of Balkan Christians, Oreshkova practically ignores religious and nationality issues.

NORMAN E. SAUL  
University of Kansas

ALAN LLOYD. *The King Who Lost America: A Portrait of the Life and Times of George III.* Garden City, N.Y.: Doubleday and Company. 1971. Pp. x, 369. \$7.95.

STANLEY AYLING. *George the Third.* New York: Alfred A. Knopf. 1972. Pp. 510. \$12.50.

JOHN BROOKE. *King George III.* With a foreword by H.R.H. THE PRINCE OF WALES. New York: McGraw-Hill Book Company. 1972. Pp. xix, 411. \$12.50.

The bicentennial tide begins to run on the eastern side of the Atlantic. Here are three English biographies of George III, America's last crowned monarch who must always occupy a central position in our national history and myth. Each study shows the marked influence of Sir Lewis Namier and provides a sympathetic interpretation of the king whose reign from 1760 to 1820 comprised much more, we are reminded, than the American secession from the First British Empire.

Mr. Lloyd's book does not pretend to scholarship. It is a "portrait of the life and times of George III; a highly entertaining portrait of the rather endearing prig who lost the colonies." It need detain us no longer.

Messrs. Ayling and Brooke have a legitimate and strong claim on our attention. Neither has produced a "definitive biography, but both have written useful and important books. Mr. Brooke, senior editor of the Royal Commission on Historical Manuscripts and Namier's former student and collaborator, contributes a volume, embellished with mild grace by Charles, Prince of Wales, to the McGraw-Hill American Revolution Bicentennial Program. Mr. Ayling, too, is an experienced student of the eighteenth century and has a professional command of the sources. His use of secondary works would appear to surpass Mr. Brooke's. Although both interpretations are in close agreement, Brooke explores constitutional and political problems in greater depth; Mr. Ayling provides a better treatment of the king as family man, patron, and book collector. Each accepts porphyria as the cause of the king's mental derangement. Mr. Ayling must be allowed the edge in style since he avoids both far-fetched parallels with twentieth-century events and personalities, and the temptation to pontificate.

Forty years ago Namier's pioneer studies in-

dicated the need for a systematic re-evaluation of King George III; but, while many aspects of eighteenth-century British politics have subsequently undergone Namierite revision, the king has remained in the garish and unflattering light shed by the Whig historians. The *mise en scène* arranged by these writers contains elements from malicious and unreliable Horace Walpole; from Scot-hating and self-loving John Wilkes and his friends; from Edmund Burke's faulty and obsessive perception of a double ministry—one legitimate, public, and dominated, of course, by his friends the Old Whigs, the other secret, irresponsible, and dedicated to royal absolutism; from the celebrated indictment in the Declaration of Independence; from Charles Fox whose personal antipathy for the king was unbounded and unending. Here are the sources of the informing spirit of works by Macaulay, Lecky, Trevelyan, and the countless multitudes who followed where they led. In 1890 a kind of historical dogma was proclaimed with the king's entry in the *Dictionary of National Biography*. Excoriated by apologists for the American Revolution, the luckless monarch also receives the lash from imperialists who punish him for having lost the colonies. George III has indeed received the worst of several worlds.

Brooke and Ayling at last give the king much justice. Drawing deeply on original sources, they succeed in overturning the legend of the obtuse, mulish (the word is Jefferson's) absolutist foiled, to his country's salvation, by American revolutionaries and his own timely insanity. The facts adduced are clear: the prince received no Tory education from his mother, Bute, or anybody else, least of all from his impeccably whiggish but hopelessly inept tutors and preceptors. Brooke's analysis of George's schoolboy essays puts the matter out of court. The king aimed at no restoration of Stuart monarchy. His pattern was William III. He planned no deep change in Britain's political system, domestic or imperial, although changes, *malgré lui*, were at work. What he inherited, he venerated and strove mightily to preserve. A conservative, he aimed to enslave no man; but he himself was obsessed with the fear that unscrupulous men aimed to dominate him and thus to subvert balanced, constitutional government. Brave, honorable, and reli-

gious as a private person, George III personified those virtues most highly prized by the English middle class which came to revere and to love him. Despising the social *haute monde*, where his son and heir was in his swinish element, this king was the most whiggish and decent of the Hanoverians. So much Brooke and Ayling have put beyond serious question; theirs is no mean accomplishment.

If they share equal credit, they also bear equal fault. Demolition is more complete than reconstruction. Neither replaces satisfactorily the interpretation of the Whigs. The obverse of the coin is displayed, but the figure is idealized and in profile. We have yet to see the king in the round; while earlier writers may have magnified the royal blemishes beyond reason, they tend here to disappear altogether. No manner of historical rationalization can conceal the fact that many of the "discontents" of the king's long reign arose from a remarkable inability to adapt to change, and some of them at any rate from actions that can only be judged manifest violations of established constitutional doctrine.

These books are nonetheless stout blows struck in a worthy cause. One is tempted to employ Mr. Brooke's favorite literary device, however, to say that they mark the end of a long *Sitzkrieg*. Many bloody campaigns remain to be fought. It should be an exciting war.

CHARLES R. RITCHESON

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RAPHAEL MAHLER. *A History of Modern Jewry, 1780-1815*. New York: Schocken Books. 1971. Pp. xxiii, 742. \$15.00.

Students of modern Jewish history will welcome the appearance of this volume, a translation and condensation of the four volumes of the monumental Hebrew original. It makes available to a wide circle of English readers the first part of the culminating work of a distinguished representative of the school of Jewish historians in interwar Poland. At the same time, this volume and the ones to follow will go a long way toward filling the great need for a re-evaluation of modern Jewish history as a whole, a field that has been rather poorly served, despite a wealth of new developments and new knowledge since the days of Dubnow and Graetz.

Professor Mahler's history is impressive in its scope and erudition. It deals in rich detail and often with profound insight not only with every major and several minor European Jewish communities in this period, but with the United States and Palestine as well. More important still, a significant place in each chapter is devoted to a systematic analysis of the economic life of particular communities, including their social structure and the class tensions and struggles within them. I know of no other recent work (except Professor Mahler's own two volumes in Yiddish, *Jews in 18th Century Poland in the Light of Statistics* [1958]) where such a wealth of documentation is to be found.

Intellectual developments are not neglected, and perhaps the most interesting chapter of all is that devoted to Hasidism and its opponents in Eastern Europe. This is not unfamiliar ground, to be sure, but it is luminously treated here with rare sensitivity and insight. In general the vitality of East European Jewry emerges in all its vigor and color from Professor Mahler's pages. Polish Jews were secure in their identity despite their precarious and often desperate economic and political condition (which non-Jewish "reformers" were constantly making worse in the name of making it better). By contrast the fallen state of Western and Central European Jewry makes painful reading. Under the Old Regime the cultural unity of Ashkenazic or Yiddish-speaking Jewry was largely unbroken from the Seine to the Dnieper. But precisely in the relatively advanced countries like France and Germany the legal and economic status of the Jews was particularly low and distorted, and their cultural life (except briefly in Prussia) was more barren than in the East. Professor Mahler makes clear the devastating impact on these communities of the revolutionary changes of the late eighteenth and early nineteenth centuries, as Jews struggled awkwardly to integrate into a new society that (in theory at least) welcomed them as men, but rejected them as Jews. Professor Mahler's condemnation of assimilationists like the members of the Assembly of Notables and the *Grand Sanhedrin* in Napoleonic France is perhaps unduly harsh, but the inevitable decline of traditional Jewish life in the age of nationalism and the nation-state is poignantly evoked and in general handled with understanding.



It is no easy task to write Jewish history in all its diversity and to strike a balance between internal developments and a welter of external influences. But Professor Mahler has succeeded magnificently. One might regret the unaccountably sketchy treatment of British Jewry or question whether nearly one hundred pages on Palestine and early schemes for the restoration of a Jewish state there are really justified. In particular the elimination (no doubt for reasons of cost) from the otherwise full bibliography of all books and articles in Hebrew and Yiddish is to be regretted. But these are relatively minor points. This volume will undoubtedly be the standard work in its field for years to come, and the next volume and its successors will be eagerly awaited. I wish Professor Mahler the strength and long life needed to complete the great task he has begun.

SOLON BEINFELD

Washington University

AKIRA IRIYE. *Pacific Estrangement: Japanese and American Expansion, 1897-1911*. (Harvard Studies in American-East Asian Relations, 2.) Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 290. \$12.00.

This extraordinary book adds a new dimension to our understanding of American-East Asian relations in the late nineteenth and early twentieth centuries. Moving beyond the traditional emphasis on diplomacy, politics, and strategy, Akira Iriye places the encounter between Japan and the United States in a psychological and cultural framework. The result is a subtle, penetrating study that analyzes for the first time the complex interaction between Japanese and American expansionism.

Iriye describes the emergence in Japan of a dynamic expansionism, one strain of which emphasized predominance in Korea and spheres of influence in China, another the flow of emigrants and trade across the Pacific. It seemed essential for a vigorous, growing nation to spread its people and culture over a wide area of the globe. Peaceful expansion would relieve population pressures and enrich the nation through the creation of Japanese colonies abroad. Many Japanese believed that, unlike the Chinese, their settlers would be welcomed in California, Latin America, and the islands of the Pacific. Japanese expansionist literature

dwelled on California's abundance and mild climate. Expansion on the continent of Asia was essential for national security, but the American Pacific Coast seemed far more promising as an area to settle.

As America's empire spread into the Pacific, tension with Japan increased, fed by a powerful current of domestic racism. By 1905 the specter of a yellow peril pervaded all strata of American society, and, as Iriye shows, affected American policy makers and army and navy strategists. These racial fears gave the Japanese-American crisis over immigration a special meaning for American leaders and endowed their policy with great urgency. Theodore Roosevelt, torn between his fears of estrangement and his hopes for amity, encouraged Japanese expansionists to turn toward continental Asia, where the United States presumably had no vital interests. The Japanese government, stunned by the racial animosity in the United States and unable to grasp its full import, moved reluctantly in this direction, aided by a gradual realization of the opportunities that lay in southern Manchuria.

Parallel with this development, however, came the emergence of a new American interest in China and a belief, transformed into policy under William Howard Taft, that the United States had a special mission in that nation. Japan resisted American attempts to penetrate its sphere of influence, and slowly the dominant image in Japan of a peaceful intermingling of the two peoples gave way to the conviction of a profound antagonism. By 1911 it was difficult for men on both sides of the Pacific to conceive of a harmonious relationship between their two nations. Instead the images of conflict and strife so deeply embedded in the American official and popular imagination became embedded in Japan as well. They would persist and contribute to the inability of both governments to comprehend the dilemmas and aspirations of one another.

Iriye explores with freshness and insight the patterns of estrangement revealed in the writings of American expansionists and the calculations of government leaders. He also opens a door into a fascinating world of Japanese expansionist literature. The relationship of this body of thought to the Japanese ruling oligarchy is not always clear, but Iriye is more inter-

ested in intellectual currents than in the structure of decision making within Japan. Thus his book complements Shumpei Okamoto's fine study, *The Japanese Oligarchy and the Russo-Japanese War* (1971). It is a landmark, transforming the foreign policy of the period by placing it in a broader framework and providing one more example of the vitality of the whole field of American-East Asian relations.

CHARLES E. NEU  
Brown University

R. SH. GANELIN. *Rossiiia i SShA, 1914-1917: Ocherki istorii rusko-amerikanskikh otnoshenii* [Russia and the U.S.A., 1914-1917: Essays on the History of Russian-American Relations]. (Akademiia Nauk SSSR, Institut Istorii SSSR, Leningradskoe Otdelenie.) Leningrad: Izdatel'stvo "Nauka," Leningradskoe Otdelenie. 1969. Pp. 416.

Rafail Ganelin's "essays" on Russia and America in 1914-17 heavily stress financial and economic relations. About one-third of the book covers the period down to the February Revolution; its bulk deals with the Provisional Government.

A detailed and professional study, the book is relatively restrained and matter-of-fact. If at times it seems wooden and petty, it is distinctly less primitive than earlier Soviet treatments of the same period. While Ganelin provides ample detail on the dealings of American promoters, banking representatives, and business agents with Russian officials and companies, he recognizes that, however "incriminating," this aspect does not adequately explain the course of Russo-American relations: political and military circumstances loom large, though with regard to these his treatment is far less adequate.

The author omits various aspects of diplomatic exchanges (e.g., concerning the Far East). He alludes only casually to prior impediments to trade (e.g., Russian anti-Semitism). There are interesting but inadequate glimpses of broader political visions, such as Russian hopes to make the United States a counterweight to Anglo-French influence.

Like others, Ganelin abandons the earlier Soviet formula that Russia had become a "semi-colony" of Western imperialism. But ambiguities remain, such as those regarding the extent of Allied pressure on the Provisional

Government. It also remains unclear in what measure business interests influenced Wilson's and Lansing's policies. Ganelin detects in American diplomacy "the absence of a firm tactical line and of coordinated action" and points up the inherent conflict—and U.S. failure to make clear choices—among military objectives (keeping Russia in the war, once the United States had joined in), politico-ideological goals (forestalling a Bolshevik take-over, even at the price of Russia's leaving the war), and the more obscure plans for American post-war economic penetration. His account reveals a familiar syndrome of American behavior—meddling, naiveté, gullibility, condescension, moralizing, profit making, and missionary zeal. Predictably, he is even less charitable in delineating the Russian protagonists of 1917.

Ganelin cites unpublished documents from Soviet archives, particularly on economic and financial dealings. His use of diplomatic archives is less thorough. He uses standard U.S. sources and relies heavily on a handful of secondary American accounts. But he fails to use a variety of memoirs and monographs that might have amplified or corrected some of his account.

All in all, though far from a definitive study, the book has some interesting and novel material. While its treatment deserves to be taken with skepticism, it is modestly encouraging as an indication of serious Soviet research in a relatively sensitive field.

ALEXANDER DALLIN  
Stanford University

KLAUS SCHWABE. *Deutsche Revolution und Wilson-Frieden: Die amerikanische und deutsche Friedensstrategie zwischen Ideologie und Machtpolitik 1918/19*. Düsseldorf: Droste Verlag. 1971. Pp. 711. DM 87.

The story of the Armistice and of the Versailles Conference has been told in detail before, but this massive *Habilitationsschrift* by an instructor at Freiburg University succeeds in making a distinctive and valuable contribution by narrowing the focus to the field of German-American relations and by assiduously mining hitherto neglected American and German sources.

Schwabe rejects the notion that the "idealist" Wilson was outmaneuvered by "realists" like Clemenceau and Lloyd George and that he

therefore betrayed both Germany and his own principles at Versailles. While conceding that the final settlement did not entirely conform to the president's stated aims, he contends that the modifications were a conscious result of Wilsonian realism and clashed more with the illusions entertained by the Germans and by "Wilsonians" elsewhere than they did with the beliefs of the president himself.

Wilson regarded the territorial settlement with Germany as conforming to the Fourteen Points and resented German attempts to deny this. He clearly believed in German war guilt from the beginning and was never convinced that that nation had been sufficiently reformed. He therefore saw neither injustice nor a deviation from principle in the provision for war crimes trials or the postponement of German membership in the League, and looked askance at the efforts of German spokesmen to defend national honor by denying culpability.

Only on the question of reparations did Wilson consciously retreat, and he did so for the most practical of reasons. Without some compromise on this issue the peace conference would undoubtedly have broken up. To Wilson that would have meant a resounding personal defeat, with catastrophic political repercussions at home. It would also, in all likelihood, have brought more rightist governments to power in Britain and France, led to a resumption of hostilities, and made far more difficult the achievement of the stable world order that Wilson sought. To risk all of this either in the name of abstract principle or in the interest of a defeated enemy was never a realistic alternative, and that fact clearly limited Wilson's room for maneuvering.

The Germans, in their dealings with Wilson, were profoundly mistaken on two counts. They failed to understand that the president's concept of justice included the belief that nations must accept the consequences of their wrongful acts and could not expect to escape these by protestations of innocence or appeals to a morality they had violated. Moreover, they grossly misread the effect of Allied disunity. Far from allowing them to play off America against the French and British, that very disunity forced Wilson, as the man most sincerely committed to peace and order, to make concessions.

Schwabe's analysis emphasizes the pragmatic

character of Wilson's "idealism" and reveals the president as an essentially conservative statesman whose new international order was far more traditional than is often assumed. It also throws serious doubt on recent interpretations that have stressed the anti-Bolshevik element in Wilson's policies. The fear of a "bolshevized" Germany in the heart of Europe seems to have affected the president only for a brief time after the Armistice. Thereafter he consistently failed to respond to German appeals for support on that basis, and he mistrusted the Ebert government, despite its anti-communist character, on the grounds that it contained right-wing and militarist elements.

Schwabe has uncovered a wealth of evidence and has succeeded in delineating the complexity of the situation both for the German government and for the United States. He is particularly good at defining the communications problem and in describing the activities of various middlemen such as McNally, Herron, and Conger on the American side and Hahn and Loeb on the German.

There are some weaknesses. Schwabe at times gets bogged down in details, and his speculations on occasion go substantially beyond what the evidence will support. While thankfully avoiding the use of impenetrable German academese, his attempts to maintain readability are marred by lapses into stylistic cuteness and by his over-reliance on exclamation points to provide emphasis. This is, however, a solid contribution to the literature and deserves the attention of all serious scholars.

MANFRED JONAS  
*Union College*

SIR JOHN WHEELER-BENNETT and ANTHONY NICHOLLS. *The Semblance of Peace: The Political Settlement after the Second World War*. [New York:] St. Martin's Press. 1972. Pp. xiv, 878. \$35.00.

This massive work is a splendid example of history written in the traditional vein and the grand manner. "It has been our aim," say the authors, "to illuminate the origins and significance of the uneasy and interrupted peace which followed the defeat of the Axis powers"; they have chosen as both epigraph and conclusion a somber passage from W. B. Yeats: "Civilization is hooped together, brought / Under a

rule, under the semblance of peace,/ By manifold illusion." As the sonorous prose rolls majestically on, it carries overtones of Shakespearean or even Greek tragedy: in a sequence of scenes exotically staged all the way from Newfoundland to Tehran, the protagonists appear and reappear as embodiments of wisdom or folly, visionary idealism or calculating *Realpolitik*; they seem to be wrestling with destiny under a lowering lead-gray sky, with a hint of mushroom cloud on the horizon. The reader finds himself caught up in the mood and almost unwittingly tempted to imitate the style.

It would be unfair to expect any startling revelations of fact in a book on this subject. The era of transition from hot to cold war has been a historians' battleground for more than a decade, and the available sources have already been combed over in microscopic detail. Indeed, for American New Left historians, explicating the origins of the cold war has become almost a major industry, or a theological testing-ground for separating Truth from Error. Time was when the origins of the 1914 war served that same purpose. Then as now, the revisionists at a certain point carried the day, and their thesis crystallized for a time into a kind of new orthodoxy. Perhaps one should see *The Semblance of Peace* as a major manifesto of counterrevisionism, heralding a new phase in the battle about the cold war.

This is not to say that the authors adopt an openly contentious stance, throwing down the gage of battle to the revisionists and challenging their interpretations point by point. Indeed, they rarely confront the revisionist view directly, but mention it mainly in brief footnote references, rather as though one of Admiral Fisher's dreadnoughts found itself steaming majestically past a PT boat. Nevertheless, they make no bones about their own position. "Between our views and those of the Revisionists there is clearly a stark contrast which is irreconcilable." Soviet foreign policy "was a re-embodiment of those imperial ambitions which in the glory of the Tsarist expansionist period" had led to a series of aggressive acts and wars. By mid-1945 "it was apparent that Stalin's ambitions in Europe and in Asia even surpassed the paranoiac schemings of the most extreme fanatics" of tsarist days. Therefore "the West-

ern Powers were constrained to accept the Soviet challenge and to make a brave and essential response of free men against aggression."

Such a view, bluntly and baldly stated, has become so unfashionable in recent years as to seem almost a novelty (at least in academic circles). No doubt it will be brushed off by many critics as both wrong-headed and terribly dated—as nothing more than an artifact left over from the cold war era itself. Possibly it is. Yet surely it must be taken more seriously than that. The authors have, after all, built their case on exactly the same body of evidence used by the revisionists. We may assume (pending proof to the contrary, anyway) that neither side has consciously warped or twisted that evidence to support a preconceived thesis, and that the contradictory readings represent honest differences of judgment, rooted no doubt in different value-systems or deeply felt patterns of prejudice. The conclusion that seems to emerge is that the available evidence does not establish, beyond the shadow of a doubt, the validity of either the revisionist or the counterrevisionist case.

Such a conclusion is hardly surprising, given the complexity of the historian's enterprise in seeking to reconstruct past events and to assess human motives. What is surprising is that historians of both schools have thought themselves able to arrive at such certainty of judgment in the absence of really dependable evidence about how the world looked from the Kremlin. Even with an assist from Khrushchev, we have precious little basis on which to judge the real motives and intentions of the Stalin crowd. Something can of course be inferred from their actual conduct; but we have nothing remotely like the inside accounts of evolving attitudes and clashes of opinion in London and Washington. Which Western leader or faction at the time guessed right about the aims of the enigmatic figure in Moscow? Does any present-day historian really have much more to go on than Stalin's contemporaries did? This, it seems to me, is the most important flaw in the case constructed by Sir John Wheeler-Bennett and Anthony Nicholls. But it is exactly the same flaw that mars the revisionist case as well. Both sides hypothesize a Stalin (and a Stalinist entourage) that may or may not have ex-

isted in fact. Yet both sides are quick to chastise certain Western statesmen for having done the same thing twenty-five years ago.

Sir John and Mr. Nicholls are especially hard on Franklin Roosevelt for what they see as his naive illusions about Stalin and his self-confident belief that he alone could manage Uncle Joe; they even find a parallel with Chamberlain confronting Hitler. But Roosevelt, as they view him, was not merely naive; he was also a shrewd, calculating political leader whose aim was to superimpose upon the United Nations "an American-Soviet alliance, which should dominate world affairs to the detriment of Britain and France." Roosevelt's abiding suspicion of British aims was shared, the authors contend, by a number of his close associates. Indeed, this theme of British-American rivalry for Stalin's favors recurs throughout much of the book as a kind of counterpoint to the main theme. It is not surprising, therefore, that most of the heroes are British: above all, the tough-minded and farsighted Anthony Eden, portrayed as a master diplomatist and a tireless defender of British and European interests. At one point Eden is applauded for his refusal to budge on an issue just "to suit the parochial prejudices of Mid-Western Americans." Not all of the Americans are fools or villains, but the chief offenders among them are scourged without mercy: notably Hull, Davies, Leahy, and above all James Byrnes. Harry Truman, on the other hand, comes off well; he "personified that combination of fundamental toughness, commonsense and goodness of heart which comprises the average American." Besides, Truman in 1946 put an end to what is described as Byrnes's "whole-hearted appeasement of Moscow," and went on to promulgate the doctrine that "was to save Western Europe . . . from Soviet annexation." If Sir John and his colleague are inclined at times to speak with a John Bullish voice, they do perform a useful service in reminding readers that East-West tensions were not the only ones that shaped the era.

Sir John and Mr. Nicholls also give us masterful accounts of the successive wartime and postwar allied conferences, of the planning and conduct of the war crimes trials, and of the complex negotiations that finally led to peace

treaties with each of the defeated enemy nations except Germany. The story of the Nuremberg Trial is enlivened by Sir John's first-hand notes (he was attached to the British prosecution)—though once again it is the British who quietly triumph when the American Justice Robert Jackson has to be "rescued" from his blunders by Sir David Maxwell Fyfe.

Despite its thorough and meticulous documentation, the book now and again leaves the reader unsatisfied. We are told, for example, that Stalin in July 1944 almost certainly expected to pull Greece into the Soviet orbit. If so, why his consistent hands-off policy in the months that followed? We are told that when Stalin described Chiang Kai-shek in 1945 as China's best hope, he was "playing the greatest game of bluff and deception of even his remarkably devious career." But what if Stalin was, once again, simply ill-informed about China? We are told that the disarray of the Grand Alliance just after V-E Day was caused by "the altered policy of the Soviet Union," yet only a few pages later Churchill is praised for his early awareness of the unchanging nature of Soviet policy. In the chapter on Yalta there is only the sketchiest reference to the Declaration on Liberated Europe, even though some historians have argued that its importance was central both to the Yalta settlement and to the misunderstandings that followed. Nor do the editors really grapple with the argument of some revisionists that Soviet unilateralism in occupied Romania and Bulgaria was only a natural response to the West's cold-shouldering of the Russians in the occupation of Italy. As for the domestic roots of foreign policy, no attempt is made to probe that vast and difficult subject.

A monument, even when flawed, remains a monument. *The Semblance of Peace*, like the Great Pyramid, will surely loom up for many years to come on the horizon of historical scholarship.

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P. J. VATIKIOTIS, editor. *Revolution in the Middle East: And Other Case Studies*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 231. \$10.00.

DAVID C. GORDON. *Self-Determination and History in the Third World*. Princeton: Princeton University Press, 1971. Pp. 219. \$7.50.

More than a decade has passed since third world countries gained their independence, and observers are now anxious to assess the significance of these movements. How much did the struggles for national independence change societies? How truly revolutionary were the policies of leaders like Nasser and Nkrumah? In general it is fair to say that the original enthusiasm that greeted the rise of nationalism and decolonization has given way to disillusionment and disappointment at the progress toward political unity and economic development. Decolonization has not seemed revolutionary. The nationalist movements did not transform their societies. Whereas books in the 1950s were full of praise for third world nationalism, a scant decade later Frantz Fanon's searing neocolonialist critique, *Wretched of the Earth*, was being read as descriptive of the plight of many newly independent states.

Readers will approach the collection of essays on revolution in the Middle East with considerable interest. Such an eminent group of scholars should shed light on the vexed question of whether there have been major revolutions in African and Asian countries, especially during the period of decolonization. But one's hopes are disappointed. To be sure, the essays underscore the prevailing unhappiness about the performance of contemporary Middle Eastern economies and polities. Dr. Roger Owen assesses the economic change that followed political revolution in Egypt, Syria, and Iraq, and while he finds "respectable" economic performance, he feels that fundamental economic and political change has not proceeded rapidly enough. Albert Hourani argues that there was no true revolutionary tradition in the Ottoman Middle East until after British and French withdrawal. Earlier upheavals, which were often called revolutions, did not produce major structural political and social change and sought only a realignment of political forces within the existing framework.

Nevertheless, the reason these essays fail is that they are short and superficial and do not take sufficient account of important works on the French, Russian, and Chinese revolutions that have decisively shaped contemporary ideas

about revolutions. Albert Hourani's use of the *Shorter Oxford English Dictionary* definition of revolution as the "complete overthrow of the established government" neglects the social and economic dimensions of revolutions that are so important in modern scholarship. In a weak and largely unsuccessful effort to create a framework for viewing revolution, the book contains two introductory essays by A. T. Hatto and Bernard Lewis on the semantics of the word "revolution" in the West and Islam. But these statements give no insight into the way contemporary scholars use the word. Would it not have been helpful to have had a historiographical essay on revolution? The best discussion is the longest and most substantial, an analysis of change and continuity in Morocco, Algeria, and Tunisia. Its authors (R. Le Tourneau, M. Flory, and R. Duchac) chart the transformations in these North African societies and offer explanations of the varying patterns. One of their most interesting ideas is that the Algerian revolution began to decelerate when the leadership became fragmented and lost its capacity to mobilize the masses.

David Gordon's volume, *Self-Determination and History in the Third World*, discusses the important subject of the transformation of third world history in the last few decades. The colonial era left a historical legacy in Asia and Africa of inferiority, backwardness, even historylessness. Gordon catalogs the reassertion of the dignity and worth of Asian and African history, an intellectual movement with strong roots among Western scholars as well as in the third world. The purview of the essay is broad; Gordon proposes to consider historical writing about the "Arab states of the Middle East and North Africa, India and Greece to a limited extent, some African states, Turkey, and the United States with respect to the Black Nationalist movement." In fact the scope is much too broad for the author's expertise. Gordon is comfortable dealing with North Africa, and his next to last chapter, "The 'Copernican' Revolution," is the most thoughtful and thoroughly researched. Here he shows how seven major themes of North African history have undergone reinterpretation as part of a movement to decolonize history. In this section the author has command of his sources and knows the views of historians, journalists, and other intellectuals.

The same cannot be said for his excursions into other third world historiography where his knowledge is much more sparse. In dealing with black Africa his only reference to a Nigerian historian is K. O. Dike, admittedly one of the pioneer African historians. But he does not seem to have consulted the writings of other Ghanaian, Nigerian, and African historians. He also comes to grief trying to define historical maturity, arguing that this is revealed when a society learns to view the colonial period "realistically as a period which, with all its bitter memories and frustrations, nevertheless has become a part of [a country's] heritage and has contributed to her modernization." Is it true then, that the colonial experience was the same for everyone? Will the Angolans and the people of Mozambique attain historical maturity only when they see the colonial period as one that brought modernizing benefits? Unfortunately much of the book is full of such quick and superficial judgments.

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IRENE L. GENDZIER. *Frantz Fanon: A Critical Study*. New York: Pantheon Books. 1973. Pp. xvi, 300. \$10.00.

Dr. Gendzier has added significantly, in this work, to the limited body of materials by and about Fanon. The work is extremely well designed and most carefully researched. Furthermore, she has not allowed herself to fall under the mystic spell of the West Indian psychiatrist and the revolutionary philosopher. For the student, she has also provided both a comparison between her findings and views and those of others, such as Caute, Geismar, and Zahar, and a guide for further study. This latter section is an unusual section in such a work.

The work is divided into four sections: "The Search for Roots, 1925-1952," "Toward a Psychology of Colonial Relationships, 1953-1959," "The Militant, 1956-1961," and "The Summing up: *The Wretched of the Earth*." In presenting Fanon's background the author presents Fanon's dilemma between his self-image and the image others had of him. Fanon was intrigued not only by his own attitudes on blackness, but the whites' consciousness of

blackness. Negritude early attracted the attention of Fanon, although in his mature years he saw it a means toward an end and not as a national ideology. The first section closes with a study of Fanon's first work, *Peau noire, masques blanc* (1952), translated into English as *Black Skin, White Masks* in 1967. This book deals largely with Fanon's Martiniquean experience—"the fact of blackness and the education to 'whiteness.'" Fanon deals with the French language as an instrument of assimilation in a most interesting manner.

The second section of the work deals with Fanon the psychiatrist through his experience at St. Albans and his time as "chef de service" at Blida-Janville in Algeria. At Blida, Fanon dealt with Muslims, people of a different cultural background, and found that the psychiatry of France did not solve the problems of his Muslim patients. During his stay at Blida he became enmeshed in the whole subject of colonialism and the rising tension of Algerian-French relations. He identified increasingly with the resistance movement in Algeria.

The third section, the heart of the study, deals with Fanon's years as a militant, 1956-61. This period saw Fanon's development as a political thinker. He was active as a writer for *el Moudjahid* between September 1957 and January 1960. In his writing of this period he concentrated on four main themes: French colonialism in Algeria, the response of the French Left, including the Communist party, to the Algerian question, the position of the FLN on minorities, and relations between developments in Algeria and the Maghrib and Africa.

The last section deals with Fanon's thoughts on violence, the role of the peasants in the revolution of spontaneity against organization, the political parties and the nation, and national culture. A chapter deals with Fanon's death and his influence on the Algerian revolution. Dr. Gendzier deals realistically with Fanon's long-range influence both on Algeria and Africa and the whole spectrum of human development.

This may well be the definitive study, at least for the time, on Frantz Fanon.

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## ANCIENT

WILLIAM A. McDONALD and GEORGE R. RAPP, JR., editors. *The Minnesota Messenia Expedition: Reconstructing a Bronze Age Regional Environment*. Minneapolis: University of Minnesota Press. 1972. Pp. xviii, 338. \$22.50.

In 1939 the late Carl Blegen began excavations at a Mycenaean palace site in the southwest Peloponnesus and immediately discovered clay tablets in sufficient quantity to make possible, in time, the decipherment of the "Linear B" script. Taking advantage of the rich increase in our knowledge that continued excavation at Pylos (so identified by the tablets) and the decipherment of the palace archives provided, William McDonald undertook a study of the region as a whole in the Late Bronze Age (roughly 1600 to 1200 B.C.). As the project grew he realized the need for the specialized knowledge of many disciplines and from all periods of history; he gained the collaboration of a variety of scientists and scholars, sixteen of whom have contributed to the present volume. The result is a pioneering study, unique for any part of Greek history, that provides us with generous information on Messenia from the Early Bronze Age (ca. 3000 B.C.) to the present day. The authors are modest in their claims and tentative in their conclusions. Contents and methods are clearly set out, accompanied by a wealth of excellent plans and figures. A superb job of editing has produced consistency of terminology and ease of cross-reference while leaving disagreements between the contributors clear and instructive.

For the Late Bronze Age there is clear disagreement on the size of the population. McDonald and Rapp believe the agricultural potential of the land was not fully used and that the population was in the vicinity of 50,000 (approximately the same as in the early years of the Ottoman occupation in the sixteenth century and again in the middle of the second Venetian occupation in 1702). Chadwick, working from the Linear B tablets, prefers a figure between 80,000 and 100,000. There is nothing very secure in the arguments adduced for either view, but the evident degree of specialization in crafts and agriculture and the much more doubtful large merchant fleet postulated by the editors would point to a higher rather than a lower figure.

In one respect there is a significant discrepancy between the evidence of botany and that of history and archeology. H. E. Wright, Jr.'s analysis of pollen cores shows much greater cultivation of the olive in the Pylos region in the Dark Age after the fall of Mycenaean civilization than at its height when the prominence of oil is clear. Wright's effort to explain the latter evidence away is unsuccessful, and his suggestion that olive culture was more prominent in a subsistence economy than under the more developed conditions of the palace-controlled period is contrary to historical probability and the analysis of traditional practice by Aschenbrenner in this volume. Either the Carbon-14 date of 870 B.C. for the olive peak needs to be corrected to ca. 1200 B.C., as Wright concedes may be necessary, or the extent of wild olive in the maquis taking over deserted arable land in a period of severe depopulation was greater than he believes.

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H. A. HARRIS. *Sport in Greece and Rome*. (Aspects of Greek and Roman Life.) Ithaca: Cornell University Press. 1972. Pp. 288. \$11.50.

Several excellent books on Greek athletics, including Harris's own *Greek Athletes and Athletics* (1964), have appeared in recent years. Another such work would seem to "send owls to Athens." *Sport in Greece and Rome*, however, includes lesser games and exercises and extends to Roman, Christian, and Byzantine events; much new and valuable material is introduced.

The first section does lightly survey traditional Greek Olympic events and their transfer to Rome. The second section works through every kind of evidence for informal games of ball, hoop-bowling, weight-lifting, and water sports. The third section concentrates on chariot racing, tracing the nature of the sport, its racecourses, and its social and political repercussions. Roman, Christian, and Byzantine aspects are as fully developed as the Greek. An appendix presents the analyses of dreams of athletes described in the *Onirocriticon* of Artemidorus of Daldis. It is a fascinating, normally neglected view of the hopes and fears of athletes and their fellow men in the second century A.D.: "To dream that one sees one's feet burning is bad for everyone alike. It signifies the loss and



destruction of all one's property, especially children and slaves. The only people to whom this dream brings good are runners, when they see it just before competing; they will run like a cat on hot bricks" (pp. 245-46).

Harris has researched an impressive range of well- and little-known ancient literature and inscriptions and has fully exploited information derived from art. The book is strengthened by his liberal use of excellent pictures and well-translated quotations. His narrative is that of an ardent and thoroughly informed sportsman. He knows muscles, equipment, rules, and problems. He can be informative about kicking a ball with the toe, the arch, or the side of the foot, or about different bits for a racehorse. His parallels to modern sports are frequent, although—regrettably for the American reader—chiefly in English terms. His judgments on disputed questions are often independent—for example, the method of deciding the winner of the pentathlon event (pp. 34-35). The historical background is superficial. He can say, in analyzing Rome's power: "the secret lay in the character of the Roman people. . . . At some time early in the first millennium B.C. there must have been a chance combination of genes in this part of Italy which in the course of generations produced the Roman character" (pp. 47-48). But such regrettable historical vagaries never appear in his discussions of athletics.

This account of ancient sports, from childhood games to syndicate betting, makes them seem delightfully contemporary. For its liveliness, its sensible interpretations, and its thorough research, the book should prove definitive. It is a worthy addition to the valuable "Aspects of Greek and Roman Life" series.

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HOMER A. THOMPSON and R. E. WYCHERLEY. *The Agora of Athens: The History, Shape and Uses of an Ancient City Center*. (The Athenian Agora: Results of Excavations Conducted by the American School of Classical Studies at Athens, volume 14.) Princeton: The American School of Classical Studies at Athens. 1972. Pp. xxiii, 257, 112 plates. \$28.00.

The Agora of Athens was the center, actual and symbolic, of one of the two societies that

most profoundly shaped Western civilization and culture; the other was, of course, the Roman Forum. The Acropolis, indeed, represents for most the achievement of fifth-century Athens. Its three major buildings dominate the city visually and spiritually. The Agora had since antiquity been leveled, buried, and built over until the only visible monument stood on its western rise, a temple of Hephaistos (mistakenly called the Theseion), converted into a church of St. George. The excavations have left the Agora a still-confusing area of foundations, now planted attractively as a park. Only a few structures rise above the ground level: three of the six colossal Giants paired with Tritons that were added in the second century A.D. to the façade of a concert hall, or Odeion, built by Augustus's general Agrippa; the Church of the Holy Apostles, dating from the twelfth century, which has been left, and restored, in the southeast corner; and the two-storied portico or stoa given to Athens by Attalus II of Pergamum (159-138 B.C.), which has been re-erected along the eastern side to serve as a museum and excavation house.

Although the Agora evokes a far less visual thrill than does the Acropolis, it should stir the imagination more deeply. In it was concentrated the civic life of Athens: magistrates, council, courts, and, initially, not only popular assemblies but also theatrical productions. About 500 B.C. the assembly was moved, for reasons unknown, to a hill, the Pnyx, some distance to the southeast and plays to a theater on the south slope of the Acropolis. Despite these removals, the Agora remained central to Athenian life. Here walked and talked philosophers from Socrates to Zeno; here were held parades, races, public harangues, and other municipal functions; shops and even houses found a place around the margins down at least to the end of the fourth century.

As an instance of urban design, the Agora, like the Forum, was only gradually given form and monumentality from a haphazard layout of public buildings. Indeed this accompanied the slow loss of freedom that began during the fourth century B.C. Then, perhaps on the initiative of the great treasurer Lycurgus, a complex of two stoas enclosing a rectangular space was erected to frame the Agora to the south and to balance older and less pretentious stoas to the

north. In the third century, as noted, the Stoa of Attalus closed off the east side and balanced the row of older public buildings along the west, beneath the Hephaisteion. Under Augustus, the central area was pre-empted by Agrippa's Odeion. The real end of the history of the Agora may be marked by two events: the visit of Pausanias about A.D. 150, whose description of its then overbuilt condition is fundamental for identifying existing remains; and the sack of Athens by a barbarian tribe, the Heruli, in A.D. 267. After the sack, the remains of destroyed buildings were employed for a wall built from north to south across the eastern side and were thus preserved to assist in the modern restoration. In the fifth century A.D. the central and southern portions of the Agora were covered by an extensive edifice, probably a gymnasium and bath.

Professor Thompson was director of the excavations from 1947 to 1967. Professor Wycherley is author of studies of ancient city design and of the volume of *testimonia* in the Agora reports. From so competent a team might be expected as definitive a study as is here provided. It covers from Neolithic and Bronze Age burials to the Middle Ages. From the archeological evidence—foundations, fragments, and objects—the authors interpret both the history and use of individual buildings and the general development of city design. The concluding chapter on the earlier explorations by German and Greek archeologists and the systematic excavation by the Americans since 1931 well illustrates the progressive clarification of problems, testing of hypotheses, and identification and reconstruction, at least on paper, of buildings. The Agora excavations have been a model of meticulous digging and of careful recording of literally thousands of fragments and objects. This report brings into focus some thirty-five years of detailed work. It is the central volume of the twenty already published or planned. Its scholarship and importance are matched by a clear style, by excellent typography, and by the careful integration into the discussion of plans and of photographs of Mr. Tavlos's models, of remains, and of objects. Bibliographical references are placed in the notes. The text concludes with a concordance for the objects mentioned, an index of authors

cited, and a general index. This outstanding book is essential for the interested tourist, the archeologist, and the historian both of city design and of Athens.

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P. J. RHODES. *The Athenian Boule*. New York: Oxford University Press. 1972. Pp. xv, 351. \$25.50.

This is the only full study in English of the council that, in Aristotle's words, "handles most business of the state, along with the other officers"; another book, by Roger de Laix of Arizona, will soon appear. Rhodes's work is thorough and dispassionate, an excellent creation of Oxford scholarship that, as he says, was helped along the way by Oxford's great school of Greek historians.

Rhodes accepts the ancient opinion that Solon founded a boule that Cleisthenes replaced. I cannot agree with this, but since the putative Solonian boule plays little part in the present treatment, one need not quarrel over it. Again, Rhodes refuses to ascribe the *Athenaion Politeia* to Aristotle—an excessive caution, but a view taken by some other scholars as well. Rhodes's approach is tightly administrative rather than "social." It would be useful to know, if we can, which men served in the boule: was it really a democratic body, or was it composed of the mildly upper-class persons who serve in Congress or Parliament?

There are detailed chapters on membership, legislation, administration, and jurisdiction, this last including an appendix on the punitive powers of the boule. Most of these sections resume all the evidence with great industry and precision. Note, for example, Rhodes's careful discussion of a disputed reading in the *Anonymus Argentinensis*. The most creative aspect of his research is a study and analysis of Athenian public resolutions. Working mainly with the introductory formulas, Rhodes determines which decrees were "probouleumatic" (that is, devised or planned by the boule) and which were "non-probouleumatic" (originating elsewhere, usually in the assembly). His statistical analysis suggests a fairly even division between the two kinds of decrees in the fourth and third centuries (p. 79). This in turn leads to histori-

cal conclusions about the degree to which the assembly took the lead in democratic policy. Other scholars will wish to use the same material or perhaps vary the method.

Naturally some of the classic problems remain, even after Rhodes's judicious consideration. The work of Ephialtes was very important, but confused sources make it almost irrecoverable. Nor can we say precisely when the prytany system, dividing the administrative year into ten sections, was installed. Perhaps nobody can "solve" these puzzles, but everyone interested will study Rhodes's suggestions. The scholarly apparatus—tables and indexes—are well ordered and useful.

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PETER GREEN. *The Shadow of the Parthenon: Studies in Ancient History and Literature*. Berkeley and Los Angeles: University of California Press. 1972. Pp. 288. \$8.50.

This book is a collection of seven essays by a British journalist and novelist, now professor of classics at the University of Texas.

"The Shadow of the Parthenon" is a downgrading of Periclean Athens, as imperialist, jingoistic, financially inept, and ruled by a "quasi-dictator." Here, as Mr. Green plays the curmudgeonly role of Old Oligarch, many readers will feel a certain nostalgia for Thucydides. "Cleo Reviewed" is centrally an attack on such "Tory Romantics" as N. G. L. Hammond, Victor Ehrenberg, and Geoffrey Woodhead, with Werner Jaeger and Gilbert Murray as incidental victims. Lest it be thought that Mr. Green's approach is entirely negative, I record his approval of Milman Parry, E. R. Dodds, and Russell Meiggs. In "Athens and Jerusalem," Mr. Green tilts against theological historicism, as represented by Father D'Arcy and Professor Christopher Dawson. He concludes, "The dear city of Cecrops is not, and cannot be, the City of God." "Myths and Symbols" discusses myth as ritual, symbol, and history, with special reference to Prometheus as a socio-revolutionary symbol. Here the heroes are George Thomson, Eric Havelock, and G. S. Kirk; one of the villains is Professor Kerényi.

"The Individual Voice" is about Archilochus

and Sappho. (There is an appendix arguing for an early birth date for Archilochus, about 716/5 B.C.) Mr. Green compliments Guy Davenport's translation of Archilochus and offers some verse translations of his own, including one of a recently discovered papyrus, which, since about a third is torn away, offers much scope for conjectural emendation: he makes it a dialogue in which Archilochus' fiancée Neobule breaks their engagement. The author has written a novel about Sappho; here he translates several of her less fragmentary poems into verse and presents her admiringly as a wryly self-critical Lesbian, with an infinite capacity for self-surrender and a sharp and crystalline talent. Certain vulgarities here ("group bugery," "no kidding" [as a translation from Sappho], and "is turned on by") suggest an origin for this piece as a lecture to undergraduates. "The First Sicilian Slave War" (135–132 B.C.) sees the slave leader Eunus as modeling himself on Seleucid kings and appealing to a Messianic tradition, thus making a revolution based on two reactionary institutions, monarchy and religion. "Juvenal and His Age" reprints with some additions and deletions the introduction to Mr. Green's Penguin verse translation of the Roman satirist. The chief addition is an attack on H. A. Mason's negative answer to the question, "Is Juvenal a Classic?"; deleted are nine pages on the manuscript tradition.

Mr. Greene is an antiestablishment classicist, a phenomenon sufficiently rare to be, at moments, refreshing. He is also disarming, as when he admits in a footnote, "Greece was not *primarily* a country of economic upheavals, ecstatic irrationals, or anti-totalitarian scientific thinkers." But it is unsettling to read his claim that "it is hard to find a first-class critical study of most major ancient authors." I offer in rebuttal Dover on Aristophanes, Kirk on Homer, Otis on Vergil and Ovid, Quinn on Catullus, and Syme on Tacitus. It is a disservice to a beleaguered field to cry down this kind of serious and exciting scholarship in the name of an often dubious and almost certainly ephemeral "radical rethinking" of classical criticism.

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Madison*

IOANNIS A. VARTSOS. *Athēnaikai Klērouchiai* [Athenian Cleruchies]. ("Athēna": Syngamma periodikon tēs en Athēnais Epistēmōnikēs Hetaireias. Series Diatribōn kai Meletēmātōn, 15.) Athens: M. Pechlivanides. 1972. Pp. 174. 150 D.

The Athenians, in the fifth and fourth centuries, established a number of settlements in new colonial areas and in the territory of recalcitrant allies, but many details of their procedure are obscure. A book of these modest proportions cannot solve all the problems. The author's intention is to decide how many of these settlements can be called *klerouchiai*. Herodotus uses the word *klerouchos* of the settlers at Chalcis (soon after 506 B.C.) and Thucydides of the settlers sent to Lesbos after the revolt in 428. No other fifth-century author uses the term, and the fragmentary inscriptions in which the word occurs (or has been conjecturally restored) create more problems than they solve. Evidence from the fourth century cannot properly be used to explain events of the fifth.

It is argued in this book that the settlements at Salamis (sixth century), Chalcis (506 and later in 446), Eion, Scyros, Andros, Imbros, Lemnos, Naxos, the Chersonese, Eretria, Histiaea, Brea, Samos, Aegina, Potidaea, Lesbos, Scione, and Melos were all cleruchies. The reasons given are that the settlers seem to have remained Athenian citizens, did not pay *phoros*, and were probably taken exclusively from the poorer classes in Athens, and that, in some cases, Plutarch or Diodorus, for what their evidence is worth, called them cleruchs. Did they take their families with them, and what obligations did they assume? How, for example, did their presence on an island compensate for a sharp reduction of the *phoros*, and how did five hundred or a thousand settlers take the place of an earlier population that had been displaced or enslaved, as at Histiaea, Lemnos, Aegina, and Melos? Unless these questions are taken into account, together with others which a moment's reflection will suggest, little is gained by deciding what technical name was given to a settlement—even if the evidence for such a decision were adequate.

These are some of the reasons why one cannot call this book a useful contribution to the study of Athenian policy. It could, however, have been genuinely helpful to students if it

had presented in full the original literary and epigraphic texts on which all discussion must rest, so as to fill in the gaps in Hill-Meiggs, *Sources*, and Meiggs-Lewis, *Greek Historical Inscriptions*. It is regrettable that this was not done; an Athenian publisher cannot complain that it costs much more to print ancient Greek than modern.

LIONEL PEARSON  
Stanford University

EDOUARD WILL. *Le monde grec et l'Orient*. Volume 1, *Le V<sup>e</sup> siècle (510-403)*. (Peuples et civilisations: Histoire générale. Volume 2.) Paris: Presses Universitaires de France. 1972. Pp. 715. 80 fr.

The series of historical studies called "Peuples et Civilisations," intended by its founders to serve as an "histoire générale," boasts volumes by such fine historians as Lefèvre, Baumont, and Renouvin. In the author of this study of the world of classical Greece they have found a peer. The series already contains a study of much the same subject by Pierre Roussel, but it was published in 1928 and is now badly out of date. That book, moreover, tried to cover the period from the Persian Wars to the Roman conquest in a single volume of about 550 pages. In arranging for a replacement the new editor acted with great wisdom. He chose France's premier historian of ancient Greece as the author and allowed him the space needed to do the job well. The present volume is the first of two, breaking at 403 B.C. and running over seven hundred pages. A second volume will carry the story to the Peace of Apamea in 188 B.C.

The book is divided into two parts, the first being a narrative history of the period. The second part, not quite half the volume, is devoted to "Civilisation," containing essays on politics, both structural and theoretical, religion, economics, and society. A splendid service is provided by the bibliographical notes that are attached to each section of the book. Though M. Will makes no claim to completeness, his grasp of the literature is unsurpassed, and each note gives a fine survey of important recent work. The plan and the apparatus of the book, therefore, are good, but the text is even better. Any writer of narrative history

must be aware of the great difficulty in giving an accurate and interesting account of events and, at the same time, making an attempt to analyze their meaning and to set them in the correct perspective. There is also a conflict between the need to tell a simple, clear, and understandable story without neglecting to give attention to conflicts in the sources and competing interpretations by scholars. M. Will has dealt with these problems in a masterful fashion, and the result is an account that is lively, interesting, careful, scholarly, thoughtful, stimulating, and a splendid introduction to the state of scholarly opinion on the history of classical Greece.

Space does not permit the citation of many instances of the book's excellent qualities, but for a brilliant example of detailed narrative combined with analysis and interpretation as well as a sense of the long-range significance of the issues, the reader may consult the section on Cleisthenes and his reforms (pp. 63-76). Here in brief compass M. Will treats an important but ill-documented, difficult, and even treacherous period with clarity and due regard for the problems and uncertainties. Political developments and constitutional technicalities are carefully examined and their interrelationship is intelligently indicated. Each new institution is examined to determine how it compares with the previous constitution, what its function might be in the political situation of the moment, and what its theoretical foundation might be. Finally the section concludes with an evaluation of the reforms from the perspectives of both past and future. M. Will shows how the program, arising as it did from the needs of a particular historical moment, nevertheless built on the lessons learned from Solon and Peisistratus. From the standpoint of the future he is able to emphasize the unique achievement of the Athenians in the time of Cleisthenes.

M. Will has written the best one-volume history of fifth-century Greece there is, graceful, learned and wise. Let us hope that an enterprising publisher will undertake a good English translation to make it available to the larger audience it deserves.

DONALD KAGAN  
Yale University

M. T. W. ARNHEIM. *The Senatorial Aristocracy in the Later Roman Empire*. New York: Oxford University Press. 1972. Pp. xiv, 246, 3 tables. \$18.00.

The purpose of this volume is to analyze the prosopographic composition of the Roman Senate during the era between Diocletian's accession and the death of Theodosius I in order that the Senate's real political and social position may possibly be discovered. As the chapters unfold, we see how "the senatorial aristocracy had considerable political power in the fourth century West, though not in the East, dating from the reign of Constantine the Great, who changed the system of appointments to high state office" (p. 4). Thus the question of the very nature of the generally accepted concept of absolute monarchy is taken under study.

Arnheim competently points out that during the Imperial period the authority of the Senate had steadily lost political influence. And, in the second half of the third century, the emperors began to advance nonsenatorials to the major magistracies without bothering first to make them senators. Diocletian even went so far as to exclude members of the Senate from the higher offices of state and to give these positions to his cronies. He was indeed an autocrat (pp. 39 ff.). But according to Arnheim's prosopographic research, Constantine reversed this trend by returning to the tradition of appointing senatorials to state office—a policy continued by his successors for as long as there was an emperor in the West (pp. 49 ff.). The author reasons that this change of imperial attitude was an attempt by the Christian rulers of the fourth century to pacify and win over an ardently pagan class of considerable landed wealth and local influence in the West. In contrast, the East never had a substantial senatorial class with which the ruler had to be concerned.

Because of Constantine's action, the emperors were forced to give the members of the Senate a greater share in the ruling responsibilities of the West, and these magistrates could use their posts as a lever to increase their wealth and political weight. In general the senatorials had two principle advantages over their non-senator colleagues. "First, they tended to be better endowed with wealth and land, and cir-

cumstances favoured the large landowner. . . . Secondly, being a close-knit intricate network of inter-related families, the aristocracy had ready-made channels for the conduction and distribution of favours and influence within their exclusive group" (pp. 170-71).

For the Roman historian, the work is an excellent example of how prosopographic information can be used in the discovery and development of a historical thesis.

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### MEDIEVAL

ALBERT C. LEIGHTON. *Transport and Communication in Early Medieval Europe, AD 500-1100*. New York: Barnes and Noble. 1972. Pp. 257. \$12.75.

It is easy enough to criticize an author for not having written a book other than the one being reviewed. It is more difficult, within the space allotted, to demonstrate that a book is not what its title claims it to be. This is the problem with Professor Leighton's *Transport and Communication in Early Medieval Europe*. Although the author states in his introduction that "the period from A.D. 500 to A.D. 1100 provides little grist for the scholar's mill," (p. 10) the title indicates that this is precisely the time span of his study. The reader will soon find that much of the evidence cited is drawn from late imperial, or earlier, sources, or from those that postdate the beginning of the twelfth century. Examples of vehicle construction are drawn from the fanciful frescoes at Pompeii, a city that was destroyed in 79 (pp. 77-78); the elder Cato (234-119 B.C.) is cited as an authority on transportation costs (pp. 157-58). At the other end of the chronological scale the curiously irrelevant information is provided that "wheat can often be shipped by rail from Kansas City to Chicago (500 miles) for about three per cent of its value" (p. 164). The extensive and very useful bibliography only confirms that many of the sources on which this study is based are noncontemporary.

One of the major disappointments of *Transport and Communications* is Professor Leighton's failure to assess more fully the efficiency of early medieval transportation. Only in his

concluding chapter has he discussed, and then all too briefly, how quickly individuals and groups of people could get from one place to another. That Julius Caesar traveled 120 miles by wagon in a single day (p. 178), or that the pony express covered the 1,975 miles between St. Joseph, Missouri, and Sacramento, California, in as little as seven days and seventeen hours (p. 184), is but remotely germane to the main subject.

There is another criticism, and this should be directed to the publisher, rather than to the author. A three-page index for a book containing much useful information is valueless. Of the names and places mentioned in this review, none are to be found in the index. This is not a very well organized study, and only an adequate index would make it valuable as a reference work for economic, social, and military historians. Commercial publishers, obviously attempting to cut costs, are by far the worst offenders in this respect.

While Professor Leighton's study falls well short of its promise, it does provide helpful details for any serious study of transportation and communication during the late imperial and early medieval periods.

JOHN BEELER  
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Greensboro*

JEFFREY BURTON RUSSELL. *Witchcraft in the Middle Ages*. Ithaca: Cornell University Press. 1972. Pp. ix, 394. \$15.00.

Russell's most recent book has many of the same virtues as his earlier monograph on *Dissent and Reform in the Early Middle Ages* (1965). He marshals an impressive array of evidence to show how the concept of witchcraft developed from the time of Augustine through 1486, the year that the *Malleus Maleficarum* was published. Russell sets himself the task of commenting on every significant incident and thinker up to 1427 and then summarizes more briefly the voluminous literature of the period 1427-86. As a result of this encyclopedic urge, the chapters occasionally lose focus, but the result is probably the best general delineation of the origins of witchcraft theory now available. Russell is also careful to avoid the old rationalist liberal thesis that the early Middle Ages were skeptical and that witchcraft was "in-

vented" by Scholastics and inquisitors. Instead he shows clearly the extent to which even the *Canon Episcopi* maintained belief in witchcraft and the crucial ways in which medieval heresy contributed to ideas of witchcraft. One can say that without medieval heresy there would never have been European witchcraft apart from evil magic.

But at this point in the argument it seems that Russell strains too hard. He insists that, like heresy, witchcraft was a form of dissent and rebellion against Church and society, that such dissent is understandable in "ages of rapid social change" (which seems to include 1050-1700), and that sorcery as harmful magic must be distinguished from witchcraft as rebellion. Unfortunately it matters a good deal if the rebellion in question was merely imagined by the fearful or actually put into practice. Russell assures us that there is ample evidence that witches did exist, but his proof usually boils down to the fact that they could have existed. Because of this confusion, he often begins telling what the witches did but then shifts subtly to what their accusers said they did. Well aware of this problem, Russell throws up a smokescreen by arguing that in either case the "phenomenon" of witchcraft existed. It is this phenomenon that he describes, and in so doing he documents in detail the origin of each element of witchcraft theory: pact with the devil, sabbath, orgy, devil's mark, flight, magical salves, familiar spirits, etc. In all of these elements there is no focus except for the nebulous notion of heresy. He suggests, as Hansen did seventy years ago, that once these elements had been assembled, the witch frenzy of the sixteenth and seventeenth centuries necessarily followed. Nowhere does Russell suggest that a major reason for the popular fear of witches was not merely their association with heresy but their ability to work harm. He neglects the intimate connection between misfortune and accusations of witchcraft and goes far toward making English witchcraft trials (involving little heresy and often no devil at all) utterly incomprehensible. If Russell argues that these English trials were for sorcery and not for witchcraft, the reader should be aware that he is making a distinction not found in his sources.

Finally, Russell's concern for the various ele-

ments of witchcraft theory proves ineffective in treating the figure of the witch, the kinds of persons accused, and the dynamics of witchcraft trials. His combination of history and homily is most effective, however, in describing man's irrepressible urge to destroy dissent and apparent evil.

H. C. ERIK MIDELFORT  
*University of Virginia*

G. CONSTABLE and B. SMITH, edited and translated with introduction and notes by. *Libellus de diversis ordinibus et professionibus qui sunt in ecclesia*. (Oxford Medieval Texts.) New York: Oxford University Press. 1972. Pp. xxix, 125. \$11.25.

An attentive reader may learn much from this little book about the crisis in the religious orders in northwest Europe during the mid-twelfth century, each of them acutely self-conscious, each with its own idea of its place in Christian society. A solitary surviving manuscript, perhaps the author's own, argues against wide diffusion in the Middle Ages. But since Martène printed it (rather inaccurately) in 1733, students of monasticism have noted it, and with the recent efflorescence of studies on the early Austin canons it has attracted more attention. A handy and emended text is therefore welcome. Professor Constable provides a brief, discerning introduction; Mr. Smith's translation reads well and seldom falters. The editors reasonably reject a commonly favored ascription to Reimbald of Liège (d. 1149) and judiciously renounce the hope of identifying "brother R.," the author, beyond placing him in a canonical house of northeast France or the Netherlands. The author's object is to expose the virtues of the variety of observances and customs of *servi dei* (the words occur regularly in contrast to the lay *plebs dei, christiani, fideles*), instead of proposing one way of religious life in preference to all others. His technique is to draw on Old Testament exemplars and episodes in the life of Christ to show how every sort of religious order has its place. He touches lightly on the dangers to which each sort of life is exposed, but he names none (except in rubrics). He is broad-minded, praising austerity, excusing laxity, seeing virtues in hermits, monks, and secular canons, as well as in the regular canons of whom he is evidently

one. His tone is sober. He deliberately opposes the common polemical literature of the day, where Cistercians slang Cluniacs and canons defend themselves offensively against monks.

G. R. CHENEY  
Corpus Christi College,  
Cambridge

R. IAN JACK. *Medieval Wales*. (The Sources of History: Studies in the Uses of Historical Evidence.) [London:] The Sources of History, in association with Hodder and Stoughton; Ithaca: Cornell University Press. 1972. Pp. 255. Cloth \$9.75, paper \$4.50.

The title of Professor Jack's book may be misleading to those who are not familiar with the series to which it belongs. He has not written a history of medieval Wales but rather a survey of the kinds of evidence available for such a history and the way these have been neglected or used. And he has much to say about the neglect. As he points out in the preface, if this book has any theme at all, it consists of the contrast between Welsh reluctance to write general surveys of their history and its sources and the zealotry of the English. He admirably fulfills his purpose, but it is not certain that his own survey will, as Professor Elton hopes, exhilarate readers by opening the road to unending inquiry. Part of the neglect Professor Jack describes has surely been caused by the painful inadequacy of the sources, and this may well persuade some readers that their investigation might more profitably be pursued in other fields.

On the other hand, there is no doubt that many readers will be inspired, if not by the sources, at least by the dedication and great accomplishment, which Professor Jack meticulously records, of scholars like Sir John Lloyd, Sir Goronwy Edwards, Glanmor Williams, and William Rees. Professor Jack's own survey entitles him to a distinguished place among the growing band of historians who are continuing the traditions of such scholars and building on their foundations. He has spread his net wide and writes about cartography and place names, archeology and numismatics, as well as the more obvious sources. He even finds space for the antiquaries, whose work he admires. In all these varied fields he shows a mastery of his material. He manages to lighten the burden of

such a survey by his capacity to write with lucidity and grace. In spite of his occasional negative attitude, his work will be indispensable for all future students of medieval Wales.

B. WILKINSON  
University of Toronto

M. M. POSTAN. *The Medieval Economy and Society: An Economic History of Britain, 1100-1500*. Berkeley and Los Angeles: University of California Press. 1972. Pp. viii, 261. \$8.75.

Professor Postan has long been known for his authoritative and specialized works on medieval and economic history, and no one has lived more intimately with the sources than he. Now he has written a survey, covering a four-hundred-year period, that brings the state of knowledge on the subject up to date. And yet it is easy reading for the nonspecialist, a work as welcome from his hands as it is surprising. The author has neglected some topics as being outside the terms of his commission; there is, for example, little tie-in with the organs of central government. The treatment of other topics is uneven, but this is because in some cases "text-book knowledge has been greatly outrun by recent researches." His chapter on "Land Use and Technology," for example, is almost twice as long as average.

Professor Postan's conclusions are not unfamiliar to students of economic history, but they are of special interest because they bear the weight of his authority. The Romans and pre-Romans, occupying the best lands, cultivated in rectangular fields using the light plow. The Anglo-Saxons added the second-rate lands of the inland plains. This heavy soil, blanketed by trees and undergrowth, called for a heavy moldboard plow with a cutting knife and iron share—a sod-buster drawn by up to eight or ten oxen. Co-aration was necessary; long and narrow plow strips were produced. Land holdings were dispersed because "the ethics of the common field demanded that every holder should have his share of good and bad land." But if some villagers had plowlands in a great field more outlying than the rest they would be at a disadvantage in making their way, to and fro, from the village crofts at the slow pace of oxen; that, too, in my opinion, would be unfair. Postan believes that lordship and the great estate were features of Anglo-Saxon so-



ciety from the start, thus coming down on the side of Trevor Aston, not Sir Frank Stenton. He defines "common field" as "a communally held and managed field even if the communal control over it was not perfect and all-embracing," thus siding with the majority. Most welcome is the authority with which the author strips off the overburden of manorialism and reveals the village community beneath. It had no legal status and it kept no records (nor needed to). "That in spite of these disabilities," says Postan, "the communal activities of the villagers can so frequently be discerned behind the silences and disguises of manorial documents goes to show that the village commune could be as active and effective a local authority as the manorial organization itself."

Postan estimates that fifty per cent of the product of an average peasant holding went for rent in cash or kind, annual or not, and tithes. Furthermore, at least half of the peasants' holdings were too small to afford sustenance throughout the year for an average family, but there was considerable part-time employment available. Yet the average servile landholder would probably have preferred more land to more freedom.

There is much up-to-date material in Postan's chapter on the wool trade and the textile industry. He believes that trade as a factor in the growth of towns has been overemphasized; freedom from the restrictive rules of feudal law and society was a major factor. The peasant class and the knightly class were both locked in place. Towns were islands of refuge in a sea of feudalism where merchants and artisans could establish a new class of society not comprehended by feudal law and society. The welcome that students will give this book will encourage Professor Postan, it is hoped, to write more on this level of communication.

W. O. AULT  
Boston University

J. R. S. PHILLIPS. *Aymer de Valence, Earl of Pembroke, 1307-1324: Baronial Politics in the Reign of Edward II*. New York: Oxford University Press. 1972. Pp. xvi, 379. \$22.00.

This book, through an examination of the career of Aymer de Valence, earl of Pembroke, presents a new and important interpretation of the troubled politics of the reign of Edward II.

The standard interpretations of the period, those of T. F. Tout and J. Conway Davies, published respectively in 1914 and 1918, both saw Edward's reign primarily in constitutional terms: as a struggle between the king and the magnates over issues of principle, a struggle to which the king responded by building up the power of his household as a kind of inner bastion, a refuge independent of baronial control then being exercised against his will by the magnates over the great public departments of state. In the midst of these struggles the earl of Pembroke, a moderate leader of consummate ability and the one attractive figure amongst a thoroughly nasty crew of otherwise disastrous politicians, in 1317-18 created and led a "Middle Party" of moderate magnates, churchmen, and administrators devoted to the principles of the famous Ordinances of 1311 yet at the same time loyal to the king.

Dr. Phillips now claims that magnate opposition to Edward II, particularly after the death of Piers Gaveston in 1312, was much less united and that personal factors and the craving of many of the magnates for access to the royal patronage played a much greater part than either Tout or Conway Davies were prepared to allow. Moreover the author claims that Pembroke, far from being a man of consummate ability, was a magnate of no more than moderate talents. Pembroke's consistent attitude, already developed in diplomatic and military service to the crown before 1307, and much affected by his close relationship with the royal family, was basically that of one loyal through difficult circumstances to a misfit of a king. Phillips also claims that the famous "Middle Party" theory is based upon inadequate and misinterpreted documentary evidence. The settlement of 1317-18 was, on the contrary, the work of a number of the more experienced members of the royal council (including Pembroke himself), the prelates of the provinces of Canterbury, and two papal envoys from Avignon. This new thesis carries conviction.

This book, however, has serious limitations. The reader will not find in it a full interpretation of Edward II's reign. Narrowly concentrated upon a series of political crises it is not a book which will make easy reading for those not already acquainted with the main outlines

and problems of the period. Thus the Ordinances of 1311, around which a good deal of the main action turns, are never summarized or analyzed. A writer can assume too much knowledge for the comfort of even erudite readers and, by so doing, obscure his own work.

J. R. LANDER

*University of Western Ontario*

JOHN FERGUSON. *English Diplomacy, 1422-1461*. New York: Oxford University Press. 1972. Pp. xxv, 289. \$17.75.

Mr. Ferguson has made a notable contribution to the hitherto quite inadequate study of English diplomacy during the central years of the fifteenth century. This work can fairly claim to be the only extant attempt to survey as a whole the diplomatic history of the reign of Henry VI, and therefore the diplomacy of the concluding years of the Hundred Years' War. The only other modern study of comparable importance is that by Joycelynne G. Dickinson, which was necessarily confined to its subject-title, *The Congress of Arras, 1435* (1955). The present work carries immeasurably forward not only the study of international relations but also our knowledge of the diplomats and diplomatic procedures during the period.

Any scholar who writes diplomatic history is confronted with special problems in deciding on the structure of his book. He must decide whether he is going to unravel the tangled skeins of diplomacy in the sequence in which they necessarily become tangled, that is, chronologically and more or less simultaneously. Exposition by this method is difficult, but it is the only one that presents diplomacy as it really was. The other method, which Mr. Ferguson adopts, is to treat the English diplomacy with each foreign country separately, in one chapter after another. Thus we have seven successive chapters devoted to relations with France, the Spanish kingdoms, the German princes, the Italian city-states, the Baltic, the Empire, and the papacy. This method makes possible treatment of each field of diplomatic effort in greater detail than would otherwise be the case, but it does introduce an element of artificiality into the exposition that is hard for the reader to overcome.

The eighth chapter, on "Law and Practice in Fifteenth Century Diplomacy," is exceedingly

valuable and would perhaps have served even better as a preliminary to the book as a whole than the present rather discursive introduction. The conclusion is interesting but rather too brief.

Appendix 1, spread over some forty pages, provides thirty-two most welcome lists of English envoys treating with a variety of foreign powers and of foreign envoys treating with England. Unfortunately these lists are not themselves listed, so that reference to any particular item is troublesome. Thirteen useful documents are supplied in appendix 2.

Mr. Ferguson has produced a work of fine scholarship, which becomes at once indispensable for the study of the realities of Henry VI's government. As he concludes, "impelled along a zigzag course by the various contending factions in the English Council and by the Duke of Bedford in France, English foreign policy during Henry VI's reign could only achieve the negative success of mitigating adversity."

S. B. CHRIMES

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Cardiff*

PIERRE DEMOLON. *Le village mérovingien de Brebières (VI<sup>e</sup>-VII<sup>e</sup> siècles)*. With a study of the fauna by TH. POULAIN-JOSIEN. Foreword by ERNEST WILL. (Mémoires de la Commission Départementale des Monuments Historiques du Pas-de-Calais, number 24 [1].) Arras: Archives du Pas-de-Calais. 1972. Pp. 338. 50 fr.

Unfortunately, medieval historians have not made as effective or as extensive use of archaeological evidence as have their colleagues in ancient history. This failure is due in large part to the relative abundance of written evidence that survives from the Middle Ages. One might even hazard the suggestion that there is a direct statistical correlation between the survival of written evidence and the neglect of archaeological data. The lower classes in medieval Europe, however, were nonliterate and our written evidence about them comes largely from a clerical elite that was tied by interest and necessity to the upper classes. Therefore it is of great importance for the medievalist who is writing social and economic history, for example, to use the results of archaeological excavations so as to balance the biases evident in the written sources. Pierre Demolon's *Le village*

*Mérovingien de Brebières* is a work that provides an abundance of evidence on subjects for which extant written sources provide little or no help.

Brebières was located five kilometers to the south-southwest of Douai, very close to the modern border between Flanders and Artois. It was only four kilometers from the Mervincian royal villa of Vitry-en-Artois and was probably engaged in helping to provide food for the king and his entourage when they sojourned there. Despite this connection with a royal villa, Brebières was a very poor village and very small. At no time during the sixth and seventh centuries when Brebières existed did it have more than three dozen crude wooden huts, and we may guess that its population never exceeded one hundred and fifty men, women, and children. An exhaustive analysis of the fauna-remains by Th. Poulain-Josien suggests that the villagers had a relatively small meat component (mostly pork) in their diet. Surprisingly, their diet was not supplemented significantly by hunting.

Demolon may perhaps be criticized for not elaborating more on the historical significance of his data. As a piece to technical scholarship, however, Demolon's work is a model of clarity and accuracy. It is a new and highly worthwhile source for a period and for subjects that are poorly documented with written evidence. Works such as this should be encouraged and supported. For any nonspecialist who may harbor a notion that the medieval world was an idyllic age in human history, even a glance at the stark reality of Brebières will be a proper corrective.

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EINHARD. *Vita Karoli Magni: The Life of Charlemagne*. The Latin text with a new English translation, introduction, and notes by EVELYN SCHERABON FIRCHOW and EDWIN H. ZEYDEL. Coral Gables, Fla.: University of Miami Press. 1972. Pp. 144. \$7.95.

A single adjective can describe the book, namely, disappointing. For a translation that purports to be faithful and implies that it has more scholarly pretensions than others, it falls short in both aspects. It translates "tyrannos"

as "rebels," "eorum" as "his," "sacrificii tempore" as "morning mass," "aeternae virginis" as "Immaculate Virgin," "testamenta" as "a will," and "metropolitanæ civitates" as "capital cities," to indicate only a few single and double word combinations.

In a bibliography of thirty-six books and articles, only one British, two American, and four French scholars are listed. The remainder are German, but even so there is no mention of the monumental work on Charlemagne edited by W. Braunsfels. Footnotes are thin, the longest section dealing with items of clothing, but none identifying important quotations from and allusions to Suetonius's *Lives of the Caesars* on which Einhard relied.

Several debatable statements occur in the introduction. Two illustrations will suffice: the apparent conflation of Louis the Pious and Louis the German, and the remark that "in the eighth and ninth centuries marriages did not involve the Church." The best features of the publication are its pictures, maps, and genealogical table, as well as its excellent production. It is, moreover, handy to have a translation and the original Latin on opposite pages.

ALLEN CABANISS  
University of Mississippi

R. D. BARNETT, editor. *The Sephardi Heritage: Essays on the History and Cultural Contribution of the Jews of Spain and Portugal*. Volume 1, *The Jews in Spain and Portugal before and after the Expulsion of 1492*. New York: Ktav Publishing House. 1971. Pp. viii, 640. \$15.00.

This volume contains articles by some of the leading authorities on Sephardic history and civilization: Solomon David Sassoon, "The Spiritual Heritage of the Sephardim"; Francisco Cantera y Burgos, "España medieval: Arqueología"; Cecil Roth, "Illuminated Manuscripts of Mediaeval Hebrew Spain"; Georges Vajda, "La philosophie juive en Espagne"; Jesus-María Millás Vallicrosa, "La ciencia entre los Sefardies hasta su expulsión de España"; Aharon Mirsky, "The Principles of Hebrew Poetry in Spain" (in Yevrit); Federico Pérez Castro, "España y los judíos españoles"; Nahum M. Sarna, "Hebrew and Bible Studies in Mediaeval Spain"; H. J. Zimmels, "The Contributions of the Sephardim to the *Responsa* Literature till the Beginnings of the

16th Century"; Zimmells, "Codifications by the Jews of Spain"; Haim Beinart, "The *Converso* Community in 15th Century Spain"; Beinart, "The *Converso* Community in 16th and 17th Century Spain"; Israel Salvator Révah, "Les marranes portugais et l'inquisition au XVI<sup>e</sup> siècle"; William Samelson, "Romances and Songs of the Sephardim"; Ramón Menéndez Pidal, "El romancero Sefardí: su extraordinario carácter conservador"; Ovadiah Camhy, "La judeo español—facteur de conservation pendant quatre siècles"; and Henry V. Besso, "Judeo-Spanish—Its Growth and Decline." There are excellent summaries of the articles in Castilian and Yevrit, but no summaries of articles written in French.

The editor states that a comprehensive index will be included in volume 2. Without such an index the value of this work for research is reduced. One finds, for example, mention of different aspects of Ibn Gabirol and Nachmanides scattered in almost every article. Had an index been prepared for this volume, perhaps there would have been some agreement as to spelling. I realize that with four languages some inconsistencies may arise, but there should be none within a given language. Moses ben Nachman is rendered as both "Nachmanides" and "Nahmanides." There seems to have been some attempt to make most of the English and Castilian conform to a rule, but the French articles are individualistic. The editors were also unsure whether "ibn-" and "ha-" should be capitalized or whether "ben" and "Rabbi" should be spelled out or written "b." and "R." As it seems that we will be having more multilingual tomes, it might be convenient if at some time an agreement is reached on how to transliterate names and places. If this cannot be done, then the index must have cross-references so that "ibn Shaprut" or "ibn Šaprut" could be found.

As in every collection of articles, there is a wide range of quality. Sassoon's article, digested from an article he published in 1957, is so chauvinistic that it is almost useless. "Few people," Sassoon states, "seem to realize that that famous period in Europe called the Renaissance, which marked the end of the Dark Ages and ushered in the revival of Learning, was directly due to the culture built up in Moorish Spain by the Arabs and Jews in part-

nership." Really? Roth's short study on illuminated manuscripts is most useful, especially the distinction he made between Jewish attitudes toward the Bible and the Haggadah. One could only wish for color plates. Villás Vállcrossa's article fills important gaps found in Sar-ton's work.

Beinart's first article on the *converso* community at the time of expulsion used much of the same data employed by Stephen Haliczer, in "The Castilian Urban Patriciate and the Jewish Expulsions of 1480-92" (*AHR*, 78 [1973]: 35-58), but arrives at an opposite conclusion. While agreeing that the old Spanish *conversos*, who still remained Jews in tradition, could not be absorbed by the Christian community, and that there was considerable hostility between the *converso* and Christian communities, Beinart insists that it was the Crown who initiated the expulsion process as early as 1477. Beinart's position seems the more sound since he presents a picture true for both Castile and the Crowns of Aragon, while Haliczer seems to ignore the Aragonese.

Beinart's second article and the article by Révah bring into focus the attempt of the Sephardic *conversos* to remain in their homeland. Besso examines the Ladino tradition and evaluates its chance of survival. Ladino, like Yiddish, produced a rich literature; it would be a pity if the revival of Hebrew doomed seven hundred years of creativity.

All the other articles are of an excellent caliber. It seems that the articles in this volume are confined to a "cultural" approach. I hope the second volume will consider the political and economic approach. I also hope that the second volume appears soon and with the index.

J. LEE SHNEIDMAN  
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C. A. CHRISTENSEN and HERLUF NIELSEN, editors. *Diplomatarium Danicum*. Third Series, 1340-1412. Volume 7, 1364-1366. Copenhagen: Munksgaard. 1972. Pp. xx, 493. D. kr. 60.85.

By the time with which these documents are concerned Valdemar IV Atterdag of Denmark was at the height of his power. He had, by various means, not only succeeded in reuniting a chaotic country, but he had gathered all the power into his own hands and de-

prived the nobles of the influence they had become used to in years gone by. He had, moreover, taken from them much of the land from which they had derived their former glory, and there are various documents in this collection showing clearly that this process was continuing. It can scarcely be imagined that the numerous estates given to the king after his return from Avignon were merely tokens of good will.

By this time, however, these were minor matters, and there are no signs that the king's authority was questioned anywhere within his realm. With the Hansa it was a different matter, and at the beginning of this period Denmark was at war with the Hanseatic cities. There are many Danish and Hanseatic documents relating to the struggle, and it is easy to follow the process whereby peace was re-established. The various moves are clearly documented, and among the papers is the draft of the Hanseatic peace treaty. Once peace was re-established the question was how to maintain it, and the minor points of irritation are clearly shown.

Others, however, were obviously more confident in Valdemar; this is particularly noticeable in the first third of this volume where there is a lengthy correspondence between him and Pope Urban V, originating from Valdemar's visit to the pope in Avignon. The close relationship between the two is well known, and these papers show clearly how it evolved, with Valdemar on the one hand freely acknowledging the pope's authority, but on the other constantly asking him to fill ecclesiastical posts with Valdemar's own nominees—requests that were usually, but not always, granted. For his part, Urban later sought Valdemar's support for appointments he made to bishoprics—and on one occasion he refused the king's request to remove Bishop Erik of Odense to a see outside the kingdom. While the king was in Avignon there was a peculiar dichotomy between his requests to the pope, many of a purely spiritual nature, and the power politics that were taking place between Denmark and the Hansa in his absence. But perhaps the cynics are right in suggesting that the whole thing was concerned with power politics in any case.

The collection finishes with the rumblings on the Swedish succession that were ultimately

to result in the Scandinavian union under Queen Margrethe.

W. GLYN JONES

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THOMAS CURTIS VAN CLEVE. *The Emperor Frederick II of Hohenstaufen: Immutator Mundi*. New York: Oxford University Press. 1972. Pp. xx, 607. \$19.25.

This book deserves full applause on every count. The reign of Emperor Frederick II covers not only a great many decades but a large number of subjects and regions. The author is equally at home in every one of them and presents a well-rounded picture in readable and intelligible style. Over forty years ago there appeared a monumental work on Frederick II by E. Kantorowicz. It was fully documented and astonished its audience as much by its learning as by the extravagance of its interpretation. Kantorowicz's scholarship made criticism difficult. Every historian felt uneasy, but nobody could quite come to terms with Kantorowicz, whose vision of Frederick owed as much to the imperial romanticism of the German poet Stephan George as to his immense erudition. K. Hampe, doyen of orthodoxy, valiantly tried to digest it in his celebrated article in the *Historische Zeitschrift* ("Das Neueste Lebensbild Kaiser Friedrichs II," 146 [1932]: 441–75). Since then a great deal has been written about Frederick II, but nobody has attempted to reopen the matter on a full scale.

Given this situation, it would have been tempting for Thomas Curtis van Cleve to fall into the trap of debunking. He could have used his own erudition to debunk either Frederick II or Kantorowicz or both. In view of the startling extravagance of Kantorowicz's picture, such a reaction would have been entirely intelligible even though it would not have advanced our understanding of Frederick II. Van Cleve has bravely resisted this temptation. His own picture of Frederick, far from being a simple reaction against Kantorowicz, is a model of judiciousness and balanced understanding. It stands independently on his own re-examination of the sources. This independence gives him the courage to portray Frederick II's staggering originality and enterprise with sympathy and sobriety. There is no attempt to fall over backward in order to disprove Kantoro-

wicz's romantic enthusiasm. The author is to be especially congratulated because the same loving care and understanding are devoted to the legal and military maneuvers as to the cultural achievements.

The one weakness of the book is the author's all too ready acceptance of a number of generalizations about the historical setting. It is not true that the "plans of the Hohenstaufen, as exemplified by Barbarossa, contemplated extending the imperial dominance over the whole of Italy and Sicily . . ." (p. 4); on page 5 the author commits himself without further ado to the view that when Barbarossa agreed to the marriage between his son Henry and Constance of Sicily, he understood and foresaw the eventual dynastic implications. But the author can be excused. Frederick II was a man of "pronounced individuality" (p. 539) and a man of "transcendent superiority" (p. 535). It is therefore legitimate to concentrate on him personally and slide somewhat cavalierly over the precise nature of the place he occupies in history.

PETER MUNZ

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ANTHONY MOLHO. *Florentine Public Finances in the Early Renaissance, 1400-1433*. (Harvard Historical Monographs, 65.) Cambridge, Mass.: Harvard University Press. 1971. Pp. ix, 234. \$10.50.

The fiscal system of the Florentine commune played a central role in both the political and the social history of the Renaissance city. The stream of money it generated enabled Florence to compete successfully with powerful neighbors, such as Milan or the papacy. But the operation of the system also created severe social and political strains within the city, which historians have not yet satisfactorily examined. Professor Molho's book is a pioneering excursion into the hitherto dimly discerned field of Florentine public finance. He concentrates his attention on the years from 1400 to 1433, during which the fiscal system underwent profound adjustments and, at the period's close, experienced a major crisis. The huge archival deposits of the Florentine office administering the public debt (the Monte) are as yet uncataloged, and Molho was unable to make use of

them. But he has made effective use of much other material, both published and unpublished—notably the deliberations of the communal councils, the records of the treasury, and private account books and memoirs. The analysis he develops is not exhaustive, but it is nonetheless original and, for present and future researchers in the same field, invaluable.

The presentation benefits from a clear organization and a vigorous style. The author considers in turn the expenditures of the commune (the costs of war far outweighed all other outlays) and the income derived from direct and indirect taxes and from forced and voluntary loans. He examines the introduction of a new system of assessment, the *catasto*, in 1427 and clearly illustrates its close ties with the older system of forced loans. The effects of high governmental expenditures both on government itself and upon the Florentine economy are also discussed. Finally, Molho is the first to illustrate the depth of the fiscal and economic crisis of 1430-33, which served as the immediate prelude to the establishment of the Medici hegemony at Florence. A suggestive conclusion indicates the points of contact, which Molho deems worthy of further study, between Florentine fiscal policy and the economic, social, and political history of the city.

Although a short study, the book succeeds in illuminating numerous aspects of Florentine fiscal history and in clearing up some points—such as the relationship between *catasto* assessments and forced loans—that have long confused historians. The comments, intended primarily as suggestions, on the relations between fiscal practice, social structure, and political history of Florence are consistently thoughtful and intelligent, and should serve as guides to future research. There is only one area in which a serious criticism can be made concerning the book. The Latin citations from manuscript sources contain numerous impossible readings. Florentine scribes rarely wrote elegantly, but almost always correctly. The mistakes are too many to be dismissed as typographical errors; they indicate that the author did not transcribe or verify his notes with adequate care.

DAVID HERLIHY

Harvard University

PETER CHARANIS. *Studies on the Demography of the Byzantine Empire: Collected Studies*. With a preface by SPEROS VRYONIS JR. London: Variorum Reprints. 1972. Pp. 364.

The republication, in a single volume, of twenty-two of Professor Charanis's articles and book reviews makes some of his important contributions to Byzantine history easily available. The articles assembled here consist of studies on the size and composition of the population of the Byzantine Empire from its beginning until about the thirteenth century. Professor Charanis has done some basic work on the ethnic composition of the Byzantine population. His main argument, which runs throughout these studies, is that the Byzantine Empire was, indisputably, a multinational state that, however, had two all-important connecting elements: Christianity and the Greek language and culture. While the ethnic structure of its population changed—with the large Slavic invasions, the rise in importance of the Armenian element in the ninth century and after, and with other developments—the strength of the Empire lay in incorporating these elements within its cultural traditions. Starting from this eminently sane principle, Professor Charanis can then study the hotly debated topic of the Slavic invasions (articles 2, 7, 10–13, 15–21), subject the few existing sources to a rigorous analysis, and reach valid conclusions. Accepting that there was wide Slavic settlement in Macedonia, Greece, and the western Peloponnesus, he also argues that the Slavs “eventually succumbed and became completely absorbed by the Greek race. They left behind them some Slavonic place names; but their long domination failed to affect materially Greek culture or the Greek language” (article 11, p. 258). While this particular quotation refers specifically to the western Peloponnesus, I think it is fair to say that Professor Charanis considers it applicable to the rest of Greece also (compare article 21, p. 34).

Apart from the studies dealing with the question of Slavic settlements in Greece, perhaps the most important work included in this volume is Professor Charanis's study of the Armenian element in the Byzantine Empire. In one major study (article 5) and some smaller ones, the author has brought to attention the

fact that Armenians were very active in the army, the navy, and the palace in the period of great Byzantine expansion (approximately from the middle of the ninth century to the end of the Macedonian period). Indeed, he refers to the Empire of the ninth and tenth centuries as “Graeco-Armenian” (article 22, p. 115; article 5, pp. 239–40)—that is, Greek in culture and Armenian in terms of defense. At the very least, the use of the term should serve as a reminder that the Byzantine Empire at its height was united not by race but by culture.

The first article republished here approaches the problem of population from a quantitative viewpoint. Demographic information of this kind is very limited, and one can only give approximate estimates of the population of the Empire; such estimates are really more meaningful for the sixth century than for any other period. After the seventh century, the only conclusions one can reach are qualitative and argue for a decline of the population, followed by gradual expansion from the late ninth century until the late eleventh in Asia Minor and the late twelfth in the Balkan provinces. The article does not discuss the thirteenth or fourteenth centuries; some information does exist for this period, and some demographic work is being done on it.

Written between 1946 and 1971, the articles published here all reflect a depth of scholarship and a respect for historical truth that should serve as an inspiration to the younger generation of Byzantinists.

ANGELIKI E. LAIOU  
Brandeis University

SPIRO KOSTOF. *Caves of God: The Monastic Environment of Byzantine Cappadocia*. Cambridge, Mass.: MIT Press. 1972. Pp. xviii, 296. \$18.50.

The author is an architectural historian, and his subject, the cave monasteries in Anatolia, west of Kayseri in Turkey. Christian monks are thought to have settled in this weird tufa landscape as early as the fifth century, though the churches they carved there out of the rock, and painted, date from later times, primarily from the tenth to the thirteenth centuries. The churches often imitate, in both their architecture and painting, the prevailing artistic cur-

rents of Constantinople and are a valuable source for the understanding of Byzantine provincial art.

The rock-cut churches—there are over sixty painted ones and more are being discovered every year—were first published by G. de Jerphanion between 1925 and 1942. Over the past ten years the area has been further explored by two intrepid women, N. Thierry and J. Lafontaine-Dosogne, and by Marcell Restle, whose three-volume publication of *Byzantine Wall Painting in Asia Minor* appeared in English translation in 1967. Professor Kostov's book has the advantage of being the first general survey of the Cappadocian churches in English in one volume. It is divided into three sections: "The Setting," "The Buildings," and "The Paintings," followed by a catalog of the major churches (sixty-six entries) arranged in chronological order.

The book has a number of shortcomings. It suffers from flaws in organization, from inadequate photographs, and from a regrettable unevenness in the editing, whereby whole sections are left in extremely unidiomatic English (especially pp. 114-41). Kostov opts for a chronological, rather than topographical, arrangement within his two main divisions of architecture and painting, but then confuses the reader by, for example, placing his chapter on the "Archaic Phase" of architecture (A.D. 850-950) before his chapter on the "Iconoclastic Phase," though the period of iconoclasm ended in 843. He frequently introduces discussions that properly belong in the painting section into his architectural section, and inserts essays on the fundamentals of Byzantine liturgy and iconography somewhat at random throughout the text; for the interested layman, these are hard to locate and, for the specialist, only serve to disrupt the train of the more technical arguments.

Kostov includes ground plans for most of the churches, but his photographs are of poor quality. Many are out of focus, and details are often only blow-ups of more general views; in fact, the only satisfactory pictures of the frescoes, at least, are those reproduced from Restle's volumes. Kostov's arguments for the dating of some churches, as well as for the sources of certain architectural forms, might have been

strengthened had they been accompanied by illustrations of comparative material.

Nevertheless, this book has valuable qualities. Kostov attempts to place these monasteries in their geographical and cultural context, not limiting his discussion to questions of iconography or style alone. He pays close attention to the architecture of these churches at a time when their paintings are attracting the most interest. He analyzes the various pictorial programs with particular emphasis on the choice of scenes and the relation of the cycles to their architectural framework. In the catalog Kostov gives us not only the modern Turkish name for each church, but also its other names and the earlier spellings found in older reports. The volume concludes with an annotated bibliography of the most important writings on the subject, useful for the beginner and the specialist alike.

NANCY PATTERSON ŠEVČENKO  
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ROBERTO DI CLARI. *La conquista de Costantinopoli (1198-1216)*. Critical study, translation, and notes by ANNA MARIA NADA PATRONE. (Collana storica di fonti e studi, 13.) Genoa: Università di Genova, Istituto di Paleografia e Storia medievale. 1972. Pp. xiii, 294. L. 5,200.

This is the first Italian translation of the chronicle of Robert of Clari, a prime Western source for the Fourth Crusade. The value of Robert's chronicle lies in its occasional differences from other Western accounts and especially in that the author reflects, unlike the semiofficial propagandistic "line" of Villehardouin, the personal views of a modest, obscure, Picard knight. No Western chronicler describes in such detail the marvels of Constantinople: its magnificent palaces, gates, columns, and especially churches and chapels with their innumerable precious relics of earliest Christianity (including two pieces of the True Cross, "large as a man's leg and three feet wide"). Patrone exhaustively annotates every passage, comparing each with other readings or interpretations of Villehardouin, Gunther of Paris, Anthony of Novgorod (especially regarding relics), and the Greeks, Nicetas and Mesarites. Though reaching no startling new conclusions, she makes useful observations, which are, for the most part, convincing: that Robert went home in



the spring of 1205, not in 1206; that Robert composed his work not so much to identify and place in context for the monks of Corbie the relics he brought back (no less than fifty-four—one wonders what each of the great lords brought!), nor at anyone's specific request (for example, that of the abbot of Corbie), but simply, in the manner of the age, to describe for his circle the expedition and what role he had played in these "heroic" events. Contrary to others, Patrone believes that, though illiterate, Robert had a remarkable memory for facts and events (despite occasionally confused chronology) that was supplemented by the recollections of his comrades. Despite the frequent dearth of solid evidence (we do not even know Robert's age), Patrone succeeds, through deduction or the gleaning of evidence based on records of the life of his class in Picardy, in giving us in six chapters some picture of Robert's character and culture and in placing him in the context of his time.

She emphasizes Robert's censure of the leaders of the crusade for cupidity and dishonesty regarding division of the enormous booty captured and stresses that the Latin prelates, between the assaults, turned the expedition into a holy war against the Greeks by branding them "traitors and assassins . . . and worse than the Jews" and then giving absolution to all Latin participants. Previous editions of the original text (P. Riant, Charles Hopf, and especially P. Lauer, on whose edition she bases her translation) are discussed along with French, German, and English translations. This Italian translation has certain nuances lacking in E. McNeal's English translation (1936), but the latter is still quite adequate for those with no Italian. The copious references to the most recent studies (A. Carile, C. Brand, H. Roscher, etc.) and the critical apparatus, extensive annotations, and analytical indexes of places, persons, and sources make this a very useful reference work for all scholars of the crusades and especially of relics seized in Constantinople.

DENO J. GEANAKOPOLOS  
Yale University

BENJAMIN HENDRICKX. *Hoi politikoi kai stratiotikoi thesmoi tēs latinikēs autokratorias tēs Kōnstantinoupoleōs kata tous prōtous chronous tēs huparxeōstēs* [The Political and Military

Legal Foundations of the Latin Empire of Constantinople during the Early Years of Its Existence]. Thessalonica: [the author]. 1970. Pp. 190.

The Fourth Crusade was a decisive factor in the historical evolution of the Balkan peninsula and western Asia Minor. But the Latin Empire, one of its many results, was a transitory phenomenon whose political influence became more and more insignificant until finally the Empire itself ceased to exist. By its very existence, however, it was a force of some importance and by its institutional development, a body politic by no means unattractive for study. Hendrickx's book, a doctoral dissertation submitted to the faculty of philosophy of the University of Thessalonike and devoted to an analysis of this institutional development, is not, therefore, without general interest.

Hendrickx makes the analysis of three documents the point of departure of this study: the agreement of March 1204, the *Partitio Romaniae*, and the agreement of October 1205. The first laid the basis for the feudal organization of the Empire and defined the position of the Venetians in it; the second provided for the actual partition among the crusaders, the emperor, and the Venetians of the lands conquered or expected to be conquered; the third, while confirming the agreement of 1204, defined more strictly some of its provisions. These three documents comprised, according to Hendrickx, the constitutional basis of the Latin Empire and served as the sources for the development of its institutions. These institutions, both political and military, were essentially feudal in nature and, as a consequence, Western in origin. Only in the ceremony of imperial coronation does the author see any Byzantine influences.

Greek apparently is not Hendrickx's native tongue. In general, however, despite some rough spots here and there, his narrative is very clear and his scholarship sound. These qualities make his book a useful contribution to scholarship. This usefulness is increased somewhat by the French summary of its contents appended by the author.

PETER CHARANIS  
Rutgers University,  
New Brunswick

ANGELIKI E. LAIOU. *Constantinople and the Latins: The Foreign Policy of Andronicus II, 1282-1328*. (Harvard Historical Studies, volume 88.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xii, 390. \$18.00.

This detailed account of the forty-six year reign of the Emperor Andronicus II is a welcome addition to the literature on the Paleologan era (1261-1453), a period that until recently has received less attention than the earlier centuries of Byzantine history. The author's emphasis is on the relations of Andronicus with Western Europe, especially with Venice and Genoa, which held important commercial privileges in Byzantium; with the Angevins and members of the French royal house who dreamed of conquering the Empire; and with the papacy which desired a united Christian church. Dr. Laiou rightly stresses, however, the influence of domestic issues on foreign policy and thus also includes a substantial discussion of internal conditions in the dwindling Byzantine Empire.

For twenty years after Andronicus ascended the throne in 1282, Byzantium's Western adversaries were occupied with the war of the Sicilian Vespers. Thus the new emperor felt he could safely withdraw from involvement with Europe, and made two significant changes in foreign policy: he repudiated the Union of Lyons negotiated by his father Michael VIII in 1274, and concentrated on the neglected defense of Asia Minor against the advancing Turks. His policy failed, however; by 1302 the Ottomans and other Turkish emirates were well established in western Anatolia. During the critical period 1302-11, Catalan mercenaries, hired to fight the Turks, turned against the Byzantines and ravaged Thrace and Macedonia. At the same time, Charles of Valois, claimant to the Latin Empire of Constantinople, was able to acquire an impressive number of allies for an expedition against Byzantium, which fortunately never materialized. Dr. Laiou clearly demonstrates Andronicus's dramatic shift in policy after 1311, as he not only sought a rapprochement with the West, especially the Ghibelline states, but also initiated negotiations with the papacy on the union of the churches. But it was too late; his earlier isolationist policy had determined the fate of Byzantium, which would remain a small

Orthodox Balkan state until its final collapse in 1453.

The author presents evidence that Andronicus had been willing to discuss union as early as 1311, and in the years 1324-27 seriously considered returning to the religious policy of his father. The title of the book might lead one to expect more discussion of Andronicus's relations with the unionist party in Byzantium, and with Catholic missionaries such as the Dominican Simon of Constantinople, who had correspondence with the imperial court. This is a minor blemish, however, in a carefully organized monograph of meticulous documentation, which should prove invaluable to scholars of this period. The author has made use of an impressive array of Western and Greek sources, and has included a useful bibliographical essay at the end of the book.

ALICE-MARY MAFFRY TALBOT  
Hiram College

RICHARD F. KREUTEL, translated, edited, and with an introduction by. *Leben und Taten der türkischen Kaiser: Die anonyme vulgärgriechische Chronik Codex Barberinianus Graecus 111 (Anonymus Zoras)*. (Osmanische Geschichtsschreiber, number 6.) Graz: Verlag Styria. 1971. Pp. 277. 140 Sch.

The defective manuscript Vaticanus Barberinianus Graecus 111 preserves the middle years, from about 1373 to 1513, and thus only the torso, of a demotic Greek chronicle of the Ottomans. Sometime in the seventeenth century a Christian author composed this work (called the Anonymous Zoras after its editor) following earlier compilations, mostly Italian. The historical merit of the Anonymous Zoras rests in its exposure of the mind of one observer hostile to the Ottoman enterprise. Dr. Richard Kreutel, known and respected for his exemplary translations from the Turkish, here publishes an excellent rendering of the text, adorned with helpful notes and glossaries. He suggests that the author, possibly a Venetian subject, was not a native speaker of Greek but a polyglot European, "ein kleiner Dolmetscher." While further investigation of dialects is necessary to decide whether this source is so conspicuously idiosyncratic, the few slips adduced (if they are slips and not current usage) are not persuasive.

The Anonymous is a peculiar and frustrat-

ing author, fortunate to have found both a sympathetic editor and a distinguished translator. Familiar with Greek, Italian, and Turkish, his unprobing intellect did not seek out Ottoman sources of information on the early period but, at best, interpolated Turkish expressions into the texts of his prototypes. A separate folio originally belonging to the manuscript relates events of 1596; so, even before the Anonymous set to work, Europe could have read printed translations of an early Neshri as well as two recensions of the Anonymous Giese. In his notes Dr. Kreutel confronts the Anonymous with his own translation of the Giese edition of Ashiqpashazade, a source for Neshri, often obscure certainly but always informative. The Anonymous either knew of no such sources or rejected them. It is provocative to realize that a European armchair historian of the time was better equipped to prepare an Ottoman history than at least one literary levanter. Dr. Kreutel forces us to ask why.

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#### MODERN EUROPE

SALO WITTMAYER BARON. *A Social and Religious History of the Jews: Late Middle Ages and Era of European Expansion, 1200-1650*. Volume 13, *Inquisition, Renaissance, and Reformation*; volume 14, *Catholic Restoration and Wars of Religion*. 2d rev. ed.; New York: Columbia University Press; Philadelphia: Jewish Publication Society of America. 1969. Pp. vi, 463; 412. \$10.00 each.

Professor Salo Baron's monumental, comprehensive social and religious history of the Jews has now reached the mid-seventeenth century. As Baron pointed out when he began publishing this revised and enlarged version back in 1952, the recent, momentous developments in Jewish history—the holocaust, the greatest tragedy in the long career of the Jews, and the establishment of a viable Jewish state in Palestine for the first time since the fall of the temple—have put a new perspective on the whole historic career of the Jewish people (vol. 1, p. vii). Part of this new perspective has been a reconsideration of how the Jews entered the modern world, how they interrelated with the European societies in which they resided, and

how this generated the forces that led to the catastrophic end of Jewish life in the Hitlerian world and to a form of the fulfillment of the millennial hope of the rebirth of a Jewish nation on the ancient soil of the Jews.

With this new focus, plus the immense accretions of fundamental information about ancient, medieval, and modern Jewish history, especially in the last three decades, as many academically trained researchers in several historical fields have turned their interests to this area, Professor Baron has been putting together a grand synthetic picture of the career of the Jewish people from its ancient origins to modern times. Utilizing an amazing amount of standard and recent scholarship, Baron has tried to delineate what he sees as major lines of development leading to the twentieth-century scene. (About half of each of the volumes being reviewed here are footnotes, encompassing an extraordinary amount of the primary and secondary literature available in most European languages and in Hebrew, Yiddish, and Ladino. Even more impressive is that this assemblage of sources has been accomplished by Baron and his wife alone, without any coworkers or research assistants [see volume 13, page vi].)

In view of the vast range of events in Jewish history, taking place all over the European, Mediterranean, and, later, American worlds, Baron has outlined in the preface to volume 9 his schema to deal with this in the period of the late Middle Ages and the era of European expansion, 1200-1650. For Baron, it is at approximately 1200 that "the fateful shift of the center of gravity of Jewish life and thought from Muslim to Christian lands" occurs (vol. 9, p. v). And, he sees the Treaty of Westphalia and the effects of the Thirty Years' War as the first bases for Jewish emancipation and for Jewish entry into the modern, capitalistic, Western European world. Volumes 9 through 12 have dealt with the effects of the medieval Christian world on the Jews. Volumes 13 and 14 cover the transition to the modern world, in terms of the effects of the Spanish and Portuguese Inquisitions, the Renaissance, the Reformation, the Catholic Restoration, and the Wars of Religion on the Jewish communities, principally in Iberia, Italy, the Holy Roman Empire, and the German states. The primary

emphasis in these two volumes is on the social, political, and religious developments in the Christian world that affect the Jewish communities and on the social more than the religious changes going on within the Jewish communities. The Jewish religious developments, especially the messianic movements culminating in Sabbatai Zevi's proclamation of himself as the Messiah in 1666 and in the rise of modern Hasidism, are given slighter treatment, as are those developments presaging the emergence of reformed or modernized Judaism. Presumably the subsequent volume covering the period of Sabbatai Zevi will go into the details of the long build-up to this traumatic event in Jewish history. And, one also presumes, other factors relevant to major modern religious developments will also be treated later on. The topographical division of the material enables Baron to concentrate on certain aspects at one time in the geographical context of specific European Christian political entities. It has, however, the defect that developments in the Jewish world taking place outside these geographical and political limits are not adequately explained or sometimes taken into account.

The first half of volume 13 deals with the crisis of Iberian Jewry engendered by the forced conversions starting in 1391, by the establishment of the Spanish Inquisition to police the *conversos*, by the expulsion of the unconverted Jews from Spain in 1492, and by the forcible conversion of the Jews in Portugal in 1497. Baron carefully traces the developments that led to the onslaught against the Spanish and Portuguese Jewish communities, ending the Golden Age of Iberian Jewry and creating the Marrano diaspora. He then traces the New Christians and/or Marranos as they fled all over Europe and to the Spanish and Portuguese overseas empires.

The tragic history of the Marranos and the economic, political, and cultural effects this drama had in Iberia and elsewhere are now a subject of major investigation by many scholars working from many different perspectives. In the few years since Baron wrote his volume much new material and many new proposed interpretations have come out, as well as a good deal of controversial literature. I think most scholars in the field would still agree with

Baron's summation that "unwittingly the Inquisition, too, set in motion historic trends which, in the long run, greatly fructified the development of modern civilization. Not the least among these new factors was the rise of a vast and variegated Marrano diaspora, which, like other great migrations of religious persecutees, served as a yeast in the growth of new forms of life and thought" (vol. 13, p. 63).

But many of us working on the effects of the Marrano diaspora would feel that Baron tends to minimize what happened. The substantial new contributions of Carlos Noreña on Luis Vives, of Claudio Guillen on Las Casas, of Stephen Gilman on the author of the *Celestina*, of Karl Kottman on Luis de Leon, of Martin Cohen on the Jews of Mexico, of myself on Isaac La Peyrère, as well as the controversial works of A. J. Saraiva, I. S. Revah, Elias Rivkin, and many others have both increased our basic information about what happened and what this may have led to. Baron usually takes a less controversial stand than others on such matters as whether Columbus was of Jewish origin. Baron tends to see the New Christians, such as Vives, mostly in terms of their Christian apologetics rather than in terms of the heterodox views that may have been engendered by their situations in the Iberian world and their roles in undermining the standard Christian outlook. The Kabbalists who became Christians are mainly written off once they leave the fold. Figures like Leone Hebreo, the son of Don Isaac Abarbenel, are truncated to just their general influence on the Christian world, with their Jewish contributions ignored or minimized. The mix that probably existed of Jews, Marranos, and New Christians—each advocating views that reflect the kind of new amalgamation taking place by the immersion of Jewish scholars in Christian topics—and of Marranos and New Christians from Servetus to Valdes, Vives, Montaigne, Sanches, Ricci, and a host of others is not given much consideration as a major force in the emergence of new ideas. Similarly the Marranos and New Christians who became involved in the Reformation and Counter Reformation are given short treatment if they remained Christians after leaving Iberia. Baron concludes his discussion of the Marranos by saying that "the large and highly

diversified Marrano diaspora thus greatly influenced much of Western, as well as Jewish history. Its impact on the nascent capitalist evolution is yet to be told in detail. . . . Because the exodus of the Marranos from Portugal and, to a lesser extent, from Spain lasted much longer than the exodus of the Huguenots after the revocation of the Edict of Nantes in 1685, it kept the Jewish communities and their neighbors in a state of more-or-less permanent fermentation for two or three centuries. Not the least were its seminal effects on the cultural life of the Jews, and to some extent, of the Christian world, which was then in the throes of the humanistic Renaissance, the Protestant Reformation, and the Catholic Restoration" (vol. 13, p. 158).

Some of us suspect, or would claim, that the impact on the Christian world of the sixteenth and seventeenth centuries was much greater and led to many basic modern intellectual tendencies including modern skepticism, Bible criticism, and secularism. Baron sees the roles of the Jews and Marranos in the Renaissance, the Reformation, and the Counter Reformation as suppliers of data, such as Hebrew scholarship, and as middlemen of ideas. The New Christian humanists are seen as having become part and parcel of Christendom. The evidence suggests to me that all of the groups—Jews, Marranos (whom I see as both a separate group that was part Jewish and part Christian, perhaps, in many cases, being what Yerushalmi has called "potential Jews"), and New Christians (whom I see as Christians with a Jewish emphasis)—played roles in generating new forms of Judaism and Christianity for the modern world. Baron, on the other hand, considers mainly the baleful effects the Reformation and Counter Reformation had on Jewish life.

Volume 14 is devoted first to the Counter Reformation and how it brought back restrictions on the Jewish communities and persecuted the Marranos returning to Judaism, principally in Italy. The second half of the volume deals in great detail with what happened town by town in the Holy Roman Empire during the Thirty Years' War. Only brief attention is given to the influence of outside Jewish forces, in the tolerated Jewish communities of Poland, Turkey, and Holland, on what was happening in the Empire. The net effect of the Thirty

Years' War Baron sees as a major turning point in Jewish history. First, since the war was fought over Catholic-Protestant issues, the Jews were no longer the most persecuted religious minority. Second, Jewish bankers, merchants, and contractors could play a vital role in providing the resources for the statesmen and generals, and thus became important in the emerging new order. Third, the military and political deadlock led to an "enforced mutual toleration of Catholics and Protestants" that was "bound deeply to affect the relations between the Jews and the majority peoples" (vol. 14, p. 294).

The latter claim, taken by itself and taken with the broader thesis that the Treaty of Westphalia was the beginning of Jewish emancipation, is bound to raise many questions and doubts. Baron ends volume 14 by coupling what he sees as the results of the Thirty Years' War with what he will presumably be dealing with in the next volume or two, the Jewish resettlement in Holland, England, and the New World. All together these developments may provide a base for his thesis. To this point it seems to me that the developments in Holland and the Dutch Empire play a crucial role in exhibiting how a tolerated and legally existing Jewish (rather than Marrano) community could function as an integral and vital part of a modern commercial state. The ideological debates in the French, English, and American Enlightenments, as well as the Judeo-centered messianic theories of the English and American Puritans, of Isaac La Peyrère, of Huguenot theologians like Pierre Jurieu, and of Jansenist millenarians like the Abbé Grégoire, provided an intellectual basis for the acceptance of Jews as equal participants in either modern secular states or messianic states. These views were not generated out of the deadlock in the Thirty Years' War but rather out of philosophical and theological considerations that were in the forefront in societies where the Jews were a tiny aspect of the social and political scene, namely England, France, and the English colonies in America.

To conclude, Baron's volumes are a most impressive compendium of what is known about Jewish life in the period 1400–1650, within the topographical domains that he treats. He has synthesized an amazing amount of material

and supplied a base for research on almost everything he touches, with the thorough bibliography in his notes. His *History* is, and probably will remain for decades to come, the most complete effort available. It already needs supplementation on the basis of new studies, but this was bound to be the case since fundamental research is going on all of the time on various aspects of Jewish history. The work is well written and full of stimulating interpretations that scholars will be debating for years to come. These volumes, plus the preceding ones and presumably the ones to follow, are indispensable for work in Jewish history. One can only be grateful to Salo Baron for his lifelong dedication to Jewish history and his desire to present the material in terms of a mid-twentieth-century focus.

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J. R. HALE. *Renaissance Europe: Individual and Society, 1480-1520*. (History of Europe.) New York: Harper and Row. 1971. Pp. 350. \$8.95.

Professor Hale, while "not ignoring the events on which a sense of chronology depends," wishes especially to understand "the quality of the lives" and "to suggest what it was like to have lived then." He is aware that an exploration of "states of mind" in what he calls "majority history" is hazardous because it can only be based on inference from "material circumstances" and accounts by individuals of how they thought and felt. In trying to cover all of Europe from 1480 to 1520, the author is faced with several problems—namely, how to deal with the enormous regional diversity; the lack of a genuine European community except of scholars, diplomats, churchmen, and possibly merchants (clearly minorities); and the extension of many deeply held attitudes far back or far forward in time. Indeed he is hard put to keep within his time limits and is compelled to stress the geographical variations.

Moreover, as he declares, "decisions about . . . what areas of experience to explore are ominously subjective." Professor Hale's historical commitment is toward "then-mindedness," and as in all such approaches the crucial question is whether it is "our" "then-mindedness" or "theirs." In his book, as perhaps in all such at-

tempts, it has to be "ours," for we supply the categories. The author meets the problem by offering a fairly conventional list of chapter headings reflecting the areas into which historians place themselves: "Political Europe," "Individual and Community," "Economic Europe," "Class," "Religion," "The Arts and their Audience," and "Secular Learning." But with a bow to structuralism he prefaces these with his first chapter on "Time and Space" in what is perhaps the most original treatment within his book. Each succeeding chapter is strongly original in its emphases, showing over and over again that the people living then did not think in terms of the categories we seek to put upon them. He offers many first-rate insights along the lines of how "they" put things together or broke them down. Despite his submission to the requirements of a semitextbook or handbook, it is clear that he has really written neither of these but something that goes a long way toward the reconstruction of the consciousness of that time and area.

Of what use then is such a book? An undergraduate, even an exceptionally intelligent and knowledgeable one, would find it extremely difficult to follow in many places because it is often presented in terms of scholarly discussions of the problems. A history graduate student, particularly one wishing to work in these areas, would find Hale's efforts to "put it all together" extremely useful; his cautions and judgments of the proper emphases to be given various topics are salutary. Scholars, too, would find these judgments valuable, though, despite his rather remarkable garnering of evidences and *exempla*, they will not find many of the topics considered new to them (but some they will). He does make mistakes, for instance lumping Pomponazzi together with the humanists and Pontano with the philosophers, but these are few and not essential. And in the area of my own concerns, his extensive treatment of humanism seemed sound; reliable, and congenial. All in all, then, this is a fine book, though it is hard to place it—neither original research, nor *la grande synthèse*, nor textbook or handbook, nor bibliographical survey and evaluation; and yet something of all of these.

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HENRY KAMEN. *The Iron Century: Social Change in Europe, 1550-1660*. (History of Civilisation.) New York: Praeger Publishers. 1971. Pp. xiv, 464. \$15.00.

Henry Kamen's *Iron Century* treats of a great variety of topics: perceptions of space and of time, population structures, rise of prices, decline in real wages, increased value of land, the role of refugees in the expanding capitalist enterprise, the mode of life of the nobility, the bourgeoisie and the peasantry, poverty and poor relief, banditism, slavery, spiritual revival of the Counter Reformation, literacy, universities, religious toleration, witchcraft, peasant and urban revolts, and the revolutions of the 1640s. On all these subjects, and on many more, the reader will find a wealth of valuable information. The material is derived from a careful and critical reading of the recent social historians and from the author's own research in contemporary sources, from which he gives many an apt quotation.

The book is conceived as an "essay in quantitative history." Indeed, the influence of Braudel and of the *Annales* school is strongly felt. The author, however, believes that "art, culture and science" are so important that they deserve a separate volume; thus their virtual absence from this book is neither accidental nor a result of blindness to these sides of the story of man. Nevertheless, a penetrating account of the theology and the mood of the Counter Reformation (chapter 7) fills in at least a part of this lacuna.

The many examples on which Dr. Kamen bases his findings cover the whole area from Portugal to Russia and from Sweden to Sicily. Thus Europe "from the Atlantic to the Urals" emerges as a palpable reality: it forms one society whose parts seem to be connected with each other, and which is beset by the same types of problems, though the solutions to these problems may differ from place to place. Inevitably, Dr. Kamen has to deal with the question of the "general crisis of the seventeenth century." In fact, each of the first six chapters, devoted to the economy and social structures, leads up to this problem; in chapters 9, 10, and 12 the author comes to grips with it directly, while giving a succinct and incisive criticism of the whole crisis theory (pp. 307-09), at least of its more simplistic forms.

"There are," he says, "two, distinct senses in which the concept of a 'General Crisis' can be manageably discussed. The first of these . . . is the notable recession in the European economy, observable in the decade of 1610-20 and pronounced after about 1640. The second of these is the series of governmental crises of the decade 1640-50" (p. 309).

The socio-economic recession, which the author discusses in detail, was complex in character and hit all of Europe. The lands in the vicinity of the Mediterranean basin, the most advanced part of Europe, never recovered from it. Northern Europe eventually effected a recovery, though at a high cost in human terms. Amidst the generally unsettled conditions, land and various other forms of rent, as well as government offices (especially in France), became the most desirable form of investment. As a result of this, the pressure on the lower segments of society increased. In Western Europe the townsmen captured the countryside and depressed much of the populace to the status of a rural proletariat, while in Eastern Europe, where there was enough land, but labor was short, the landowners both captured the cities and imposed "new serfdom" on most of the peasantry. This is the background to the peasant and urban uprisings that are so ubiquitous in the sixteenth and seventeenth centuries. When the administrative and fiscal needs of the governments, occasioned by the pan-European war, rose to unprecedented heights in the 1630s, this burden produced a series of explosions all over Europe that led to the general governmental crises of the 1640s. These revolutions, taking different courses in different parts of Europe, were all stillborn, and the general pacification that followed them was characterized by a conservative reaction: the aristocracy became socially more firmly entrenched than ever, and absolutism emerged as the new form of government.

Obviously one cannot do full justice to Dr. Kamen's complex work in a brief review. A few questions and criticisms are, however, in order. The term "bourgeois" as used by the author covers such a variety of groups that it seems to include almost all the "forward-looking" elements of society. A person like Pascal would be surprised, and a president of the Parlement of

Paris indignant, if he saw himself qualified as a "bourgeois." Dr. Kamen regards the yearning of the upper bourgeoisie to acquire land and "live nobly" as a "backward step" (p. 178). If one adopts the system of values of a chamber of commerce, this may have been so. But the upper bourgeoisie who bought land and offices and then penetrated into the ranks of the nobility not only helped to rejuvenate the noble class, but also raised its educational standards, thereby helping to produce that "polite society" without which the cultural picture would have been bleak.

The popular uprisings, which Dr. Kamen so ably dissects, did not begin in mid-sixteenth century: they have a long pedigree, going back to the fourteenth century, and in some places even earlier. Would we then be justified in extending the "crisis" period to cover four or five centuries? This would indeed be a *reductio ad absurdum*, and Dr. Kamen is of course far from any such notions. Moreover the uprisings did not end in mid-seventeenth century, but went right on throughout the age of Louis XIV, though with less intensity than in the 1640s. Perhaps conservatism and absolutism were not as all-pervasive and successful as the author leads us to believe. Furthermore, in examining political thought we find that many of the absolutist writers of the early seventeenth century had put forth claims that went much farther than those advanced by someone like Bossuet; in this sense, at least, we can properly speak of a retreat of absolutist theory in the seventeenth century. Absolutism, no doubt, existed in the minds of Louis XIV in the first decades of his personal rule, of Colbert, of Peter the Great, or of Charles XII. But it was never fully implemented until the French Revolution; nor did it ever last more than a few decades. In France, for instance, a noticeable retreat from it began in the 1690s. In each instance it was the aristocracy, whom Dr. Kamen tends to regard as its ally, that helped to set absolutism back. I must add that none of these critical considerations detract from the very real value of this thought-provoking book.

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YOSEF HAYIM YERUSHALMI. *From Spanish Court to Italian Ghetto. Isaac Cardoso: A Study in Seventeenth-Century Marranism and Jewish Apologetics.* (Columbia University Studies in Jewish History, Culture, and Institutions, number 1. Edited under the auspices of the Center for Israel and Jewish Studies, Columbia University.) New York: Columbia University Press. 1971. Pp. xx, 524. \$20.00.

The scion of a Marrano family, Ferdinand Cardoso was born in Portugal in 1604 but was raised and educated in Spain, where he lived for several decades as a nominal Christian. As a teacher of philosophy and medicine at his alma mater, the University of Valladolid, and then as a practicing physician in Madrid, Cardoso participated fully in the rich intellectual life of Spain. In 1648, when he was forty-four and at the height of his career and in no apparent danger from the Inquisition—he had testified before this body in 1634 on behalf of another New Christian, his only recorded contact with the Holy Office—Dr. Cardoso moved to Venice and five years later to Verona, where he died in 1683.

Fernando and his brother accepted Judaism, becoming Isaac and Abraham respectively. In spite of his duties as a physician to the Jewish community of Verona, Isaac Cardoso was able to publish in 1673 "a sumptuous Latin tome of over 750 folio pages, entitled *Philosophia Libera* . . . , the summary of a lifetime of thought in science, medicine, philosophy and theology" (pp. 216, 218). Six years later his *Las Excelencias de Los Hebreos* appeared; Yerushalmi calls it "his apologia, and it is so in a double sense: a vindication of Judaism and Jewry before the nations, and a culminating justification of his own life and the choice he had made" (p. 350).

Like M. J. Bernadete's *Hispanic Culture and Character of the Sephardim* (1952), this work by Yerushalmi views the Sephardic world from within. The seventeenth-century Marrano who fled the Iberian peninsula and returned to Judaism was the descendant of Jews who had become Christians in Spain before 1492 or who had been forcibly converted in Portugal in 1497 without the possibility of flight. What must have been the character and spirit of these men and women, like the Cardosos, to maintain and even to remember their Jewish identity? They were forever haunted by a



Spanish Inquisition that persecuted the *conversos* and their descendants by fire and torture for not being good Christians. Did the Inquisition, by these methods, hope to bring about total assimilation? And, at the same time, the Spanish laws of racial purity—*limpieza de sangre*—which had so patently violated the teachings of the Church, prevented this same assimilation of these New Christians. Perhaps this impossible dilemma could only be resolved by martyrdom or flight and return to Judaism.

Yerushalmi's book opens up to the student of early modern Europe, who is well acquainted with Spinoza's contributions, an aspect of Marrano history hitherto almost unknown.

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J. M. ROBERTS. *The Mythology of the Secret Societies*. New York: Charles Scribner's Sons. 1972. Pp. x, 370. \$17.50.

The secret societies with whose mythology this book is concerned are those held responsible for the French Revolution and for later attempts to subvert regimes under Napoleon and Metternich. Mr. Roberts has set out to trace the growth of a belief in international revolutionary conspiracy that seemed compelling to nineteenth-century statesmen down through Disraeli's day. His stated aim is to "explore the subject as a whole." He deliberately leaves limits undefined and devotes as much attention to the societies as to the myths they inspired. France and Italy furnish the main scenes of action; Abbè Barruel and Buonarroti loom large among mythmakers; the Masons, the Illuminati, the Carbonari are the chief societies discussed.

The book is loosely organized along chronological lines encompassing a century between the 1720s and 1820s. The framework is flexible enough to allow for detailed accounts of certain mythmaking texts and of internal intrigues within the societies. Intermittent reviews of political events are also offered, in order to deflate certain myths and to uncover the "real" factors at work.

The book yields some fresh data based on mythmaking sources, but, for the most part, Roberts pieces together secondary accounts. His

lightly sprinkled footnotes offer inadequate guidance to the works he uses. Historiographical analysis is needed but is not supplied. The absence of a bibliography and the meager index are also major flaws. Still Roberts merits praise for mastering a formidable body of literature. He cannot lay claim to many "unexplored areas" but his book surveys more ground than any other comparable work I have seen. Despite its broad coverage, however, it falls short of fulfilling the author's laudable aim to block out "the subject as a whole."

For one thing, the professional blinkers worn by the specialist in modern European history are never set aside. One has the impression that pertinent studies by Simmel, Hofstadter, and even Georges Sorel were excluded as "outside reading" from the start. Political events are emphasized; social and intellectual dimensions are ignored. Relevant issues pertaining to the powers of the press, to "reading societies," and the clandestine book trade are never raised. Furthermore, given a subject that is expansive and cosmopolitan, Roberts adopts a staunchly insular approach. English Masonry is seen to cross the Channel (and become subject to foreign delusions), but it is also arrested at the ocean's edge. Thus Franco-American interchanges are not taken into account. English Masonry moreover is severed from earlier Continental influences. There is no glimpse of the rich Rosicrucian background amply furnished by Frances Yates, and no hint of the conspiratorial mythmaking that flourished under Louis XIV.

By beginning with convivial gatherings in early Hanoverian England, Roberts seems to have chosen the wrong point of departure for viewing his topic as a whole. His ending with "the collapse of the whole world of the secret societies" after the exposure of one of Buonarroti's networks is also open to question. Blanqui and Barbès would be surprised to learn that secret societies thereafter had "real substance only in . . . backward Balkan states." If Roberts had carried his story beyond the fiasco of 1823, at least as far as 1848, then he might be less inclined to dismiss the fears of nineteenth-century statesmen as "baseless rubbish."

Overreacting perhaps to long study of a

lunatic fringe, Roberts uses terms like "rub-bish" incautiously and is too quick to cast aspersions on others' mental health. He often invokes a consensus among all sane historians and implies his subject was dismissed as "nonsense" long ago. Yet he himself works at deflating certain myths in a way that suggests they have not all collapsed. He fails to exercise discriminating judgment and rarely airs issues that are subject to dispute.

There is no consensus at present about how seriously one ought to take the plots and police reports with which Roberts deals. Conflicting verdicts have been rendered by several of the authors he cites. Given problems posed by conspiracy, no verdict can be taken for granted. Even skeptics are sometimes deceived. Too often the skeptical author of this book seems to land in the same position as the true believer. He leaves little room for other scholars to differ and allows no margin of uncertainty for himself.

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BÉLA KÖPECZI. *La France et la Hongrie au début du XVIII<sup>e</sup> siècle: Étude d'histoire des relations diplomatiques et d'histoire des idées*. Budapest: Akadémiai Kiadó. 1971. Pp. 624, 41 plates. \$20.40.

This learned and important study presents the interpretations of Hungary's leading Rákóczi scholar, Professor Béla Köpeczi of the University of Budapest, on the diplomatic and political relations between Hungary and France at the time of the great social and national uprising, led by Prince Francis Rákóczi, from 1700 to 1715. These interpretations are based on numerous monographic studies and scholarly investigations in the Hungarian National Archives and the Széchenyi National Library in Budapest, the Archives du Ministère des Affaires Étrangères and the Bibliothèque Nationale in Paris, the Czartoryski Library in Cracow, and the archives of Dresden.

The first part of the study constitutes the author's most important contribution to scholarship. It examines the origins, extent, and course of Franco-Hungarian diplomatic cooperation during the War of the Spanish Succession. This initiative played a considerable role

in sustaining the socially, religiously, and constitutionally inspired Hungarian movement of Rákóczi against Habsburg domination, while it also benefited France by creating a serious military diversion for her chief opponent. Professor Köpeczi's detailed study clarifies several aspects of the Franco-Hungarian relationship. One of these is the now-established fact, formerly disputed or unknown, that Prince Rákóczi requested French support and that Louis XIV agreed only reluctantly to aid the Hungarians. The author describes the efforts of sympathetic Polish noblemen, of the Marquis du Héron, the French envoy in Poland, and of Count Miklós Bercsényi, Rákóczi's deputy, to persuade the French king of the seriousness of the impending Hungarian uprising. Louis XIV agreed to support the prince only after a widespread peasant revolt swept northern Hungary and Rákóczi assumed command of a general military insurrection. Another clarification concerns the refusal of Louis XIV to sign an alliance with the Hungarians. Prince Rákóczi repeatedly sought to obtain an alliance, believing that only French support could sustain his movement and assure Hungarian participation in the peace negotiations. His most controversial act, the deposition of the Habsburg monarch at the Diet of Ónod in 1707, was also motivated by this objective, but the French king refused even then to accept a treaty. The author discusses another interesting diplomatic move, the conclusion of an alliance between Peter the Great and Rákóczi in 1707, through which the enterprising prince hoped to gain international recognition for an independent Hungary or Transylvania.

On the basis of these and other conclusions the author presents several reinterpretations of Rákóczi's politics and diplomacy. He sees the prince, contrary to earlier views, as an enterprising, imaginative, and realistic diplomat who consciously pursued the goal of a self-governing Hungary or Transylvania. Professor Köpeczi argues that the idea of an independent Transylvania represented a realistic alternative to membership in the Habsburg Empire. In his view, Rákóczi achieved important financial and diplomatic support from France and thus made possible a Hungarian role in European politics. Furthermore, his proposal of a Franco-

Russian alliance suggests his understanding of European diplomatic alignments.

Space limitations forbid an appraisal of these viewpoints. In any case, they represent stimulating reinterpretations worthy of further exploration. Furthermore, the author's sustained narrative of Franco-Hungarian relations represents an original and valuable contribution to European diplomacy. Its great merit is the clarification of Rákóczi's diplomacy in relationship to European international politics, the politics of Habsburg absolutism and eighteenth-century European history.

In the second part of his study the author examines a somewhat related theme, the reception of the Rákóczi uprising by French public opinion and the significance of French opinion on Hungary in the evolution of eighteenth-century political ideas. While an immense quantity of useful information is cited here, the attempt to relate these data to the general theme is only partially successful. The lengthy and amorphous information of this section is not sufficiently analyzed and intergrated with the author's overall theme. Nevertheless, the conclusions, in which he points out the primacy of political considerations in judgments of the role of Hungary by Western thinkers, are valid and suggest important viewpoints. So, too, is the final conclusion that Rákóczi as a diplomat and as a representative of an emerging enlightened absolutism made a notable impact on eighteenth-century European politics. For these reasons the study as a whole is recommended as the most authoritative presentation available in a Western language on the political role of the Rákóczi uprising. The scholarly value of this publication by the Hungarian Academy is enhanced by an index of names, forty-one illustrations, careful footnoting, and a historiographical essay.

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MARIO D. FENYO. *Hitler, Horthy, and Hungary: German-Hungarian Relations, 1941-1944.* (Yale Russian and East European Studies, 11.) New Haven: Yale University Press. 1972. Pp. xii, 279. \$10.00.

Any historian seeking to do original work on the political and diplomatic history of Hun-

gary between 1918 and 1945 finds himself struggling to uncover evidence and suggest insights that were not already provided by C. A. Macartney in his unique and indispensable study of the period. Probably no historian can ever hope to match Macartney's intimate knowledge of personalities and events in Hungary during the 1930s and 1940s. It is thus a tribute to any author, in this case to Professor Fenyo, when he is able to produce a study that improves significantly on portions of Macartney's account.

This monograph, which concentrates on German-Hungarian political relations during World War II, introduces important new source material relating to a number of crucial episodes. In some cases, such as the knotty problem of the origins of the Kassa bombing incident of 1941 or Horthy's conversations with Hitler at Klessheim Castle in 1944, the author carefully sifts old and new evidence only to conclude that we may never know the true story. On other matters, however, he is able to draw more definite conclusions. For example, a close examination of Kállay's ambiguous policies leads Professor Fenyo to suggest that the Hungarian prime minister was hardly the hero of anti-German resistance that some writers have portrayed.

The book's greatest strength is the broad range of unpublished sources on which the narrative is based. The author demonstrates an enviable command of the German, Italian, American, and Hungarian documents found in the National Archives. However, it is to be regretted that the records of the British Foreign Office, now open to 1945, could not be consulted, for London was unusually well informed about Hungarian affairs, and the British played a prominent role in negotiations for a separate peace.

Professor Fenyo's work will be of great value to students of East Central European history, but it probably is not suitable for nonspecialists. Little attempt is made to provide introductory information: the political, economic, and social structure of interwar Hungary is only briefly discussed in somewhat disjointed flashbacks. A brief concluding chapter does not deal in depth with some of the more intriguing questions raised in the narrative, such as the nature of possible anti-German sentiments

among Hungarian peasants or the reasons for the prevalence of fascist and pro-German sympathies among army officers. It is to be hoped that Professor Fenyo in future studies will turn to a careful analysis and interpretation of the history of the German-Hungarian relationship that he has here so skillfully reconstructed.

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LAWRENCE STONE. *The Causes of the English Revolution, 1529-1642*. New York: Harper and Row. 1972. Pp. xiv, 168. Cloth \$12.50, paper \$2.95.

G. E. AYLMER, editor. *The Interregnum: The Quest for Settlement, 1646-1660*. (Problems in Focus Series.) [Hamden, Conn.:] Archon Books. 1972. Pp. viii, 248. \$10.00.

CHRISTOPHER HILL. *The World Turned Upside Down: Radical Ideas during the English Revolution*. New York: Viking Press. 1972. Pp. 351. \$10.95.

These three books are the work of eleven well-known students of the English revolution, an event that has, in the last thirty years, attracted more interest than any other period of English history. The authors share some excellent qualities: they are immensely learned; they write well; and they transcend the sterility of mere pedantic learning and lead us into a world of good judgment, delicate sensibility, and a concern for important human values. In addition, their personal biases are as candidly stated as one could wish. The three books are models of contemporary historical writing.

The authors are in considerable agreement on many of the basic questions. They all think of the events of 1640-60 as a "revolution," they describe the course of the revolution in the same tone, and they give generally compatible accounts of its causes and results. Yet they are careful to acknowledge that other scholars of equal learning and judgment, but with other points of view, would dispute most of their conclusions; for example, Professors Elton and Trevor-Roper and Mr. Laslett, for different reasons, would not agree that this was a "revolution."

Although all of these scholars have contributed to that recent and remarkable explosion of knowledge about the basic facts of the pe-

riod—"what really happened"—they do not look forward to a time when the sheer weight of established fact will eliminate differences of interpretation. Historians, they seem to assume, are humanists whose notions of what is important in history will continue to vary and whose debates, consequently, will never be concluded by additional evidence. Additional evidence will, of course, alter judgments. And the knowledge that judgments are never final may induce a humility that will eliminate such arrogant incivility, for example, as Professor Trevor-Roper's scurrilous attack on R. H. Tawney. The authors of these three books are urbane, civilized, and modestly tentative.

Professor Stone's book contains one major interpretive essay, "The Causes of the English Revolution," and two short historiographical essays, "Theories of Revolution" and "The Social Origins of the English Revolution." In "Theories of Revolution" he makes a determined effort to be kind to the social scientists who have written on the subject. He discusses "situation[s] of multiple dysfunction exacerbated by an accelerator," "dissensus," "J-curve theories," and the like, but he does not demonstrate that these are of any practical use to historians. Indeed, he himself warns us against this "obfuscating jargon, much of which conceals solemn statements of the obvious."

But if his essay on theory is unconvincing, his practice as a historian proves him at least partially right: he has made exemplary use of the methods of measurement, systematic sampling, and statistical testing that the social scientists have perfected.

"The Social Origins of the English Revolution" opens with a judicious summary of the controversy over the "rise of the gentry," the most famous historical controversy of the last half century and one to which Stone himself contributed with distinction. The questions raised are not yet answered and are, perhaps, not even the right questions. But Stone indicates that as a result of painstaking research and imaginative generalizing, "truth—partial, imperfect, provisional truth—is starting to emerge." "Looking back at the controversy today, it seems clear that all parties . . . failed to see that revolutions have extremely complicated origins, and that social causes are only one among many." "A more sophisticated view

of the causes of the English Revolution is beginning to emerge."

"The Causes of the English Revolution" contains both a concise review of previous interpretations and a catholic and magisterial synthesis of his own. Starting with the reasoned presupposition that this was a real "revolution," although not a Marxist one of class conflict, he divides the causes into "the preconditions, 1529-1629," "the precipitants, 1629-39," and "the triggers, 1640-42."

The preconditions include such long-term causes as the weaknesses of Tudor policy, the doubling of population between 1520 and 1640, and many others, all subtly woven together to demonstrate how everything affects, and is affected by, everything else. The preconditions made some redistribution of power almost inevitable, but whether by peaceful or violent methods depended upon the actions of government and opposition. The precipitants, a series of short-term developments such as Laud's ecclesiastical policy, brought the collapse of government from "the realm of possibility to that of probability." Finally, the "triggers" were "a series of unfortunate accidents and misguided personal decisions" that made war inevitable.

Professor Stone's analysis is balanced and informed. Another of his strengths is his candid admission that "every historian is obliged to follow his own judgement in the role he ascribes . . . to accident and to individual personality." He must ask himself what would have happened had so-and-so acted differently: "no logical scientific defense can be offered for such a procedure, but merely the weak argument that it seems to make sense to the working historian." And to the reader, may I add, when the historian is Professor Stone. But again in this essay, his references to the abstractions of the social scientists do not strengthen his analysis: the quotation from Chalmers Johnson that "revolution is always avoidable if only the creative potentialities of political organization can be realized" is a pompous truism.

Professor Stone's study ends with some interesting remarks about the significance of the English revolution. Its lasting importance lies in "the intellectual content of the various opposition programs and achievements after 1640. . . . For the first time, there came on the stage of history a group of men proclaiming liberty

not liberties, equality not privilege, fraternity, not deference. These were ideas that were to live on, and to revive again in other . . . ages." "It is this legacy of ideas which makes it reasonable to claim that the crisis in England in the seventeenth century is the first 'Great Revolution' in the history of the world, and therefore an event of fundamental importance in the evolution of Western civilization."

*The Interregnum* contains eight essays and a thoughtful and graceful introduction by the editor. The nine authors agree that this was truly a revolution, although, as Aylmer points out, they might have a hard time arriving at a common definition of the term. All agree that the Restoration was not inevitable: long-term trends worked in favor of a return of the old order, but it might not have come about except for some of those adventitious events which Stone calls "triggers."

The most interesting of these essays—that by Quentin Skinner—is a wholly convincing proof that Hobbes was not an isolated and eccentric thinker, but one who dealt with precisely those problems of political authority that were of most immediate concern to his contemporaries. Hobbes's distinction is that he was a profound philosopher who saw infinitely deeper than the pamphleteering propagandists who dealt with the same questions. In this brief, learned, and elegant essay, Skinner shows us the way of ideas in history and the relation between genius and its time.

Keith Thomas's critical examination of the thesis that the Levellers were not democrats but champions of property rights settles that issue. But he goes astray, I think, when he says that the Levellers' "economic program was backward-looking" because they wanted to create a society of small, independent proprietors: Thomas Jefferson and those of his successors who gave away quarter-sections of the public land to all comers and who wanted to give every freedman forty acres and a mule were not reactionaries, although they may not have been riding the wave of the future.

The essays by Valerie Pearl, Claire Cross, J. L. Cooper, and Ivan Roots examine the role of London in the Interregnum, and various aspects of Cromwellian policy. Professor Underdown gives a concise summary of the Court-Country tensions and generalizes in an

interesting fashion about the tendency of modern revolutions to nationalize and to destroy provincial loyalties. Finally, Professor Woolrych describes the last attempts (1657-60) to achieve a settlement without Charles II. None of these, he says, was necessarily unworkable, but the "folly and inadequacy" of the leadership made them unworkable.

Christopher Hill is the master historian of his chosen field—"Hill's half-century." In this dazzling new book, his best work so far, he demonstrates again his enormous capacity for painstaking research, his fertile, warm, and sympathetic imagination, and his command of an English style exactly suited to his purpose.

The leaders of the English revolution did not worry about the welfare of the lower half of the population, but with the collapse of the old order there came a brief moment of "glorious flux and intellectual excitement" when spokesmen for that lower half called in question every traditional belief and ancient institution. Hill looks at the most radical critics of the day, the "lunatic fringe," in the conviction that they "have something to say to our own generation," which is coming to understand that "madness itself may be a form of protest against social norms, and that the 'lunatic' may in some sense be saner than the society which rejects him."

This is a guide book for the Island of Great Bedlam—inhabited by Familists, Seekers, Diggers, Grindletonians, Muggletonians, Millenarians, Quakers, Baptists, Fifth Monarchists, Ranters, and others without a name. The author's learning is astonishing: we have known something about some of these people, but Hill has dug up dozens of unknown radicals and radical pamphlets dedicated to turning the world upside down.

The opening chapters summarize the changes in the century before 1640 which gave birth to the suppressed lower-class radicalism that burst into light during the anarchic days of the revolution. Hill acknowledges that we need to learn more about the origins: here is a rich field for future diggers. Chapter 7 introduces us to the hero of the work, the Digger Gerrard Winstanley, and later chapters describe the ideas of a number of lesser-known radicals.

The variety of radical opinion that Hill examines does not fit together as a consistent

social philosophy. But he demonstrates that by selecting, we can find in it anticipations of most of the radical ideas of today. These thinkers were mostly egalitarians and democrats; some wanted not only communal ownership of land but collectivized, large-scale, scientific agriculture; some were rationalists who rejected the Bible, heaven and hell, and the central dogma of Puritan Christianity, original sin ("no damn merit in salvation"); some attacked traditional learning and science and the universities as supporters of the oppressive status quo and found alternatives in astrology and alchemy; a few preached sexual equality and sexual freedom; others were anti-imperialist and nearly all distrusted military, legal, and political establishments; finally, many of them rejected the ethic of work which Hill (the hardest working of historians) calls Protestantism's chief contribution to the dark world of capitalism.

We can glimpse in this fragmented radical literature a new culture or counter-culture that rejected "private property for communism, religion for a rationalistic and materialistic pantheism, the mechanical philosophy for dialectical science, and asceticism for unashamed enjoyment of the good things of the flesh. . . . Its ideal would have been economic self-sufficiency, not world trade or domination." The Puritan emphasis on sin led to the ethic of work, the compulsion to labor, to save, to accumulate. While some of the radicals simply rejected this ethic, Winstanley suggested an alternative: exploitation, not labor, was the evil; abolish exploitation, and labor to beautify and furnish the commonwealth would be a pleasure. "Coolly regarded," Hill says, "we must agree that this was never more than a dream: the counter-acting forces in society were too strong." But then he adds, "it came nearest to realization in the Digger communities which might have given the counter-culture an economic basis." Brook Farm?

These obscure radicals, Hill claims, sketched for us "a possible society which would transcend the property system, of a counter-culture which would reject the protestant ethic altogether." We might be grateful to those men "who foresaw and worked for not our modern world, but something far nobler, something yet to be achieved—the upside-down world."

As he comes to the end of this remarkable book, the reader may forget that he has been reading a history: he will have heard a richly orchestrated *Te Deum*, an epic oratorio celebrating the triumph and tragedy of the lunatic fringe. "Their poetic insights are what seem to me to make them worth studying today." The reader's judgment of the merits of the book will be influenced by his taste in poetic insights.

The reader who is also a historian will have some questions. Did these madmen have any chance of succeeding? Hill says that they did not; he agrees, with all the other historians we have been discussing, that the historical tide was running the other way. But like them, he is also aware of the contingent and unexpected in human affairs: we would not expect President Nixon to invite Abbie Hoffman to the White House for a serious talk, but, as Professor Aylmer points out, Cromwell did just that for George Fox, who was as wild as any yippie. Still, Cromwell did not take Fox's advice.

Were the radicals a large and influential group? Hill agrees that they were a small minority, but he has demonstrated that there were more of them than we had previously imagined. Moreover if we may assume that they came from social levels normally illiterate, we may think of them as the visible tip of a massive, hidden iceberg. This raises the question of what happened to the iceberg after 1660—but that is not Hill's problem.

Did the ideas of Hill's radicals live on to inspire later generations? He agrees that they were pretty much forgotten, but he has dug up some evidence to show that more lived on in Milton, Bunyan, Blake, and the radical underground than we have hitherto suspected. But in the end these radicals are influential today, not because they survived but because they have been revived—by Hill.

In so rich and varied a book the critical reader will find many judgments to challenge. But the basic question is, what are we to think of Hill's picking and choosing from the writings of his radicals, not by any criterion which they would have used, but by his own criterion of what he judges useful today? He acknowledges that he has "picked out the most favorable examples. . . . a lot of nonsense was

talked and written." Surely the result of his method is to give us a badly skewed picture of seventeenth-century radicalism.

To which Hill might well answer that he is not interested in an exact rearticulation of the skeletons of dead controversies. He wants to show the present generation how the revolution liberated "the imagination as Christ rose, however briefly, in sons and daughters." He concludes with some reflections on the writing of history.

"The radicals assumed that acting was more important than speaking. Talking and writing books, Winstanley insisted, is 'all nothing and must die; for action is the life of all, and if thou dost not act, thou dost nothing.' It is a thought worth pondering by those who read books about the seventeenth-century radicals, no less than by those who write them. Were you doers or talkers only? Bunyan asked his generation. What canst *thou* say?"

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WILFRID R. PREST. *The Inns of Court under Elizabeth I and the Early Stuarts, 1590-1640*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. xii, 263. \$13.50.

Mr. Prest's book is one of those rare first-books-from-thesis that really meets the standard so nicely put in the Oxford statute for the D. Phil., "an original contribution to knowledge in fit form for publication." It is the first full-scale study of the Inns of Court for any period to appear for many years, and its scholarly quality is measurably superior to all previous studies. It sets a high mark for subsequent studies to attain. Those studies are not far away: Walter Richardson is completing a book on the Tudor Inns of Court and a young scholar at Newfoundland, Thomas Evans, is working on the Restoration Inns. Mr. Prest's book also advances the laborious task of understanding the Tudor-Stuart legal profession, work that has engaged the efforts of Eric Ives and, latterly, Louis Knafla and William Jones. It will stand as a pioneer study in what is becoming a major scholarly assault on the enigma of the early modern common law.

Approximately one-half of the book deals

with the structure, the administration, the finances, the composition (benchers, readers, utter-barristers, students), the maintenance of order, and the pedagogical function of the Inns. This is finely detailed analysis, drawn from a wide range of sources including the muniments of the Inns, commonplace books of students and barristers, memoirs, and the growing literature of the early seventeenth century on legal education. Pains taken to deal quantitatively with status and regional origins of entrants, numbers of calls to the bar and the bench, continuance at calls, attendance of benchers, and incidence of casual violence result in clear and useful tables. It is more a quibble than a criticism to question whether the entrance statistics based on years or moving averages of years were uniformly rectified to a calendar year beginning January 1. The statistical data are never allowed to serve for qualitative analysis and description, and the chapter on "Learning the Law" is a model for the history of pedagogy. For the first time we are afforded a concise, comprehensive description of how a barrister learned the law, with full recognition of how the learning process changed in the period to the point of virtual disappearance by the end of the seventeenth century. This chapter and the one following, "Legal and Liberal Education," bury the myth of the Inns as the "third university" of Tudor-Stuart England. Mr. Prest establishes beyond question that the Inns' pedagogic function, already in decline, was aimed wholly at training professional lawyers, not teaching law to gentle scions. Yet, perhaps he concludes too severely that sojourn at an Inn was largely formally noneducative for the casual student. There was a wide gap between the benefits rather shamelessly touted by the Inns (who did well by the gentle horde) and the benefits that actually accrued to the sojourner. But the more we learn of litigation in the period the more apparent becomes the "learned lay client," the landed litigant sufficiently knowledgeable about the law to instruct counsel directly and intelligently. If the growing body of legal literature was the principal source of his knowledge, a year or two at an Inn probably contributed to his ability to use that literature. This, however, demands more study, and the onus is now on the advocate of this position to

prove the educative value of the Inns of Court to the casual student.

The last three chapters, two of them dealing with the religious complexions of the Inns and the last chapter on "The Inns of Court and the English Revolution," are provocative and yet inconclusive. Mr. Prest's treatment of Puritanism is a long-needed corrective of the excesses of Eusden's thesis, and a short chapter on "Papists" is a valuable contribution to recusant history. The final chapter demonstrates once again the problem of connecting Stuart professional experience with revolutionary ideology, a problem that Gerald Aylmer encountered in his study of Caroline bureaucracy. Mr. Prest's problems in these chapters have less to do with shortcomings of research (there are none) or of conceptualization (this is remarkably sophisticated) than with our continuing ignorance of lawyers in their professional roles. Indeed, this lacuna haunts the book and the work of us all in Tudor-Stuart legal history. Mr. Prest is thrown back on a list of practicing counsel, numbering 489, drawn from appearance in the reports for the period 1590-1640. My own research on counsel in the Star Chamber indicates almost 1,500 counsel signed pleadings in that court, 1603-25, comprising virtually the entire bar in that period. Mr. Prest shows some ambivalence in assessing the importance of the Inns as the principal vehicles for conferring professional recognition upon practicing barristers over the course of their careers precisely because we know very little about the economic dimension of legal practice and even less about how a barrister's practice was begun, advanced, structured, and founded.

If the next step is a detailed and comprehensive study of the legal profession, Mr. Prest has pointed to areas of investigation that cannot be ignored in that task. The role of the Inns' benchers in the practicing profession is a central concern; if the readership had lost its practical distinctiveness, the status of bencher perhaps still had a critical influence on professional weight. The political dimension of being a lawyer obviously turned on the particular Inn, and we might gain something by talking less about "lawyers in Parliament" than about the "Lincoln's Inn saints in Parliament." Mr. Prest has provoked these random notions. With



a long and productive career yet ahead of him, he will doubtless raise other, more fundamental, questions and likewise answer them. He has made a bold start.

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RAYMOND H. LOUNSBURY. *Pennoyer Brothers: Colonization, Commerce, Charity in the Seventeenth Century*. Philadelphia: Dorrance and Company. 1971. Pp. xiv, 237. \$5.95.

Dr. Lounsbury's book is a filiopietistic work of genealogy, which was a very popular genre in the nineteenth century but has few practitioners at the present time. As a descendant, evidently, of Robert Pennoyer, he has collected most of the extant evidence about that colonist's altogether undistinguished career. Dr. Lounsbury was more attracted by Robert's elder brothers William and Samuel, who cut an important figure in Commonwealth government and trade policy and from the former of whose will the author seems to have been a beneficiary. The text and administration of William Pennoyer's will take up one-quarter of the book. It is always helpful to have primary sources printed, and the details of administration are instructive as to seventeenth-century practice. I share Dr. Lounsbury's belief that the Pennoyers have been unduly neglected as historical figures, but the present work does not remedy the deficiency. Virtually all his sources are printed ones, and much of the argument is derived from previous authorities such as V. T. Harlow and W. E. Foster. Although Dr. Lounsbury consulted a few English Chancery records, mostly in connection with the settlement of William Pennoyer's will, he missed the Chancery sources on William and Samuel's pre-1640 career in the Levant trade, which had an important bearing on their later politics and commerce. Such research has been done by others, such as Robert Brenner, who place their findings about the Pennoyers in a much more impressive interpretive framework than the trite generalities that characterize this book.

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B. S. CAPP. *The Fifth Monarchy Men: A Study in Seventeenth-Century English Millenarianism*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 315. \$18.50.

In a slightly modified version of his doctoral dissertation (Oxford, 1970), B. S. Capp has written the best analytical study of the Cromwellian Puritan sect, the Fifth Monarchy Men. It surpasses the sixty-year-old political study of Louis Fargo Brown as well as the recent narrative treatment by P. G. Rogers. Capp's book, however, does not achieve the theological insight of Alfred Cohen's doctoral dissertation, "The Kingdom of God in Puritan Thought: A Study of the English Puritan Quest for the Fifth Monarchy" (1962).

Capp is at his best when he analyzes the distribution and composition of the Fifth Monarchy Men. Two extremely useful appendixes, one identifying some 280 Fifth Monarchy Men by name and the other giving the locations of some seventy congregations and groups, enable Capp to say that the Fifth Monarchy sect, unlike the Quakers who were drawn from the country and from a wider social range, was an urban movement that attracted "the very bottom strata of society" (p. 85). The influence of a strong local leader, not an economic crisis, was an important circumstance in the rise of a Fifth Monarchy group, and nearly one-third of Capp's 280 men served as officers or rank and file in Cromwell's army, where millenarian ideas were rampant.

Capp is on sure ground when he says that "millenarianism . . . was indeed the *raison d'être* of the movement" (p. 14) and that the "Fifth Monarchists were unanimous that the saints should reign with Christ their King" (p. 137). The major differences were over whether Christ was to come in person and whether Christ needed any help from the saints. Capp is surely right when he says that Fifth Monarchism had a political program that was elitist in the rule of the elect over the ungodly, a social program that was egalitarian within the ranks of the saints, an economic program that was not anticapitalist, and a program for law reform that was based upon the Mosaic Code.

In the absence of a declaration of faith similar to those produced by the Independent and Baptist churches, it is difficult for Capp to

identify a theological position for the Fifth Monarchists, but he does note their combination of "biblical literalism and inward inspiration" (p. 188). Although he states that the Fifth Monarchy Men's devotion to the Mosaic Code made them hostile to Antinomianism, he observes that several of them had advocated Antinomianism in their earlier years, perhaps when they were imbibing its mystical ideas, together with millennial ideas, in Cromwell's army. Professor Cohen has suggested that Antinomian theology was closer to the internalized, Spirit-centered, and passivist kind of millenarianism that was to become so characteristic of the Quakers, whereas the externalized, Christ-centered, and activist kind of millenarianism was closer to the Covenant theology of such Fifth Monarchist leaders as Vavasor Powell, William Aspinwall, and John Tillinghast. Indeed, those very men whom Capp cites as having influenced Fifth Monarchy ideas of law reform (William Perkins, John Cotton, and Hugh Peters) also advocated Covenant theology. The tension in Covenant theology between man's fragile capability and God's overwhelming design, which was less present in deterministic Antinomianism, is a possible theological explanation for the reluctance of nearly all of the Fifth Monarchy Men, despite their seditious rhetoric against Cromwell's government, to take up arms against the Protectorate without a sure sign of the impending millennium.

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JOHN KENYON. *The Popish Plot*. New York: St. Martin's Press. 1972. Pp. 300. \$10.00.

This book is attractively produced with seventeen illustrations well chosen from contemporary prints and portraits. Unfortunately the notes are inconveniently placed at the end (pp. 276-91). The author is well known for his excellent life of *Robert Spencer, Earl of Sunderland, 1641-1702* (1958). The Popish Plot in one or another aspect has been treated in an interesting novel by John Dickson Carr, and in a brilliant exposition, particularly of the criminal procedure of the times, by Sir John Pol-

lock, as well as in other articles and monographs, to which Kenyon refers and with some of whose conclusions he differs. Pollock, he feels, was still influenced by the reign of Edward VII—that is, somewhat anti-Catholic in his judgments about the mysterious murder of Sir Edmund Berry Godfrey (p. 266). Kenyon relegates to an appendix a murder he thinks may have had little to do with the "Plot," though its effects upon public acceptance of the stories of Titus Oates and William Bedlow were so important. Kenyon's emphasis is less exclusively upon the effect in the capital of the mass hysteria of 1679-81 that sent so many Catholics to an undeserved death than upon the situation of the papists in the country at large before 1678 and the effects of the plot upon them thereafter.

The usefulness of *The Popish Plot* will be found, not so much in the ways in which its judgments of events and people differ in detail from earlier writers, but in the care with which the author has examined the records in provincial England, as well as in London, and the estimates arrived at as a result as to numbers, strength and weakness, treatment by neighbors, and decline of the various families, communities, and regular and secular clergy comprising the Catholic population of the reign of Charles II. Noblemen in most cases, save for Viscount Stafford, and a few self-exiled, protected by rank and often tolerated by their fellow gentry, nonetheless felt frustration at being excluded from the normal public service of their class. As many as ten members of a family of eleven took vows of celibacy in one instance; the frequency of this occurrence was, perhaps, one cause for a diminishing Catholic population. The plot itself dealt a great blow to the Jesuits, most hated of orders, but also to Benedictines, Dominicans, and schools maintained abroad by the religious for the education of English children. The Glorious Revolution, though it took fewer lives and could boast of no Oates, resulted in further weakening of their position. Some conformed; other families died out. There remained a substantial residue after the shouting had died down, to renew the faith when more tolerant days arrived.

Mr. Kenyon is judicious and careful. He has done a great deal of work for this book and has studied those problems connected with the his-

tory of the Popish Plot, which have until now received comparatively little attention.

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JOHN CRESWELL. *British Admirals of the Eighteenth Century: Tactics in Battle*. [Hamden, Conn.:] Archon Books. 1972. Pp. 263. \$13.50.

This book surveys a century of British naval tactics from Malaga to Trafalgar. Its main concern is to show why the line-of-battle formation remained the basis of tactical doctrine throughout the period. Captain Creswell makes a persuasive case. He displays the powerful advantages of fighting in line, as well as the hazards that were inherent in any attempt to achieve a concentration of force on a portion of a well-ordered enemy line (in fleet engagements one could not count upon sufficient speed and deception to bring it off). "Apart from hard fighting," he notes, "there could only be the hope that the enemy's commander-in-chief would so misconduct his line as to give you an opportunity of bringing such a concentration to bear." Finally, we are made to see that practically every admiral of the eighteenth century recognized all this. In short, the book is a response to those naval historians—A. T. Mahan, Sir Julian Corbett, indeed, almost everyone—who have supposed that Lord Nelson's predecessors might have made short work of their opponents, if only they had discarded the formalism of the fighting instructions in favor of the bold initiatives that marked Nelson's style. But, as the author points out, Nelson had good reason to be confident that his opponents were incapable of reacting quickly enough to frustrate his plans. Such confidence was seldom justified during most of the eighteenth century.

For the general historian the book's first three chapters are likely to be the most valuable. They offer a brief, clear explanation of the logic of tactical development and its relation to the technical characteristics of the ship of the line. Even readers whose knowledge of seamanship is feeble—there are some helpful asides for their benefit—will grasp the main points without much difficulty. In the remaining chapters, which deal with the significant battles of the century, the author amply illustrates his argument that the Printed Fighting Instructions were neither permanent nor imposed

upon fleet commanders by Admiralty authority. The notion that officialdom stifled initiative and innovation is false. Any commander in chief could issue his own Additional Instructions to his captains, and many did so. All in all, what emerges most strikingly from the author's authoritative commentary on these battles is that British failures arose less often from flaws in the instructions than from their improper execution and a lack of aggressive pursuit of an enemy in disarray.

Captain Creswell does not exhaust the subject; his study focuses on what occurred when two roughly equal fleets engaged and does not consider how changes in the logistical balance may have affected tactics under other circumstances. His concluding discussion of British and French gunnery skill is somewhat puzzling and unsatisfying. And one could wish that in his battle commentaries he had kept his main themes more plainly in view. Nevertheless, this is by far the best informed and most judicious study of eighteenth-century tactics that exists. Anyone who ignores it in favor of the orthodoxies propounded eighty years ago is likely to be wrong.

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R. S. NEALE. *Class and Ideology in the Nineteenth Century*. Boston: Routledge and Kegan Paul. 1972. Pp. viii, 200. \$9.25.

This slim volume is a collection of seven articles, chiefly on Victorian social history. Among them are "Class and Ideology in a Provincial City: Bath 1800–50," based on the author's M.A. thesis at the University of Bristol; a review of John Vincent's monograph on poll books; and "The Colonies and Social Mobility: Governors and Executive Councillors in Australia, 1788–1856." Since most of the articles have already appeared in print, the reason for assembling them in book form is not immediately apparent.

Presumably, the justification for the book is to be found in the first essay, "Class and Class Consciousness in Early Nineteenth Century England: Three Classes or Five?" In it Neale argues that historians have been misled by the "three-class model of social structure in the early nineteenth century," in which individuals are placed in the aristocracy, middle class, or

working class. He suggests that it be replaced by a five-class model: upper class, middle class, middling class, working class A, and working class B. In an introduction written for this volume, the author explains that all of the articles except one have been presented within the framework of the five-class model.

Unfortunately, the essay that forms the centerpiece of the book leaves a great deal to be desired. For one thing, Neale's target is no more than a straw man at best. Historians have not in fact been taken in by a "three-class model," although they may use that sort of terminology. J. F. C. Harrison, for example, in *The Early Victorians* (London, 1971), emphasizes the extent of stratification within both the "middle classes" and "working classes." In one sense, therefore, Neale is discussing a nonproblem of his own creation. Having set up an artificially rigid "three-class model" as his target, however, he has been trapped into incorporating much the same sort of rigidity into his own "five-class model," which turns out to be a slightly modified version of the original. Thus, Neale's "middling class" comprises "petit bourgeois, aspiring professional men, other literates and artisans." What they have in common is not made clear. In fact, throughout the essay the author flits from topic to topic without providing the analysis required to handle a subject whose complexity he has underestimated.

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GEORGE L. HERSEY. *High Victorian Gothic: A Study in Associationism*. (The Johns Hopkins Studies in Nineteenth-Century Architecture.) Baltimore: Johns Hopkins University Press. 1972. Pp. xx, 234. \$15.00.

STEFAN MUTHESIUS. *The High Victorian Movement in Architecture, 1850-1870*. Boston: Routledge and Kegan Paul. 1972. Pp. xvii, 252. \$19.25.

PAUL THOMPSON. *William Butterfield*. Cambridge, Mass.: M.I.T. Press. 1971. Pp. xxix, 526. \$25.00.

The rewriting of Victorian architectural history continues. There are three accelerating trends. One rejects what Professor Hersey calls "the invective tradition" (p. xviii) in stylistic

evaluation. Another approaches style itself as more than mere description, attribution, and pedigree. Instead "we must try to reconstruct the Victorian vision of the past from pictures and descriptions and then see the Victorians' own buildings and their own choice of historical motifs in the light of that reconstruction" (Muthesius, p. 3). A third trend sets individual architectural effort in a firmer historical and sociological perspective—emphasizing the structure and role of patronage, the economics of the building industry, and the professionalization of practice. These three studies illustrate clearly how these new approaches can transform existing knowledge and former judgments. They also have much to say to nineteenth-century historians whose interest in architecture per se is minimal.

Hersey's basic contention is that the eighteenth-century esthetic doctrine of associationism—the investing of artifacts with anthropomorphic and mechanical properties—was transmitted from its progenitors, men like Ledoux in France and Alison in Scotland, via Loudon, Ruskin, and the Ecclesiologists, into High Victorian Gothic precept and practice, until eclipsed by the new iron and glass technology and Ruskin's retreat from the Gothic. The thesis—a kind of architectural Great Chain of Being—is an exciting one that ought to convince, given Hersey's very wide reading and evident sensitivity to Victorian architectural language, both verbal and visual. Yet it does not. One problem is that the style—a hectoring compound of Arnold Hauser and the later Ruskin—does not engender confidence in the argument. In a sense Hersey's strengths are literary and descriptive rather than historical and analytical (his comment on page 53 on how "the pre-Victorian ruin cult is beautifully discussed by Rose Macaulay" is quite revealing) and often lead to some dazzling and original insights. "For the ecclesiologists the church building was a penitential appliance, an architectural paraphrase of the suffering body of Christ" (p. 68). "Despite his clamorous and witty prose [Pugin] often breaks into setpieces that remind one of Leigh Hunt, Hazlitt, or even Chateaubriand. . . . It would be interesting to investigate the French side of Pugin's intellectual heritage" (p. 66). But there are also too many minor errors. Ruskin was not exactly

"a protege of Loudon's" (p. 23). Butterfield's dislike of hassocks was on democratic rather than puritanical grounds. Those who viewed the interior of All Saints Margaret Street in 1859 were hardly likely to do so with a "Wilkie-accustomed eye" (p. 114) at a time when the High Church laity, among others, was already on familiar terms with the Pre-Raphaelite esthetic. But the central deficiency is methodological. "Buildings," Hersey tells us, "are discussed only insofar as they contribute to the elaboration of my ideas" (p. xix). Hence the main argument never receives the precise handling it deserves. One is never quite clear whether associationism in such a context was an explicit doctrine, a cognitive mode, or a mere stylistic preference. One also wonders whether, utilizing the same sources, a case could be made for the presence—among architects, clients, and public—of a Romantic, Keatsian brand of associationism (where "new works of art are created as poetic responses to existing ones" [p. 260]) in parallel to the Alisonian species. But too often the prevalence and potency of associationism is assumed rather than proved. A more effective way of tapping architectural streams of consciousness might be by a thoroughgoing "structural" analysis involving the comprehensive collection and analysis of key architectural metaphors. Such an analysis might reveal that associationism was but one component of a High Victorian Gothic esthetic that also embraced functionalism, antifunctionalism, neoclassicism, order, and disorder.

Dr. Muthesius, in his closely written monograph, approaches such an analysis by suggesting that for the High Victorian architects, the traditional eighteenth-century esthetic categories of the sublime, beautiful, and picturesque had a significance that was either predominantly formal, psychological, or associational according to the architect, patron, or school involved. "For the Ecclesiologists 'sublime' is not only an aesthetic or stylistic alternative but also a general moral principle" (p. 13). For Pugin and Lamb, the picturesque predominated. In G. G. Scott, whose St. Pancras is brilliantly analyzed here in a single paragraph (pp. 179–80), we discover "the ultimate combination of Pugin's picturesque and Ruskin's sublime." Functionalism in turn was not a technologically induced *deus ex machina*, but evolved,

notably in the industrial or colonial church, from traditional considerations of picturesque utility. Muthesius also shows convincingly how the apparent eclecticism of much High Victorian architecture was not the product of ideological confusion but the result of an increasing exposure of English architects, through published source-books and the Continental railway, to highly selective features of the European tradition. It was France, not Italy, that provided most impetus after the mid-fifties, but as Muthesius points out, in terms of *contemporary* inspiration and example "between the continent of Europe and England there seemed to be very few exchanges either way" (p. 207). There is much else besides. Ruskin's influence on High Victorian architecture was probably less than it was on High Victorian design. In the Home and Foreign Office competitions of 1857, as with the designs for the Houses of Parliament in the 1830s, "decisions were made and influenced by the taste and opinions of a generation before" (p. 162). Proper attention is paid to the emergent sense of division between what Burges called the "art-architects" and the "professionals," and, as Muthesius shrewdly observes, "as to 'professionalism,' in spite of their criticism, the Ecclesiologists showed many symptoms of this development, with their specialization in one type of building and their specialised communication through the journal and in the meetings of the Society" (p. 161). There are weaknesses. Few leads are given, beyond what is already known, concerning the transition from Gothic to Old English and Queen Anne revival in the early 1870s. There is little about patronage. The style is occasionally tortuous and marred by unnecessary repetition (especially where the picture captions merely reproduce, verbatim, what has already appeared in the text). But these are minor blemishes in what is essentially a major re-evaluation of two crucial decades in Victorian architecture.

Dr. Paul Thompson's study, if superficially more restricted in scope than Hersey's or Muthesius's, turns out in practice to be the reverse. "I wish not merely to describe Butterfield's work, but to interpret it; to discover the forces which shaped it, and the meaning which he and his patrons and contemporaries found in it" (p. xxiii). This *verstehen* approach, so unsatisfac-

tory in Hersey's hands, succeeds triumphantly in Thompson's. The book is divided into two sections, one biographical and analytical, the other mainly descriptive, including a complete catalog of Butterfield's architectural works. Both, despite far too many substandard illustrations for a text of such quality, are continuously fascinating. Historians who may wish to fight shy of chapters headed "The Wall," "The Roof," and "Colour" are advised not to, for Thompson invariably discusses the social and historical context of architectural techniques as well as lucidly describing the techniques themselves. For with Butterfield "no other architect so consistently explored both the *material expressiveness* of wall architecture, and its *discipline* through wall planes; and, at the same time through colour, the triumphant joy of *faith*, and through line and pattern, the *insecurity* of an age of doubt and change" (p. 377). Historians will also welcome the full use Thompson makes of primary manuscript sources in creating a depth study of both an individual (devout, authoritarian, and withdrawn) and an architect (highly professional, scrupulous, and elitist). We learn of Butterfield's opposition to Ruskin's and Morris's doctrines of free craftsmanship (although more on his precise attitude to Ruskin would have been welcome), and how, approving of the shift of taste back to English Gothic in the 1860s, he regarded Bodley's All Saints' Cambridge as "one of the few churches in which he could worship" (p. 93). But the real strength of this study lies in the skill with which Thompson, as a social historian, firmly establishes, rather than merely alludes to, the social, economic, and religious context of Butterfield's work. He makes a good case (far more convincing than any associational equivalent) for the way in which the pervasive religiosity of Victorian life sensitized architects to the intellectual doubts, social bias, and doctrinal divisions of the established Church in a manner which compelled some of them to create architectural forms that would both transcend and resolve them. Butterfield's own religious background—Nonconformist forebears and a High Church mother—and attitudes—prayerbook fundamentalism, antiritualism, yet sympathetic to the ideals of the Oxford Movement—was as original and eclectic as his architecture. "Was it indeed," Thompson rightly

asks, "only for its architecture that he modelled his two most famous buildings upon the upper church at Assisi?" (p. 39). There are also important chapters on patronage (where it becomes clear that the bulk of Butterfield's clients were not drawn from an emergent provincial nouveau riche but from a traditional upper class, often kinsmen of local incumbents), on the builders (seven only of whom were responsible for over a third of Butterfield's ecclesiastical commissions), and an outstanding one on historicism. In short, to judge from Thompson's own work in this volume, Victorian architectural history, in its current transformation, is far too exciting a field to be left to the architectural historians alone.

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MICHAEL ANDERSON. *Family Structure in Nineteenth Century Lancashire*. (Cambridge Studies in Sociology, 5.) New York: Cambridge University Press. 1971. Pp. ix, 230. \$16.00.

Michael Anderson's *Family Structure in Nineteenth-Century Lancashire* offers the most outstanding historical analysis of the role of the family in the process of industrialization. While providing an important model for the quantitative analysis of family structures, the book also represents the first successful attempt to infuse statistical data with qualitative judgments of their broader social significance, and to interpret them within a well-defined body of sociological theory. The study presents an analysis of family and kinship patterns of about seven thousand individuals, including textile laborers, small artisans, and shopkeepers, representing ten per cent of Preston's population. It also includes population samples from rural districts in Lancashire and Ireland, from the places or origin of the in-migrants to Preston. The census data are augmented by rich documentary evidence on the entire textile region. Anderson analyzes migration, family structure, and occupational patterns, thus interpreting family behavior in its social and economic setting. The study is dynamic in its emphasis on migration and on the constant fluidity of the population. The role of the family as an agent of continuity and stability is tested in the context of rapid population turnover, changing in-

dustrial conditions, and social and economic crises.

While recent historical studies of the family have focused on either structure or ideology, Anderson carries his analyses on the structural as well as on the phenomenological level. The study gains depth from the author's effort to analyze family relations as one of several possible alternative relationships. Instead of studying the family in isolation he assesses its functions in comparison with public assistance agencies and a variety of other groups, such as neighborhood organizations and workers' associations.

An important contribution of this exciting book to the study of the nineteenth-century family is in its overthrow of the myth of the "isolated urban nuclear family." Most current census-based family studies have confused the "household" with the "family" unit and therefore concluded that urban families were predominantly nuclear and isolated. Anderson refutes this assumption in his demonstration of critical areas where kin relationships were crucial in facilitating adjustment to urban life. The picture that emerges from his involved multilevel analysis is one of a predominantly familistic society where kinship ties offered the most desirable and most satisfying short-term reciprocal relationships in an urban setting.

Although sociologists and historians have traditionally studied the family as a victim of industrialization, Anderson emphasizes, instead, the role of the family as an active agent. While social disorganization theory stressed the disruption and loss of kinship ties through migration, Anderson argues that kinship survived migration and economic crises and continued as the most pervasive tie, as well as the most important sustaining force in mutual aid relationships. Kinship served in channeling chain migration, in finding work, in sharing residences, in support during dependency and old age, in the care of young children of working mothers, and in assistance during unemployment, illness, or the death of the breadwinner. (Quantitative as well as literary evidence bears this out.) Kin relations were preferable to other organizations or reciprocal institutional relationships because in a fluid society kin provided a more reliable chain of immediate assistance than neighborhood associations or

public welfare organizations.

The socioeconomic model that Anderson constructs for the interpretation of the role of kinship in periods of rapid social change is particularly refreshing in its dynamic view of the family. "Family" is not merely seen as a structural unit, an artifact, or an abstract concept; it emerges as a process that takes different forms over the various stages of the life cycle. "Family" is translated into a series of reciprocal relationships and into various paradigms of exchange and interaction. Kin relations are "functional" and "calculative," based on economic need and rewarded by mutual benefits. While in rural areas family relations are "normative" (sanctioned by the requirements and ideals of a traditional, cohesive society), in the city they are functional and are governed by short-term reciprocal "instrumental" arrangements.

While Anderson's theory provides the most important explanation for kinship and family functions in nineteenth-century society, it also raises some problems, most of which are derived from the intrinsic limitation of available data, rather than from Anderson's theoretical constructs. First, one is not entirely convinced that under the circumstances of rapid population turnover, kin could be a more reliable source of assistance than neighbors. Second, Anderson's interpretation leads one to believe that "affective" relationships (based on emotion rather than on ulterior motive) were secondary in the experience of the working class, especially where critical life situations followed in rapid succession. His model of change in the family over several centuries further deepens the unnecessary dichotomy between instrumental and affective relations. He argues that while preindustrial family relations were normative-based, urban working-class family relations during most of the nineteenth century were predominantly calculative. As working-class affluence increased and as bureaucratized welfare carried some of the burdens of family support, calculative kin relations were replaced by affective relations. The problem with this model is in the assumption that calculative and affective relations are mutually exclusive. This weakness is especially reflected in the chapter on ideology, where Anderson argues that working-class socialization did not instill much love or

devotion to parents, especially not to fathers. In this argument Anderson falls into the trap of the "middle-class" characterization of working-class experience, which he successfully deflates in his book.

Historians of the family who have employed sociological models have often erred on the side of the superimposition of theoretical constructs on a body of historical evidence, or on the marshaling of historical data toward the refutation of a specific sociological theory. Sociologists, on the other hand, have often used historical data to illustrate theory, without attention to the historical context. Anderson overcomes the pitfalls of both approaches in this successful fusion of historical data with sociological theory.

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BRIAN HARRISON. *Drink and the Victorians: The Temperance Question in England, 1815-1872*. [Pittsburgh:] University of Pittsburgh Press. 1971. Pp. 510. \$11.95.

Although he lived in what was demonstrably a much poorer society than did his descendants a century later, the average Englishman of 1831 drank more than five times as much liquor and at least half again as much beer. There were three times as many public houses per capita in which he might indulge his thirst. Alcohol enhanced almost every festivity, it was a component of almost every medical prescription, and it was popularly associated with physical well-being. The public house served as a coaching inn, as a center for news, entertainment, and club meetings, and as a welcome refuge from drab, ill-lit, ill-heated cottages. Troops were billeted there; laborers often received their wages there; and the taxes on the beverages sold there supplied a third of the national government's annual income. Any movement that sought to separate beer and spirits from Britain clearly had its work cut out. To bring to an end the consumption of alcohol in nineteenth-century Britain implied a veritable social revolution.

Such a social revolution failed to take place in early or mid-Victorian Britain but not for want of effort on the part of the numerous temperance associations whose origins and purposes Dr. Harrison chronicles, whose leader-

ship he analyzes, and whose affiliations and interconnections—religious, political, and social—he explores. *Drink and the Victorians* is a remarkable book in that, except for the internal history of the brewing and spirit-manufacturing industry, the author appears to have investigated every conceivable facet of his complex subject on the basis of primary source evidence while at the same time demonstrating a mastery of the secondary literature on kindred social reform movements in England and elsewhere. Yet the outcome is rather less a monument to polished historical scholarship than an immense quarry of facts and figures, of fascinating insights and troublesome paradoxes. The sheer mass of the data leaves the reader in a state of befuddlement and the task of distillation largely in his own hands.

Harrison identifies four distinct though overlapping types of temperance reform: (1) the pre-1830 advocates of free trade in beer as a counterpoise to indulgence in hard liquor; (2) the "anti-spirits" movement of the early 1830s; (3) the teetotal or "moral suasion" movement that gained strength in the 1830s and 1840s; and (4) the "prohibitionist" movement exemplified in the United Kingdom Alliance (1853), which, as a first step, sought parliamentary legislation permitting individual communities to forbid the sale of alcohol by a ratepayer plebiscite. Like many Victorian social reform movements, the temperance crusaders were stronger in the North of England than in the London area and far more closely affiliated with Non-conformity and political radicalism than with the Church of England and Toryism.

To the extent that drunkenness had declined in England by the 1870s—and available statistics do not support the case for any remarkable degree of improvement—Harrison attributes such change more to the increased supply of piped clean water and of nonalcoholic beverages, to the supplanting of the coach (and the coaching inn) by the railroad, and to the greater availability of recreational "counter-attractions" than to the efforts of temperance reformers who were often fanatical, puritanical, and hostile to "popular culture." In the words of F. R. Lees, "No people were ever yet theatred into sobriety, danced into morality, or fiddled into practical philosophy" (p. 334).

Not that the author sees Lees and his cohorts



as no more than witting or unwitting capitalist stooges diverting workingmen from their true class interests. To the contrary, the temperance movement did call attention to a genuine evil, show compassion for the victims of that evil, and foster working-class respectability and self-reliance in the face of upper-class hostility. It also forwarded political democracy, demonstrated a willingness to use state power for purposes of social reform, and promoted Victorian social stability.

Successful as he is in steering between twin pitfalls—the partisanship and antiquarianism of the temperance enthusiast on the one hand, the patronizing contempt and facetiousness of the outsider on the other—Harrison does not altogether escape conveying an occasional sense of inconsistency and amorphousness. His final chapter, a somewhat uncritical summary of the twentieth-century socialist critique of the temperance movement, does not truly fit into an account that ends somewhat arbitrarily with the Licensing Act of 1872. Yet his study more than makes up in energy and industry what it may lack in organization, and future historians of Victorian England will have no excuse for neglecting a subject whose centrality Harrison has established in such abundant detail.

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ALEXANDER LLEWELLYN. *The Decade of Reform: The 1830s*. New York: St. Martin's Press. 1972. Pp. 221. \$7.95.

There is a tide in the output of specialized monographs, which, when taken at its flood, leads on to synthesis. With all due apologies to the Bard, one might say that Alexander Llewellyn has taken the tide at its flood. Building on the research of Gash, Kitson Clark, Briggs, McCord, Best, and a host of other specialists who have turned their microscopic attention to various aspects of the Reforming Thirties, Llewellyn has produced a soundly reasoned and well-written synopsis of the period.

In the first three chapters he deals with the rise and decline of Whig liberalism, from Lord Grey through the Reform Bill to Melbourne. Then he turns to the issues of poverty, Chartism, and the origins of the campaign against the Corn Laws. The final section, perhaps the

most disappointing, is given to public opinion as expressed on foreign affairs and imperial interests on the one hand, and on matters of church and state on the other. By its very nature, such a panorama runs the risk of leaving the specialist frustrated, tantalized by the sketchiness of the analysis. Certainly one would like to have documentation on the ideas and data that the author has obviously lifted from recent works.

Yet the synthesis, good and helpful, represents a sensitive blending of ideas, men, and events. It presents the issues of reform as complex as in fact they were. With a firm grasp of the recent trends in research, Llewellyn stresses the regional as well as the class distinctions in the England of the 1830s. And to his credit, he avoids a whiggish view of that period dominated by the Whigs and subsequently frozen by Whig historians. Here are no angels (liberal Whigs) or devils (reactionary Tories), nor are we exposed to any of those retrospective suggestions that in the reforms of the 1830s can be perceived the first steps toward democracy or the origins of the welfare state, ideas far removed from Grey, Melbourne, Chadwick, Cobden, and Oastler. "A generation should be portrayed with reference to its own values," according to Llewellyn. He adheres admirably to his principle.

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NORMAN GASH. *Sir Robert Peel: The Life of Sir Robert Peel after 1830*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. xx, 743. \$22.50.

This second volume (the first appeared in 1961) completes Professor Gash's monumental biography of Sir Robert Peel. The whole provides, for the first time, an account of Peel's entire career that meets modern standards of scholarship and that reflects the recent enormous increase in our knowledge of British political history in the years of Peel's active life.

Peel played an important though ambiguous part in the development of the new structure of British politics after the First Reform Bill. His progressivism offered an acceptable alternative for liberal, moderate men who were dissatisfied with the Whigs. He also managed to appeal to leaders of the professional, mercantile,

and industrial communities and to persuade many of them that their true interests lay in maintaining the institutions of the aristocratic elite. By his middle line he was able to broaden the appeal of the Conservative party and to attract to it a wider support that enabled it to win, in 1841, a major electoral victory.

Yet, his great administration of 1841-46 proved to be a difficult one, despite its enormous initial advantages. Tensions among the Conservatives remained and were actually heightened by Peel's efforts to modernize the party by combining different elements within it, which made it more powerful but less harmonious. Also Peel, once in office, went ahead a little fast for the always apprehensive right wing of the party, and began to lose the support of sections of his former followers as early as 1844 and 1845. He showed force and determination in pushing on, even when he anticipated that his policies might result in the overthrow of his government. His force appears also in the extraordinary efficiency of his administration, an efficiency that was achieved by his constant, minute supervision of the work of every department, dealing with each cabinet member separately, at great cost to himself in effort and fatigue. Peel's difficulties with his party were increased by his strong feeling that a minister should follow his own judgment rather than party wishes and by his repeated declarations, which his actions confirmed, that he would not be the instrument for giving effect to opinions in which he did not concur. There was a contradiction for Peel, Gash writes, between his attitude as minister of the crown and his position as head of the party, that he never resolved. The situation was aggravated by his lack of adroitness in dealing with the party backbenchers: his aloofness and impatience, and his unwillingness to spend time with them explaining his policies or cultivating their good will.

Gash has an obvious partiality for his hero. He argues, and makes a good case, that Peel's thinking on major issues was original and intelligent, directed not toward palliatives but toward resolutions of basic problems. Peel had, by the end of his second ministry, an impressive list of legislative and administrative achievements to his credit. He addressed him-

self to the task of social reconciliation, and the belief was central to his policy that the interests of industry, commerce, and agriculture were not in conflict but were interdependent. In the 1840s he sought to avoid treating the Corn Law issue as a class question and to prevent the aristocracy from becoming engaged in a battle over supposed class interests in which it could only lose. Though he favored the aristocratic institutions of the country, he insisted that the aristocracy could survive only if it showed a readiness to promote the welfare of all other classes in the community. Gash deals in effective detail with the development of policy on, among other things, the Ecclesiastical Commission, the income tax, the Corn Law question, the budgets of 1842 and 1845, and Ireland. He particularly stresses Peel's intense concern with the condition-of-England question, for which his solution was not the reduction of working hours but, rather, fiscal policies through which he hoped to make England a cheaper country to live in and to increase the purchasing power of the masses. In this sense, Gash holds, Peel's apparent obsession with tariffs and finance had a strong human and social side.

The book, however, is not uncritical. Gash has reservations on small matters, such as Peel's architectural taste, and large ones, such as Peel's misjudgments on the important Bank Charter Act of 1844 or his unskillful handling of the sugar question in the same year. More generally, the picture of Peel that emerges from this close study, though enormously impressive, is in some way unattractive: a greatly talented but difficult man, and a masterful and dictatorial administrator who tended to take too much into his own hands.

Peel is a hard subject for a biography, partly because certain phases of his career were and still are controversial, and partly because his life and work bring up broad issues of British politics and can be understood only in this general context. Gash has unusual qualifications for the task. His two earlier monographs, which deal with the 1830s and 1840s, the decades covered by this volume, have become standard—required reading for everyone working in this period—and constitute an admirable background for writing the biography of this most distinguished statesman. Gash is par-

ticularly good on the larger questions, and some of the most interesting parts of the book are his discussions of these general themes. He has, in addition to his command of the primary sources, also read widely in the recent articles and monographs on the period; though he does not often cite them, those in the field will catch many points where he has taken advantage of new findings or new insights and incorporated them into his presentation. His picture of Peel is perceptive and observant, and takes us a good way into the mind of this acute but complex man. The account is rather full, but this is justified by the interest of the subject. The manner of presentation is lucid, careful, and unpretentious; Gash seems to share something of Peel's own dislike of display. The book, despite its length, is firmly constructed, and the author, as he nears the end, is able to pull his threads together effectively and to keep the perspective of the whole remarkably well. The profession is much in his debt.

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JOHN PREST. *Lord John Russell*. Columbia: University of South Carolina Press. 1972. Pp. xvi, 558. \$17.95.

Few nineteenth-century parliamentary careers were more distinguished than Lord John Russell's. For about fifty years he was a commanding public figure: principal architect of the First Reform Bill, Whig leader in the Commons from 1835 to 1855, and twice prime minister. In the great age of Liberalism few statesmen promoted Liberal causes with more confidence and enthusiasm. Yet historians have been reluctant to celebrate his achievements. Until Mr. Prest came on the scene, they had little more to show than the official life written by Spencer Walpole under the unhappy sponsorship of Russell's widow and second wife (whom Walpole called "The Influence"), together with four volumes of correspondence unsatisfactorily edited.

This neglect is not altogether unwarranted. As Mr. Prest tells us, Benjamin Jowett once observed about Russell that "no one who is so deficient in social and external qualities can ever have justice done him" (p. xv). Russell's personal defects were indeed serious, as Mr. Prest makes abundantly clear. He was shy and stiff

and proud (as only Russells could be proud). He was markedly inept in handling people, cabinet colleagues no less than his miserly brother, the seventh duke of Bedford. He was too much the doctrinaire to deal effectively with Irish troubles in the 1840s, despite his great and unusual compassion for the Irish. And he was too little the man of business to bring the sort of authority that Peel could bring to the affairs of state and society. By the 1860s when Jowett took Russell's measure, these defects had taken their toll. Professor Conacher's *The Aberdeen Coalition* has fully revealed the querulous and demeaning decline of Russell's political career in the 1850s. And Mr. Prest himself declares that when Russell's ministry fell in 1852, "he should have retired. . . . He had, after all, as H. G. Ward told him, done more in twenty-five years than was ever done before by any British statesman, and had he given up at this point posterity would have recognized this" (p. 349). Instead, "before he died Lord John had thrown away a great reputation" (p. 350).

This strikes the characteristic note of Mr. Prest's book. Although he strives valiantly to restore Russell's reputation, he acknowledges that the effort of rehabilitation—particularly in the 1850s and 1860s—is too much for him. There is perhaps some ground for complaint here. Is it fair to attribute the later fiascos of Russell's career solely to personal defects? Was it Russell's fault alone that Palmerston overwhelmed him? Elsewhere in Mr. Prest's book, for example in his account of the passing of the First Reform Bill, the wider issues of society and politics—in this case, of the sort that Professor D. C. Moore has raised in recent years—are lost in the thickets of biographical detail. Still this is not a book to grumble about. It is the product of vast research, meticulously elaborated, equally shrewd about economics as about politics, and written with wit and verve. It is full of nice touches: Russell's casual way with budgets (p. 172); the oddities of Charles Wood's behavior in cabinet meetings (p. 229); or Grey's coming to London in the crisis of December 1845 "in the sort of ill temper men reserve for the occasions on which they are about to injure their friends" (p. 206).

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D. A. HAMER, *Liberal Politics in the Age of Gladstone and Rosebery: A Study in Leadership and Policy*. New York: Oxford University Press. 1972. Pp. xv, 368. \$16.25.

This book might seem more original if the gist of it had not already appeared in "The Irish Question and Liberal Politics, 1886-1894," which Professor Hamer published in volume 12 of the *Historical Journal* in 1969. But it would still fall short of his claims, either for originality or for importance. For one thing, it adds little to our understanding of the reasons for the decline of the Liberal party. For another, it repeats the traditional, though unconvincing interpretation, derived from the Webbs, of the Lib-Lab alliance; it tells nothing about the relative strength or social composition of the sections whose differing priorities receive so much attention; and it says nothing about the electoral or financial structure of the party. In other words, *Liberal Politics in the Age of Gladstone and Rosebery* leaves us, despite its rather abstract tone, with the same emphasis on personalities that Hamer finds unrewarding in the work of others—the more so because Hamer tends to accept the arguments of these personalities at face value. In the case of John Morley, whose biography he published in 1968, Hamer seems to have adopted them, with certain qualifications, as his own.

Like Morley, Hamer would have us believe that the absence of any generally accepted system of thought constituted the one great weakness in the Liberal party. But other parties have been known to survive without the benefit of systems, whether Benthamite, Federalist, or Marxist, and sometimes to have survived all the better. As for the Liberals, the point, surely, is not that they were divided in purpose and principle or that the various sections competed for attention, but that they were fundamentally at odds over policy. Hamer admits that the Nonconformists, in his opinion the most "vital" of the sections, cared not at all for social reform, which was precisely what labor, the other dominant section after 1886, cared about most. Had he discussed the problem of irreconcilable interests in terms of the decline of the party, tracing it back to the eighties, if not earlier, he might then have written a very suggestive book.

Instead Hamer addresses himself to the ways

in which the Liberal leaders tried to hold the party together and, in particular, to their differences of opinion as to the relative merits of raising one issue at a time as opposed to adopting a program or, in the absence of either, of resorting to a vigorous, if negative, anti-Toryism. This last approach he associates with Harcourt, though as chancellor of the exchequer Harcourt was responsible for the most constructive reforms achieved by the Liberals from 1886 to 1906; the second Hamer associates with Joseph Chamberlain; the first with Morley, Gladstone, and Rosebery. It is they who command his attention. Stressing "form and relationship" in the determination of policy, Hamer goes on to explain Rosebery's commitment to imperialism and the commitment of Morley and Gladstone to Home Rule in instrumental terms; that is, as a means of persuading the sections to subordinate their separate interests to the achievement of one great cause, thereby creating a spirit of system and at least a temporary sense of order. This is the most convincing part of the book and perhaps the most significant, for it means we must revise our estimate of Gladstone as an impractical idealist. Yet again, the point is not that Gladstone or Rosebery or any other leader was concerned about the unity of his party but that he failed so notably in achieving it. Home Rule was the wrong issue around which to rally the Liberals. Even in 1885 it should have been clear that it was the wrong issue; and one is entitled to ask why it seemed so appealing or why, for that matter, a single issue seemed preferable to many.

Actually, the questions are related, for on the basis of *Liberal Politics in the Age of Gladstone and Rosebery*—to be more precise, on the basis of Hamer's evidence, not his commentary—it would seem that differences of opinion about party strategy had less to do with the number of issues than with their substance. Those favoring a programmatic approach favored not any but a radical program, whereas those favoring a single issue wanted one specific kind of issue: it had to be transcendent and "national" and avoid, if not shelve, the awkward problem of social reform. Home Rule, the House of Lords, disestablishment, imperialism—issues such as these were in some sense calculated to stem the pressure for reform; and for this reason one or another was considered

by each of the contenders for the leadership of the Liberal party after Gladstone retired. The differences in purpose among the contenders were not nearly so great as Hamer suggests. Indeed, stripped of the intellectualizations with which his book deals, their contests seem as personal and paralyzing as we had been led to believe.

BARBARA MALAMENT  
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NANCY MAURICE, editor. *The Maurice Case: From the Papers of Major-General Sir Frederick Maurice, K.C.M.G., C.B.* With an appreciation by SIR EDWARD SPEARS. [Hamden, Conn.: Archon Books. 1972. Pp. vi, 245. \$12.00.

The "Maurice case" is the most acute example of maladjustment in the civil-military relationship to be found in the annals of twentieth-century democratic war leadership. On May 7, 1918, in the midst of the German offensive, the London press published a letter from former director of military operations General Sir Frederick Maurice accusing the leaders of the government of "mis-statements" with respect to British troop strength and the reinforcements thereof prior to the offensive. The letter was designed to precipitate parliamentary censure; a debate did in fact take place two days later, but Asquith's motion for a committee to investigate the charges was overwhelmingly rejected. The government won; "the soldiers" lost.

The battle still rages, however, after more than fifty years. The present work is prefaced by a lengthy diatribe by the distinguished military author and man of parts General Sir Edward Spears. The significance of this "appreciation" lies primarily in its emotional tone.

General Sir Maurice's daughter Nancy has assembled a potpourri of documents on the case and its subsequent controversial historiography as a memorial and "contribution to history." Of greatest interest to the historian is the general's 1918 diary and "Story of the Crisis of May, 1918," which is dated later that month. The diary, however, is not reproduced after April 20—the crucial period in the history of the crisis! There is little to be found in either of these regarding Maurice's motivations, allegedly to save the chronically threatened Haig and maintain GHQ morale, or his relationships with other soldiers or politicians

with respect to the attempted epistolary coup d'état. The correspondence with his former chief, General Robertson, and appended diary snippets indicate Robertson's encouragement of the action and Maurice's own decision "not to see Asquith. Must take sole responsibility."

In the months and years thereafter, however, General Maurice's "pen" was at the service of several Asquithian organs, including the *Westminster Gazette*, which in 1922 as part of what might itself be regarded as an intrigue against the declining Lloyd George coalition published Maurice's *Intrigues of the War*, here reprinted. It is a general indictment of the prime minister's conduct of the war. Much of the rest of the book is devoted to the hapless Maurice's attempts to get the justice of his accusations acknowledged by Lloyd George.

This documentation will be of value to students of the Maurice controversy. They will, however, have to make their own analysis of the issues involved.

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*The Art of the Possible: The Memoirs of Lord Butler, K.G., C.H.* Boston: Gambit. 1972. Pp. x, 274. \$10.00.

Lord Butler's autobiography is too short. This is not an unmixed compliment: he writes well, he has lived in interesting times (twenty-six years in ministerial office), and he is not too bound by official restraint in what he says; but he seems to lose interest about halfway through. His boyhood in India has the charm of an impossibly dead past, his youth the promise of stepping modestly through a succession of open doors, and his first political office the interest of seeing at close quarters the fight over the 1935 Government of India Act. His account of a conversation with Litvinov in September 1938 is an interesting minor piece of evidence to help defend Britain's Munich policy—though there is no point, in a book this size, in his trying to go over the arguments about Munich.

Butler is at his best when writing about the 1940s, when he handled the 1944 Education Act and the modernization of the Conservative party in opposition. In both operations he was trying to retain what was worthwhile from the

past and at the same time to fight off churchmen—who imagined they could go on fighting each other instead of finding some way to face the growing secularism of the age—or reactionaries—who imagined that the achievements of the postwar Labour government could be reversed. His real period of authority was as chancellor of the exchequer in the early 1950s, but at this point in the book his energy seems to have run down; there are some splendid moments with Churchill, but too much of Butler's account of events is from official sources or from the *Economist*. There are some flashes of deadly candor over Suez, and then six years of silence under Macmillan. Butler is determined not to say how much he wanted to be prime minister, but he makes it quite clear how badly he and Macmillan got on. If 1951–55 is too much a matter of scissors-and-paste, 1957–61 is almost a blank. Butler's interest revives when he is describing his work in dissolving the Central African Federation in 1962–63, and he goes into considerable detail about his second disappointment for the premiership. The book is a pleasure to read, and it would be even more welcome for historians if its favorable reception led Butler to write another book about his work as Churchill's chancellor and Macmillan's home secretary.

TREVOR LLOYD  
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JEAN-PIERRE LABATUT. *Les ducs et pairs de France au XVII<sup>e</sup> siècle: Étude sociale*. (Publications de la Sorbonne, "N.S. Recherches," 1. Université de Paris—Paris-Sorbonne. Travaux du Centre de Recherches sur la Civilisation de l'Europe moderne, number 13.) Paris: Presses Universitaires de France. 1972. Pp. 456. 58 fr.

According to Labatut the great feudal peerage of the thirteenth century was transformed, largely by the creation of new dukes and peers, into a group that in the later Middle Ages became increasingly dependent on the Crown for its status and income. The assertion of what Labatut calls the state over this powerful group would never be complete, however, for newly created peers fomented rebellions and frequently pursued policies of intimidation that severely limited royal power. In the sixteenth century the Crown undertook a large-scale effort to redefine the roles and powers of

the dukes and peers, at first by drawing new creations largely from military officers of good but not particularly illustrious nobility, to act as a counter to Montmorency's and Guise's intimidation of the Crown. Labatut's thesis, then, is that it was the state, albeit with vicissitudes, that fixed the number, rank, and at least partially the income of the dukes and peers.

Labatut does well to begin his general analysis of the dukes and peers in the seventeenth century by examining whether the ideal conformed to the reality, at least for the new creations. By analyzing the surviving charters of creation and comparing their accounts of the honoring of the individual and his family through a family history drawn from other sources, Labatut proves that the remarks about military valor, fidelity to the Crown, and the antiquity of family really were true. The dukes and peers were not devalued by the Crown through creations of titles that did not conform to the ideal of military valor and personal fidelity to the king. Verifications were made to insure that the families of new creations were indeed of ancient nobility, and over seventy-five per cent of the new creations during 1589–1723 were army officers, usually of high rank. Labatut remarks that there is a striking similarity between the descriptions of chivalric behavior in charters of creation and those in the tragedies of Corneille, a fact that sheds some light on the degree of consensus about the roles and social values of this powerful elite group among the literate, wealthy, and bureaucratic elites who enthusiastically attended *Le Cid*.

The total number of dukes and peers, including the new creations from 1589 to 1723, amounted to 386, from 113 families, including ecclesiastical peers and 30 women who were titled in their own right. The average age of new creations was 48 years, indicating that this supreme honor was bestowed on persons whose military career was about over; the average age at marriage for all dukes and peers was 24½ years, and the average age at death, 59½ years. Since the list of new creations is given, it is possible to determine the number of new creations by decade, but the failure to provide the dates at which titled families die out makes it impossible to determine the absolute number of peers at any one time, or even to compare the

size of the group, say in the reign of Henry III, with its size in 1715. Clearly Labatut is interested in family history and in elite groups, but he has not pursued his investigation either to reconstitute their families or to provide career-line analysis.

Labatut's discussion of the social origins of the new creations, and of their wives, is perhaps the most important part of the book, for he discerns a pattern of new creations that shows a relationship between their families' average age of antiquity and years of social and political turmoil. Prior to 1589 ancestors of new dukes and peers on the average could be traced back to the eleventh through the thirteenth centuries, whereas for 1589-1660 the average age of antiquity of the new creations dropped to the fourteenth and fifteenth centuries. Labatut also shows that the number of new creations increased in the decades of social upheaval (though we are not told by how much), for the first time providing precise evidence about the dynamics of social change and aristocratic revolt during the Wars of Religion and the Frondes. On the average, after 1660 the antiquity of newly honored families could again be traced back to the eleventh to thirteenth centuries, which certainly suggests that Louis XIV respected the wishes of the dukes and peers regarding new creations. Only one robe official, Chancellor Pierre Séguier, was given a ducal title, significantly in the tumultuous year 1650, but even he, claiming that he lacked a son, requested that his title be bestowed on a nephew-in-law who was of gentle birth. The dominant group, then, was not tainted by robe blood. Furthermore, while Louis XIV may have favored the great ministerial families, notably the Le Telliers and Colberts, he never bestowed the pre-eminent dignity of a dukedom on one of its members.

The three principal legal ranks of dukes and peers—those with Capetian blood ranked highest, foreign princes (e.g., Turenne) came next, and those of gentle birth followed—not only established their precedence but, according to Labatut, also reflected the size of their fortune. This hypothesis is tested by a comparative study of incomes and fortunes, and on the average each legal rank did in fact represent a certain standing in fortune. Thus a hierarchy of fortune—and therefore of lands held, income,

and dignity of title—brought about an extraordinary coherence to dukes and peers, a hierarchy no doubt discernible both to royal officials and to other seventeenth-century observers.

Average incomes for dukes and peers remained steady over the century, except for the Capetian princes, whose income declined under Louis XIV. It seems correct to conclude that if the dukes and peers underwent a "crisis" in 1589-1660, neither their dignity nor their income was basically altered, and this is confirmed by the rather disconnected remarks about the malaise striking the nobility as a whole in the Regency of Louis XV. But here I go beyond the author's intentions. Labatut is not really interested in the broader questions of the dynamics of social change; his subject is the relationship between the dukes and peers and the state.

There is other evidence to support the conclusion that no "crisis" occurred. The frequency of intermarriage among children of dukes and peers not only remained steady, but actually increased for the sons and grandsons of newly created dukes and peers. When wives were sought for sons (the dukes and peers almost never let their own daughters marry below their rank), and a certain amount of *mésalliance* appeared inevitable, daughters were selected from robe families who were predominantly in the Council of State and therefore close to royal favor, rather than from among parlementaires. Furthermore, when this marrying down took place, it occurred most frequently within ducal families of considerable wealth and prestige. Families that were having difficulty sustaining their rank dared not marry down.

When Labatut turns to the spiritual and cultural aspects of the group he is studying, the results are fragmentary, and there is no discussion of the charitable foundations of the dukes and peers, or of their patronage of artists and scholars. Were these also steady for the entire seventeenth century? The impression, based on literary evidence, is that both declined. One senses that Labatut has all the evidence, but since these aspects of family history, or of the history of elites, do not interest him, he has left them out. The remarks about how the dukes and peers viewed their estates, their Parisian *hôtels*, and their expenditures in clothing and

furniture do help complete the picture. The fact, for example, that expenditures on luxury items were roughly the same among all the dukes and peers, regardless of their income, is brilliantly suggestive of how these families competed with one another, and how destructive this could be for families with declining income.

To conclude, it is simply not fair to criticize this work for not being an *histoire totale* of a social group. Labatut does not belong to the *Annales* school, and perhaps because of this there is an occasional lack of refinement in his statistical computations and his sense of the significance of such subjects as philanthropy and patronage. And yet in this study of the social history of an elite group Labatut has returned to one of the classic questions the *annalistes* so often side-step, whether the group is self-generating and in control of the state or whether it is dependent on the state. Labatut actually gives evidence to support both of these contentions, but instead of delineating a mechanism that delimits the dynamic both of the state and of the elite group, he simply reiterates that it is the state which is in control. True, the materials presented about the state's defense of the dukes and peers (largely a gloss on Saint-Simon) against the Parlement, for example, reflect continued support for the dukes and peers. But does not the Crown's failure to have the legitimized sons of Louis XIV fully accepted as Capetian princes score a point for the intimidating powers of the dukes and peers against the Crown?

Clearly this provocative and important study will be of great interest to social historians of France and of elite groups in general.

OREST RANUM

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HOWARD M. SOLOMON. *Public Welfare, Science, and Propaganda in Seventeenth Century France: The Innovations of Théophraste Renaudot*. Princeton: Princeton University Press. 1972. Pp. xv, 290. \$12.00.

Although he was one of the most important protégés of Richelieu, Théophraste Renaudot (1586-1653) has not so far been studied in depth. Founder of the *Gazette*, the earliest successful French newspaper, sponsor of the *Conférences du Bureau d'Adresse*, originator of

employment offices in Paris, as well as of a low-interest loan bank, pamphleteer and popularizer of science and erudition, his free medical consultations for the poor led to his becoming the victim of a rather sordid drama involving two cardinals, the medical faculty, and the Parlement of Paris, and ending his life in misery and disillusionment. His enemies succeeded in depriving him of posterity's approval, creating a mass of legends that Dr. Solomon shows to be false in many respects. This amply documented portrait will permit the rectification of accounts of Renaudot found in many otherwise largely authoritative studies of the social history of seventeenth-century France.

Modern attitudes and interests are much in evidence here, particularly in matters of public welfare, health, medical care, adult education, the arts of communication, and the liberation of the practice and teaching of medicine from traditional routines. Dr. Solomon's findings have considerable relevance today, the more so as they offer an instance of how a powerful ruler can mold and conduct a nation's affairs in a period of foreign wars and deep internal stress, and finally leave his creatures helpless, their visions ruined, when he vanishes from the scene. Differences between our age and that of Richelieu are immense, but an account of this struggle between innovative government and established institutions in areas of public concern and the general good has its echoes in 1973.

Appendixes contain a nearly complete list of editions and translations of Renaudot's ninety or more publications, a dozen pages of illuminating extracts concerning the *Conférences* of the Bureau, a rich bibliography of primary and secondary sources, and a well-organized index. There is a thoughtful conclusion, but in spite of all these qualities one must say that the reading of proof is unworthy of the publisher and the author: there are innumerable misprints in the citations from French as well as in the main text.

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SAL ALEXANDER WESTRICH. *The Ormée of Bordeaux: A Revolution during the Fronde*. (The Johns Hopkins University Studies in Historical and Political Science, 89th series [1971], num-



ber 2.) Baltimore: Johns Hopkins University Press. 1972. Pp. x, 160. \$10.00.

The Ormée of Bordeaux has long been one of the most serious gaps in the historiography of popular uprisings. The precocious radicalism of the movement was striking. After following the Parlement and then the princes into rebellion during the Fronde, this elusive mass constituency turned against all traditional authorities and began holding rallies, issuing manifestoes, and intimidating their betters. Finally three thousand men "carrying hatchets and mallets" took over the city in a sophisticated, planned insurrection and set up an autonomous government that lasted for over a year, complete with assemblies, courts, and police. Merchant and artisan judges dispensed class justice while crowds persecuted their social enemies in a sort of primitive reign of terror. Westrich is right: the Ormée, if not a "revolution," was certainly revolutionary, and he has performed a real service in giving us the first study of it in English.

Unfortunately this book is not a comprehensive treatment of the subject. In his excellent introduction Westrich notes the complexity of the rivalries among warring corporate factions in Bordeaux, but he fails to trace the evolution of their loyalties clearly, and thus it is hard to assess the overall situation. The role of the various groups is elusive. It would be fascinating to know how the princes viewed this dictatorship of shopkeepers and what sort of loyalties rallied the masses to their side. The influence of the Huguenots is also unclear, and we never learn what part the frequently mentioned *présidial* played, or why "youths" emerged in the last phase and not before.

Westrich has done more justice to the revolutionaries themselves. His data on the backgrounds of their leaders are useful. His analysis of their ideology makes clear its interesting combination of radical and traditional elements, though it is hard to determine how widespread the various strains of class struggle and republican sentiment were, and who held what opinion. It is also disappointing to learn so little of the social dynamics of the movement—how the members treated each other, their principles of organization, their method of infiltrating institutions, their reactions to adversity and success, their ceremonies. The

book is a good beginning, and sources were no doubt a problem. What stands out is the distinctiveness of this "class war," not its similarity to other contemporary disturbances, yet there is little sense of why such an anomaly occurred. We are left unsatisfied, wishing for a fuller and more comparative analysis of this significant subject.

WILLIAM H. BEIK

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MICHEL LAUNAY. *Jean-Jacques Rousseau: Écrivain politique (1712-1762)*. Cannes: C.E.L. 1971. Pp. 511.

Rousseau has continually fascinated, influenced, or enraged readers since he won the Academy of Dijon's essay contest for 1750 with a learned essay on the morally corrupting effects of learning. This self-proclaimed "man of paradox" has provided so many scholars with a seemingly inexhaustible subject of research that one has the right to ask whether a new study is worth our attention. After all, it is to be presumed that Rousseau himself—having been so critical of "that crowd of elementary authors" (*First Discourse*)—would have preferred that we read and judge his works without relying on interpretations by others.

Michel Launay's *Jean-Jacques Rousseau: Écrivain politique* is a legitimate exception to these strictures: the fruit of over a decade of scholarship and writing in collaboration with a new generation of researchers, Launay's work can truly be said to add significantly to the twentieth-century reader's appreciation and understanding of Rousseau and his times. As in previous books and articles, Launay combines a thorough knowledge of Rousseau with an indefatigable quest for relevant contemporary documents. The result is a careful study of the main themes in Rousseau's political writings, in which philosophical interpretation and historical analysis are admirably joined.

Scholars interested in Rousseau and his epoch will find Launay's first chapter ample justification for acquiring and reading the volume. Utilizing effectively a multitude of unpublished sources, as well as the scholarship of the last two centuries, Launay presents a perceptive analysis of the political conflicts between the Genevan populace and the upper classes, which provided the basis of Jean-

Jacques's "political education." Equally instructive is Launay's study of Rousseau's experiences in the years 1728-48, which are usually ignored by critics and students.

It is, however, Launay's treatment of Rousseau as a "political writer," from the *First Discourse* to the *Émile* and *Social Contract*, that will be of widest interest. To an unusual degree, Launay succeeds in placing Rousseau's works in their political as well as social context. Yet this is no mere survey of eighteenth-century politics, nor does Launay reduce Rousseau to a merely passive reflection of his times.

To be sure, not everyone will always agree with every interpretation. This reader, for example, finds that Launay's approach sometimes underestimates the extent to which Rousseau's thought is directed to the perennial problems of political philosophy, especially as posed by Plato, Aristotle, Machiavelli, Hobbes, and Locke. But such questions concern complementary interpretations rather than criticisms, and it is no small feat to have shown how closely Rousseau's formulations were linked to contemporary political issues in Geneva, France, or other eighteenth-century societies.

In every generation, there have been a few "Rousseauists" whose commentaries are the basis of continued scholarship and informed reading; to such names as Derathé, Guéhenno, Hendel, Lanson, Masson, Mornet, Schinz, Spink, Starobinski, and Vaughan must be added Michel Launay. Indeed it is to be hoped that *Jean-Jacques Rousseau: Écrivain politique* will be translated into English, since it is probably the most useful recent work for students and nonspecialists who otherwise might not follow current French scholarship on Rousseau.

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PETER FRANCE, translator. *Diderot's Letters to Sophie Volland*. New York: Oxford University Press. 1972. Pp. 218. \$11.75.

Denis Diderot became acquainted with Sophie Volland about 1755, and their attachment continued until her death in 1784, only five months before his. During this time he wrote some 553 letters to her, of which only 189 survive. Peter France, lecturer in French at the

University of Sussex, has selected and translated 47 of these. His selections are representative; his translation is deft, idiomatic, colloquial, and accurate; and he has supplied an enlightening introduction, helpful footnotes, and a useful index.

The extant letters date from 1759 to 1774. They were first published in 1830 and have had through the years an enormous influence in enhancing the critical estimation of Diderot as a writer. For these letters to Sophie Volland are unexcelled in their revelation of a particularly interesting social milieu and of an infinitely rich, complex, and humane personality. Inhabiting these pages are most of the great names of the Enlightenment—d'Alembert, Mme d'Epinay, Falconet, Galiani, Garrick, Mme Geoffrin, Grimm, Helvétius, d'Holbach, Hume, Marmontel, Raynal, Rousseau, Sedaine, Voltaire, and many lesser lights. As the reviewer of this volume in the *Observer* put it, this correspondence is "one of the most fascinating of all documents about everyday life among the educated classes under the Old Regime . . ."; and, according to the *London Times*, "It is amazing that none of these letters has been published in English before."

Of the leading figures in the intellectual history of France in the eighteenth century Diderot is usually conceded to have outstripped Montesquieu in importance; and while he is not generally regarded as being the equal in significance of Voltaire or Rousseau, it is probable that he was more nearly representative of the whole Enlightenment than they. The letters to Sophie Volland are an incomparable means of looking into Diderot and into the eighteenth century. The edition at hand is a splendid book both for instruction and for entertainment.

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RUTH F. NECHELES. *The Abbé Grégoire, 1787-1831: The Odyssey of an Egalitarian*. (Contributions in Afro-American and African Studies, number 9. A Negro Universities Press Publication.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xviii, 333. \$11.00.

The principal title notwithstanding, this is not a full biography of Grégoire. Rather, it is an

account of his efforts to win legal equality for French Jews during the early 1790s, and for West Indian Negroes from 1789 to the end of his life. Mixing Enlightenment thought with a messianic Christianity, Grégoire believed in the equality of all men and sought emancipation as a first step in converting Jews and West Indians to Christianity and the other values of European civilization. He was a stubborn advocate of his views, often antagonizing potential support by extreme statements.

Grégoire usefully serves as a focus for the discussion of legislative moves toward legal equality during the revolutionary years, though he was not the main actor in most legislation dealing with the West Indies. Under Napoleon and then the Restoration, when Grégoire was far from the centers of power, he still served as an organizer for antislavery efforts. Treatment of Grégoire's career as an egalitarian also points up the diverse opposition to emancipation legislation, from anti-Semites during the early stages of the French Revolution to colonialists as slavery issues became more prominent. The general indifference of the French political public is also brought out, particularly as fear of radicalism became fashionable after the Terror. Without a special impetus, such as Grégoire's unusual religious convictions, French egalitarian sentiment had distinct limits.

These are some of the interesting themes that Professor Necheles brings out. Her narrative of events and publications is thorough and reliable, though at times the chronological framework produces rather choppy chapter divisions. The book does suffer from its ambivalent status, between biography and general account. Though the author clearly understands Grégoire, his motivations are not always explained; particularly, there is no effort to go beneath his professed principles to get at other factors that influenced him. Yet this is not a thorough treatment of the egalitarian movements. The main forces involved are not fully characterized, nor is the relevant balance of political power analyzed. One can agree with the appreciation of Grégoire's heroic advocacy and still want a more sweeping survey of the issues with which he was involved, particularly because Grégoire, consistently a loner, was more often influential than decisively in

charge. Without question, however, this book will be a valuable source for anyone interested either in Grégoire's biography or in the more general history of emancipation efforts during the revolutionary and post-revolutionary periods.

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RAGNAR SIMONSSON. *Frankrikes författningar* [The Constitutions of France]. (Skrifter utgivna av Statsvetenskapliga Föreningen i Uppsala, number 55.) Stockholm: Almqvist & Wiksell. 1971. Pp. 617. 55 S. kr.

From 1791 to the present France has had sixteen different constitutions. Simonsson, who for many years was a docent in political science at Uppsala, did not originally propose to describe all of these constitutions and intended at the outset only to describe the major changes of the recent past—the fall of the Third Republic, the Vichy Regime, the Fourth Republic, and de Gaulle's taking of power in 1958. But as he says in his preface, "It is impossible to understand the Fourth and Fifth Republics without full knowledge of the Third, and in order to understand that one must return to the Great Revolution of 1789" (p. 7). He has therefore set himself the task of writing a constitutional history of France since the Revolution, although to be sure, his emphases are different. The first third of the book covers the entire period 1791–1939. The most detailed coverage is given the period 1939–46, which takes up the second third, and the period 1946–69 is surveyed in the remaining third.

This is straightforward and old-fashioned political science of a kind not often seen today. Social and economic matters play little part here. His analysis of the Vichy Regime and of the Resistance clearly suffers from his ignorance of important questions that have been raised not only by French historiography but by German and Anglo-Saxon historians. In the most recent period he barely finds room for five pages on the student revolts and strikes in May and June 1968. An examination of the notes and bibliography reveal a sound knowledge of printed materials directly related to political matters, but provide no reference to other printed materials or to unprinted sources or, in

fact, to anything not readily available in many places. Simonsson's prejudices are carefully submerged, but it seems clear that he stands as a strong supporter of the parliamentary democracy of the Third Republic and its immediate successor. He clearly finds much that is praiseworthy in the democratic Left and, in the continuing debate over the question of a strong executive versus a strong legislature, prefers the latter. His prose is clear and convincing, but does not convey any excess of imaginative insights.

The book is very difficult to use. Not only does it lack an index, an all too common failing, but also lacks a reasonable table of contents, for the one so labeled lacks both pagination and numbering of chapters. In a book replete with personal names this is a serious drawback. At times there may be questions raised about his methodology. It is clear that his quotations from de Gaulle's television speeches in March and April 1962, for example, are drawn from volume 2 of the general's own *Memoires d'Espoir* (Paris, 1971). Given de Gaulle's rigorous sense of the elegance of the French language, one might expect that he would carefully edit his own speeches. Simonsson's book, in conclusion, is a good outline history of the constitutions of France. It is unclear, however, to what audience it will appeal. How many outside of Scandinavia will read French constitutional history in Swedish? The French summary, in fact, probably represents all that most persons interested in the subject will wish to examine carefully.

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JEAN-PAUL DESAIVE *et al.* *Médecins, climat et épidémies à la fin du XVIII<sup>e</sup> siècle.* (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Civilisations et sociétés, 29.) Paris: Mouton. 1972. Pp. 254. 59 fr.

The Perpetual Secretary of the Royal Society of Medicine, Félix Vicq d'Azyr, planned to publish a book like this in the 1790s. His project, the "Medical Topography of France," was to depict morbidity and mortality since 1776, epidemic disease, unusual illnesses, the abundance of water, and the influence of the environ-

ment including the weather on the "epidemic constitution." Progressive doctors all over France were keeping daily charts, urged on by Vicq d'Azyr, who insisted on faithful performance and accurate measurement (Réaumur's thermometer was just winning in France over Fahrenheit's and Celsius's and "pure" water was being defined by Lavoisier). The participating doctors would soon form a national staff for dispensing social medicine, or so Vicq d'Azyr projected in his "New Plan for the Reform of French Medicine," submitted to the health committee of the National Assembly in November 1790.

The French Revolution disrupted these plans, abolished the society, and seized its archives, thus preserving the statistical information that Jean Meyer recently "rediscovered" (as this reviewer did; see "Le droit de l'homme à la santé . . .," *Clio Medica*, 1970).

The present book promises to outrank Vicq d'Azyr's project owing to teamwork and improved methodology. But the volume under review deals only with the nation's climate and with epidemiology and medicine in Brittany alone. Its national purview is a promise, and, to keep us waiting patiently, the editor reprints four recent articles. Thus only half the book is new to the informed reader.

In 1966 Jean Meyer called for a broadly based study of the records assembled by the Royal Society of Medicine that was confusingly entitled "Investigation of the Academy of Medicine on Epidemics." Two research teams have divided the work: the climatologic experts under Le Roy Ladurie now present their findings. The study of epidemic disease is still being completed by J.-P. Peter.

In "Computerized Study of Meteorologic Data Assembled by the Correspondents of the Royal Society of Medicine, 1776-1792," the information is critically analyzed by Le Roy Ladurie and Desaive, correlated with grape and grain harvests, and with Labrousse's findings. Many tables are appended. The relationship of the weather to the French Revolution is becoming clearer. There follows a critical discussion of statistical validity of unreliable barometers and thermometers by O. Muller of the mathematics center at the Maison des Sciences de l'Homme.

J.-P. Peter's task is even more complex, for

medical data are less easily measured than the weather. Having decided to study not physicians or patients but diseases, Peter plunged into the differential study of diagnostic terminology. His computer has now learned a vocabulary encompassing the medical data of ten years for 120 localities. He hopes to offer answers to the hitherto unexplained high mortality of the years 1782-84 and to elucidate the "epidemic constitution" of pre-Revolutionary years. Volume 2 will reveal the results.

"Medical Personnel in Brittany in the Late 18th Century" is too modest a title for Jean Meyer's essay. He places medical services in the context of agricultural, economic, and climatologic problems and provides a foretaste of the integrated studies that should eventually result. He adds that "the aim of this investigation is merely to place markers on a long and arduous path: this is to be a collective adventure" (p. 210).

The concluding article by Goubert, "The Epidemic Phenomenon in Brittany at the End of the 18th century," analyzes why the Breton population decreased at a time when the general French population rose. The reason: epidemic disease.

The studies published here initiate a climatologic, epidemiologic, and medical manpower study of late eighteenth-century France. The researchers are evidently buoyed by the extraordinary richness of their initial archival resources. But only if provincial archives are found remunerative can a nationally valid inquiry result.

The present book whets one's appetite for further teamwork from these *Annales* scholars.

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ALISON PATRICK. *The Men of the First French Republic: Political Alignments in the National Convention of 1792*. Baltimore: Johns Hopkins University Press. 1972. Pp. xviii, 407. \$15.00.

This work is a thought-provoking introduction to the prosopography of the National Convention. Mrs. Patrick admits at the outset that this is not a complete analysis of the *conventionnels*. Though she has read widely, her basic sources are: Kuscinski, Aulard's *La Société des Jacobins*, the *Moniteur*, the *appels nominaux* of 1793, the printed speeches of the deputies on

Louis's trial, and the *procès-verbaux* of the electoral assemblies of 1792. It can be noted, for example, that the *Moniteur* should be supplemented by the *Archives Parlementaires* and Aulard's volumes by the *Journal des Débats* and the *Journal de la Montagne*. The author, however, has consciously limited her materials to workable proportions. She, therefore, concedes that her conclusions can only be tentative. Could a properly directed *équipe* exhausting all available evidence produce a definitive prosopography?

Mrs. Patrick presents voluminous statistics, some of which, according to her own admission, are based on inadequate information. At times it seems as if the compilation of statistics has become an end in itself. There are, nevertheless, many extremely valuable statistical materials.

Beginning with an analysis of the political alignments in the Convention during the Montagnard régime, the author then goes back to the *appels nominaux* of January-May 1793, finding consistency with later behavior, and then back to the elections of the deputies in 1792 by the secondary assemblies to discover correlations with the later political pattern. She finally presents a collective biography of the deputies (their political experience, ages, geographical origins, occupations, and their later careers) and attempts to relate it to political activity in the Convention. The work is somewhat marred by typographical errors, occasional inaccuracies, and a style that is at times infelicitous.

The first half of the book is largely an elaboration and slight emendation of the author's article in the *Journal of Modern History* ("Political Divisions in the French National Convention, 1792-93," 41 [1969]: 421-74). The controversy with M. J. Sydenham, which was continued in the journal in June 1971, is herein renewed. Mrs. Patrick reverts to a more traditional picture of the Convention's division into three factions. She recognizes correctly that neither the Mountain nor the Gironde was a disciplined group and that each was composed of fluctuating cliques. She continues to use Sydenham's "inner sixty" in a fashion to make them appear as a solid core of Girondins, an approach that Sydenham has maintained is a misinterpretation of his work. She is convincing in

her claim that the Mountain was largely carrying the burden of government after January 21, 1793.

Mrs. Patrick also raises important questions about various interpretations of other historians, including G. Pariset, J. M. Thompson, A. Mathiez, and Alfred Cobban (particularly his thesis on the nature of "the revolutionary bourgeoisie"). Despite the tentative character of many of her generalizations, Mrs. Patrick's work will long be used by anyone interested in further research on the parliamentary history and the prosopography of the National Convention.

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ROBERT L. HOFFMAN. *Revolutionary Justice: The Social and Political Theory of P.-J. Proudhon*. Urbana: University of Illinois Press. 1972. Pp. xvii, 429. \$15.00.

Few social theorists have left so enigmatic a heritage as P.-J. Proudhon, and few have been put to so many political purposes. Of the several recent attempts to draw coherence from Proudhon's often confused but always exhilarating ramblings, this book is the most ambitious, for it attempts to reconstruct his general moral philosophy and to reveal its humanist core. In many respects this quest is successful. More significantly, Hoffman recreates Proudhon in such a way that this observation takes on real meaning: "Possibly the most important aspect of his legacy is not what he thought, but what he leads us into thinking for ourselves" (p. 5).

While Proudhon's anarchism, egalitarianism, and concept of justice appear in his early works, it was the disaster of 1848 that gave him his essential philosophical bearings. His critique of the revolutionary tradition since 1789 hardened his opposition to the state in any form, including democracy. More important, Proudhon's antiutopianism and his notions of the continued peevishness and irrationality of contemporary man became more firmly fixed. (Both of these help explain his brief flirtation with the Second Empire.) Finally, and central to all the rest of his thought, the experience of 1848 convinced him once and for all that the

only hope of mankind was in the urban and rural working classes because, despite their present brutishness, their very being embraced the essence of man: productive labor, the taproot of human dignity and fulfillment. The mature Proudhonian philosophy emerges thereafter. Justice, the product of "respect, spontaneously felt and reciprocally guaranteed, for human dignity," lies at the heart of Proudhon's thought. Through this concept he reconciles radical freedom with the goal of an organic society. It is the medium through which the manifold contradictions of life may be held in dynamic, unfolding balance. It is the means by which self-interested individuals contract with one another, first in the context of their natural communities, then between such groups, to generate the "collective reason," the essential moral law of a society. From it derives his well-known social ideal of mutualism. Hoffman also brings coherence to Proudhon's concept of revolution. It is the long, slow, and never-ending process of the moral regeneration of man to be implemented through the development of a rational appreciation of the individual benefits of reciprocal justice. Force, even in the name of justice, is likely to result only in other forms of domination.

Yet one cannot accept Hoffman's assessment uncritically. For one thing, the scope of Proudhon's constructive thought is rather narrow. In terms of sheer volume, his critical talents, his merciless exposure of human foibles and contemporary social and economic contradictions, dominate his writing. Frankly, it is this negative Proudhon that remains most compelling for me. But Hoffman gives less attention to this Proudhon. As a result, while he produces an erudite appreciation of the deeper wellsprings of the man's philosophy, there is a flat and often redundant quality to this book that will disappoint many readers. There are also important inconsistencies at the center of Proudhon's thought that are glossed over. It is a strange libertarian who can accept aspects of anti-Semitic mythology and take an equivocal position on the question of black slavery. More fundamentally, Proudhon's male chauvinism and patriarchal concept of the family weaken his arguments against domination in an almost irrevocable manner, especially, as Hoffman says, since Proudhon feels that "from . . . the

family experience itself [children] can acquire the basis of a moral perspective and inclination out of which justice in the larger society will evolve." Moreover, Proudhon the atheist would endorse religion as good medicine for women much as Voltaire prescribed it for the masses. There is perhaps more mindless parochialism in Proudhon than the author might care to acknowledge.

A final chapter deals with the problem of Proudhon's influence and his meaning today, underlining—as did Alan Ritter in an earlier study—the relevance of his concept of human autonomy and dignity.

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F. W. J. HEMMINGS. *Culture and Society in France, 1848-1898: Dissidents and Philistines*. (Studies in Cultural History.) New York: Charles Scribner's Sons. 1971. Pp. 280. \$12.50.

This potentially valuable study is exasperating to read. Wide-ranging erudition is frittered away by an uncertain focus and chaotic organization. To be sure, the rubric "Culture and Society" is at the bottom of the difficulty. Neither term receives a precise denotation and their relationship is ambiguous. The author recognizes the hazards of theorizing "about the causal connections between social and cultural development" and avoids making such connections, but what he provides is a grab bag of information about the milieu of the arts.

The presentation is framed by a discussion of Lamartine's part in the revolution of 1848 and of Zola's crusade in behalf of Dreyfus fifty years later. This framework implies that art is to be judged by the way in which it relates to public affairs, and that the abstention of French artists from participation in public life is somehow atypical. Within this framework the discussion is vaguely chronological and insufficiently topical. For example, a chapter entitled "The Artist as Pariah," dealing with the rise of Impressionism, is broken in the middle by an account of the events of 1870-71 and the reaction of artists and writers to those events.

The central fact in the cultural life of this period, one of which the author is clearly aware, is the conflict between convention and revolt, but he does not give this collision its

full dramatic value. Both the Second Empire and the Third Republic were suspicious of and hostile to innovation. The academies tried to suppress it by ridicule and ostracism, just as government, somewhat unpredictably, used its powers of censorship when it felt itself threatened. The culture of this age, like that of most ages, is best seen in terms of the simultaneous presence of three currents: an undertow of convention and academicism; an increasingly dominant forward thrust of innovation; and, of course, frothy eddies representing what we call popular culture running athwart and often concealing the main currents of art. The gathering momentum and ultimate triumph of these new forces in literature and painting need organized presentation, not dispersed commentary—especially in the case of drama, which is barely mentioned. Above all it needs to be stated, and restated, that the forging of the modern in France in spite of the apathy and stupidity of authority is one of the great cultural achievements of all time.

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FRANCINE AMAURY. *Histoire du plus grand quotidien de la III<sup>e</sup> République: Le Petit Parisien, 1876-1944*. Volume 1, "La Société du Petit Parisien": *Entreprise de presse, d'éditions et de messageries*; volume 2, "Le Petit Parisien": *Instrument de propagande au service du Régime*. [Paris:] Presses Universitaires de France. 1972. Pp. xii, 649; 656-1352.

In this elaborately produced, extensively documented work, Francine Amaury presents a history of *Le Petit Parisien*, the newspaper with the widest circulation in France during much of the Third Republic. Based primarily on the archives of the Société du Petit Parisien, this account of the now-defunct journal describes the economic and institutional structure of the organization in the first volume and the political role of the newspaper under the Republic in the second.

Founded in 1876 at the moment when the Republic was taking root, *Le Petit Parisien*, after a rather hesitant beginning, acquired a very large circulation by the time of the Dreyfus affair. This impressive achievement was due in part to the able management of L.-P. Piegu (1884-88) and Jean Dupuy (1888-1919).

Both publishers structured the Société so as to retain maximum control over financial and editorial policy.

Of primary importance in explaining the increase in circulation of the newspaper from 33,000 copies in 1879 to 1,453,000 in 1914, to 2,185,000 in 1932 was the publishers' ability to take advantage of existing opportunities. These included improved means of production and distribution, increased literacy and political awareness among the popular classes, and the absence of a competitive press in the provinces.

The primary purpose of *Le Petit Parisien* was to promote the interests of the Third Republic. Founded with the approval of both Thiers and Gambetta, the newspaper was supposed to become an "instrument of propaganda at the service of the regime." While the editors always insisted that the paper was obligated to inform its readers, they nevertheless undertook, by means of effective presentation of the news, "that difficult task of inspiring and guiding" those *nouvelles couches sociales* that Gambetta believed would become the mainstay of the Republic. The management soon became aware, however, that in a democratic society the reading public often influences the editorial policy of a newspaper rather than vice versa. While *Le Petit Parisien* was able to keep its readers well informed about the development of the Boulanger crisis and the Dreyfus affair, it refrained from taking a strong stand against Boulanger and in favor of Dreyfus for fear of offending its clientele. This reluctance to assert itself editorially in the interests of preserving the Republic became particularly apparent during the 1930s when the regime was foundering.

The decline of *Le Petit Parisien* occurred as the Republic itself declined, although not entirely for the same reasons. Production costs, the success of competitors, and the advent of the radio all contributed to the ultimate demise of the paper. Despite its considerable potential for influencing grass roots opinion, *Le Petit Parisien* lost sight of its original goal, and instead of forthright editorials in defense of the Third Republic, the management appeared only too eager to determine which way the wind was blowing during the tempestuous decade of the 1930s. After the collapse of the Republic, *Le Petit Parisien* tried to save itself by

advocating a policy of collaboration, an effort that assured the paper's expiration at the time of the Liberation.

This important work will be of interest not only to students of modern French history but to anyone interested in the role of the popular press in a democratic society.

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LUDWIG ZIMMERMANN. *Frankreichs Ruhrpolitik: Von Versailles bis zum Dawesplan*. Edited by WALTHER PETER FUCHS. Göttingen: Muster-schmidt. 1971. Pp. 299. DM 65.

Zimmermann had an extraordinary opportunity to investigate the archives of the Quai d'Orsay, but the result of his research is only ordinary. He had his opportunity during the 1940-44 occupation when he was assigned to provide the German Foreign Ministry with material appropriate to its purposes. Making notes for his own purposes Zimmermann wrote this study of the Ruhr action from the point of view of a peace-loving liberal. Unfortunately the commonplaces of his language, taking France from "striking while the iron was hot" (p. 13) to "losing the fruits of her Ruhr victory" (p. 272), reflect the commonplaces that dominate his thinking. He has added nothing to the generally accepted characterization of French policy as a destructive failure.

Completed in February 1945, the study remained in manuscript while Zimmermann went on to write his similarly bland *Deutsche Aussenpolitik in der Ära der Weimarer Republik* (1958). The Ruhr book, implicitly critical of German policy in the Nazi period, is necessarily critical of French policy in the twenties, and Zimmermann died before he felt he could publish it. His successor at the University of Erlangen-Nuremberg, adding references to a few other sources, prepared it competently for publication.

Zimmermann's findings provide another argument that the secrets of the archives are rarely important or truly secret. The archives make it clear enough that the French civil and military proconsuls were trying to detach the Rhineland from Germany, but this was being loudly demanded in the French Chamber. As for the man who made policy, Zimmermann



could only show that Poincaré gave his agents ambiguous support at first, and no support when their initiatives began to produce new dangers and absurdities. The author has documented what was, for the most part, well known.

Conceived narrowly as diplomatic history and essentially limited to what Zimmermann found in the files, the book gives little sense of the greater forces behind French policy. The real factors in domestic politics and international economics tend to disappear behind the sheafs of diplomatic messages and the idiosyncracies of Poincaré, Curzon, and others.

The author's failings reduce the value of the book *an und für sich* but make it otherwise useful. Lacking firm opinions, Zimmermann collected his material with a fine lack of discrimination. He had produced an undistorted review of archival sources now unavailable and given other historians their opportunity to exploit that material to more illuminating ends.

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ROBERT O. PAXTON. *Vichy France: Old Guard and New Order, 1940-1944*. New York: Alfred A. Knopf. 1972. Pp. x, 399, xix. \$10.00.

*Le gouvernement de Vichy, 1940-1942: Institutions et politiques*. (Fondation Nationale des Sciences Politiques. Travaux et recherches de science politique, 18.) Paris: Armand Colin. 1972. Pp. 372. 59 fr.

For the past thirty years, the Vichy regime has exercised an hypnotic effect on and an insidious attraction for students of modern French history. It has been terra incognita, with most of the large and even detailed questions concerning it remaining unsolved. Now, in the most impressively researched treatment to date, Robert Paxton, author of an important study of the French officer corps under Vichy, answers these outstanding questions.

Paxton examines his subject from five different points of view: (1) active collaboration with Nazi Germany from 1940 to 1942, promptly, arduously, and enthusiastically pursued by Vichy on the confident assumption that Hitler would quickly win the war; (2) the "National Revolution," for which the Vichy traditionalists, parodying the Thermidorean *incroyables* and *merveilleuses*, furnished the

tone but not the substance; (3) the collaborators themselves, spawn of prewar civil strife; (4) collaboration during the sordid last two years of the regime, now bound *durch dünn und dick* to the Reich because of its origins, inner contradictions, and aims; (5) the "legacy" of Vichy. This final chapter is a superb examination not only of the "breaks and continuities" involved—purge of the "Old Guard" traditionalists at the Liberation, perpetuation of the "New Order" professionals, technicians, and experts—but also of the two essential questions: was France better off under Pétain than a Gauleiter? was Vichy morally justifiable? Paxton's answer to both queries is an unequivocal "no!"; there are times, he concludes, when disobedience to the state is necessary to ensure the survival of the nation, and Vichy was one of those times in French history.

Paradoxically, the book's great virtue—solidly based and scrupulously handled evidence from the German documents—creates a major problem for Paxton, who with one exception was unable to gain access to the pertinent French papers, above all to the incredibly rich dossiers and *instructions* for the postwar trials. The result is that in the analysis of collaboration, the author relies chiefly upon material that tells only the German side of the story and that, as documentary evidence, often reflects only the information that the subordinate thinks his superior wants to read. Without reviving either of the two tiresome apologies for Vichy—the *double jeu* and "sword and buckler" theses—the French documents may well provide an account with more nuances, showing that much more is involved in the collaboration moves than just a frantic desire to become part of Hitler's New Order. Relying too heavily on Warner's flawed account of Laval, and unduly influenced by the revelation in the German documents that collaboration was supported, if not tried, by the entire Vichy hierarchy, Paxton unfortunately diminishes the supreme importance of Laval and Darlan: they were, after all, the two men who promised at various stages to involve France on the side of Germany in war with Britain and even the United States.

In addition, there are various defects in the prologue dealing with the military defeat, the armistice, and the overthrow of the Republic,

and also in the account of the connections between the National Revolution, the Third Republic, and fascism. The legal bases of the Gaullist claims to legitimacy are omitted. More material is needed on the cultural life of both zones, on contacts between Germans and Frenchmen, on the activities of the young French Nazi *ultras*: Jünger, Breker, even Peyrefitte's Manouche come to mind, as well as Benoist-Méchin after 1942, and certainly Lucien Rebatet. Fortunately, some topics that Paxton does not treat at sufficient length, such as the Vichy constitutional acts, the Conseil National, and the Légion Française des Combattants, are examined in detail in the second volume under review, part of the proceedings of a colloquium on the first two years of Vichy held by the prestigious Fondation Nationale des Sciences Politiques in March 1970.

To be sure, many of Paxton's points and judgments are supported by the papers and debates contained in this publication, especially in the conclusion by René Rémond. Some have been reached before in treatments by Scherer, Hoffmann, Juin, Dhers, Marin, Jaekel, Milward, Novick, Michel, Noguères *père* and *fils*, and myself. But they are brought together here with such skill and grace that such earlier general treatments as those by Farmer and Aron are completely bypassed, and such recent neo-Pétainist examinations as those by Isorni, Auphan, Raïssac, Ryan, Tournoux, Bourget, and Blond are entirely discredited: we understand *at last* what Gaullism after 1944 is all about. Paxton's book will remain the definitive work in any language on the subject until the descendants of the Vichy bureaucratic experts, whom he so brilliantly analyzes, finally open the French archives.

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EMILIO MENESES GARCÍA, editor. *Correspondencia del Conde de Tendilla*. Volume I (1508-1509). (Archivo documental español, number 31.) Madrid: Real Academia de la Historia. 1972. Pp. 836.

Until now there was little firsthand information on the social, cultural, and even the political history of Spain in the years 1508 and 1509. With the long overdue publication of Tendilla's

correspondence from a manuscript in Madrid's Biblioteca Nacional, an invaluable eyewitness appears in the person of one of the most important contributors to making that history: Don Íñigo López de Mendoza, second count of Tendilla (later raised to marquis of Mondéjar). Tendilla ruled the former Moorish kingdom of Granada from its conquest by Ferdinand and Isabella (1492) to his death (1515) as its first Christian captain-general and *alcaide* (castellan) of the Alhambra. He had been a renowned general and negotiator in the war on Granada; he was a successful diplomat, responsible for achieving the peace between Naples and the pope in 1486 that opened the era of Spanish hegemony in Italy; he furthered the spread of the classical Renaissance in Spain through his import of Italian humanists, artists, and architects; and he was the father of the famous five Mendoza brothers, who served Charles V (among them Don Antonio, the first viceroy of Mexico, and Don Diego Hurtado, the statesman, poet, and historian), and their sister Doña Maria Pacheco of legendary Comunero glory, who defied that emperor. (Much new information on the siblings' characters and earlier years—and for the first time their correct chronological order—are found here.)

From the cumulative effect of these letters, the impression emerges of a magnetic personality larger than life. For example, if it had not been for Tendilla's not uncritical loyalty to King Ferdinand of Aragon—then barely tolerated in Castile as regent in place of the mad queen Joan, his daughter—the unification of Spain, precarious since Isabella's death, might have broken apart. With persuasion and by example Tendilla brought back into the royalist fold a number of disgruntled Castilian magnates who were on the point of reverting to the anarchy of the previous century; he also managed to keep obedient to the king the many Andalusian municipalities; and, under the persistent threat of a new Moorish invasion, he kept southern Spain safe, even though he had the difficult task of keeping pacified a conquered people of a different creed, culture, and language. Convinced that a people "cannot love [a ruler] of whom they are afraid," he constantly took care to protect the Moriscos (former Muslims who were forcibly converted), the majority of whom were loyal to him, from the

overzealous or dishonest tax collectors of the monarch he supported and from other civil servants and clerics harassing the hard-working people. The numbers of his letters in their behalf to Ferdinand and various members of what amounted to the royal cabinet are legion. To help people in grave trouble Tendilla did not hesitate to confront even the Inquisition. (We find here that, though by the peace treaty the Holy Office had not been allowed into Granada, the Catholic Kings' pledge was in fact circumvented all the time: the inquisitor-general of Córdoba acted as "Inquisitor of Granada" as well. The results, if any, of the count's pleadings do not appear in this volume.)

Reflected throughout Tendilla's letters—in his vivid, individual style—with grace and wit and studded with proverbs are the social aspect of his time, full of curiosities; the economic base; the wide range of personalities of his friends, rivals, and relatives; and his extraordinary gallantry toward women. From his splendid residence in the Moorish royal palaces (he calls Granada "a paradise with which nothing can be compared"), he comments on world events, among them the Castilian conquest of the Peñón de Velez and of Orán, the League of Cambray, and the marriage of Catherine of Aragon to Henry VIII.

Thanks are due Emilio Meneses García for devoting his Ph.D. thesis for the University of Madrid to editing the letters and the Real Academia de la Historia for making this major publication possible. Meneses's biographical introduction, discerning and extensive, covers also the forthcoming final volume 2, where the correspondence will continue to 1513. One looks forward to finding there more of this living history, and one hopes to encounter also the index, regrettably absent here.

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J. A. FABER. *Drie eeuwen Friesland: Economische en sociale ontwikkelingen van 1500 tot 1800* [Friesland over Three Centuries: Economic and Social Developments from 1500 to 1800]. In two volumes. (A.A.G. Bijdragen, 17.) Wageningen: Afdeling Agrarische Geschiedenis Landbouwhogeschool. 1972. Pp. 400; 403-760.

The province of Friesland has long enjoyed a

special attention from Dutch historians. This agrarian and seafaring region on the North Sea has been an object of study, not because of dramatic events or its impact on the rest of the world, but because of what it was, and is. Its settlement patterns, the persistence of its language, its nonfeudal political heritage, and other, more parochial, phenomena have inspired a flourishing school of local historians and provided the subject, through the years, for a long string of historical monographs of a higher quality than is usually associated with provincial history.

The latest of these is J. A. Faber's *Drie eeuwen Friesland*, an economic and social history of Friesland from 1500 to 1800. This thesis is another of the regional studies produced at the Dutch Agricultural University's department of rural history under the direction of B. H. Slicher van Bath. The author, following a pattern established in earlier studies, considers in successive chapters demographic trends, the development of occupational structure, agriculture, major nonagricultural sectors of the economy, and social developments.

Faber does not set out to weave his material into an argument more complex than that Friesland's economy was affected by the same long-term trends as the rest of Europe. Beyond the establishment of this structure—amply demonstrated from parish records, fiscal records, price and rental data, and shipping statistics—Faber organizes his book around the explication of the available archival sources. Quite inevitably, the author's attention is attracted to the relatively abundant eighteenth-century documents; the reader searching for either a narrative account of Friesian economic history or an account with strong theoretical underpinnings will be disappointed by this study, which sticks close to the documents.

If the book has shortcomings, its strengths certainly outweigh them by far, for the Friesian archives possess some remarkably useful materials to which Faber applies skillful analysis. The chapter on agriculture succeeds in establishing the severity of the "depression phase" of 1660-1750. The production of long-term trends for rental values, taxation, and drainage costs confirms the existence of a severe profit squeeze in these decades. The famous epizootics of the eighteenth century,

which literally destroyed the cattle herds on which Friesian agriculture depended, are also the object of careful statistical analysis. Faber's work would seem to deflate the legendary importance attributed to these cattle plagues; only when livestock raising was unprofitable to begin with was the loss long-lasting and economically debilitating.

When, in the last chapter, Faber turns to the province's social history—to questions of class structure, social mobility, and the composition and conduct of the upper class—he makes the most imaginative use of his sources and speaks to issues with the broadest general interest. He is able to confirm that landowning families divided themselves into supporters and opponents of Habsburg rule during the Dutch revolt along the same lines as had their ancestors in the factional strife of the fifteenth and early sixteenth centuries, which destroyed the province's autonomy and placed it within the Habsburg orbit in the first place.

Equally impressive is the analysis of the entry of new wealth into the ruling circles as the old nobility died out during the republican period. Faber shows this process to have been consistent with the growing oligarchical organization of political power, and he is able to conclude that a remarkably stable social structure with little vertical mobility characterized the late seventeenth and eighteenth centuries. It is to this that he attributes the preservation of the Friesian language in the face of the long-standing administrative, clerical, and urban use of Dutch.

When one accepts the limitations imposed by Faber's approach to his material, *Drie eeuwen Friesland* represents a major advance in the historical data available for a uniquely interesting region.

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J. WOLTRING, editor. *Bescheiden betreffende de buitenlandse politiek van Nederland* [Selected Documents on the Foreign Policy of The Netherlands]. Second Period, 1871-1898. Volume 5, 1891-1894; volume 6, 1895-1898. (Rijks geschiedkundige publicatiën, Major Series, numbers 132 and 138.) The Hague: Martinus Nijhoff. 1970; 1972. Pp. xlvii, 786; 1, 846.

The study of late nineteenth-century diplo-

matic history and the world-wide repercussions of the "new imperialism" has already been greatly assisted by the publication of the official Dutch state papers (see *AHR*, 70 [1966-67]: 134-36; 72 [1966-67]: 1409-10; 75 [1969-70]: 519). Now that the last decade of the previous century has become the focal point of the latest bulky volumes, the more limited and parochial (translate as exclusively Dutch) matters tend to be diminished somewhat and the global politics of major nations begin to invade any and all considerations. The far-reaching implications and consequences of Dutch foreign policy in these few years involve several major disputes or confrontations that American, German, British, French, Japanese, and even Russian historians should carefully scrutinize, especially in volume six. Although perfectly understandable, the use of the mother tongue in more than ninety per cent of the documents will cause some difficulty and maybe even keep some investigators away from the collection entirely. It should not do so, especially if source materials are sought on the Boer War, Anglo-French rivalry in East Africa, and both Russian and American interests in the Pacific. At least some fresh perspectives on the prelude and initial commercial and strategic steps of the Spanish-American controversy are included.

Woltring has selected primarily documents that only peripherally touch on issues beyond direct Dutch activity or involvements. In a well-done introduction, which can be read with profit by those who have only a casual interest in these years and their events, the editor carefully but briefly sketches the basic contributions of the contents in the two volumes. An enormous amount of effective work has been done, and it is, in one sense, unfair to comment about the failure to give brief biographical portraits of the main figures or cross references to additional state and related private materials, both of which would have lightened the load of future researchers. It is not inappropriate, however, to note such omissions, since many scholars of non-Dutch history requiring background on major personages and other helpful collections will consult these pages.

The entire study of the hectic era of international tension and overseas power politics of

the imperialist countries is supplemented for the first time in the last volume. This is conveyed in the growing caution of the official Dutch policies, which often stand in direct contrast to the seemingly precarious situations designed by the greater powers. The old and the new intermingled; the long and tortuous Archine War continued, reflecting the traditional and isolated Dutch colonial endeavors of the past, while the nineties introduced serious economic interests that led the Dutch (and other powers) into recurrent diplomatic crises.

These publications have been delayed too long. The impressive results so far will render valuable aid to many specialists. They also suggest that the last period from 1899 to 1919 will probably contain even more significant contributions.

PIERRE-HENRI LAURENT  
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L. DE JONG. *Het Koninkrijk der Nederlanden in de tweede wereldoorlog* [The Kingdom of The Netherlands in the Second World War]. Volume 4, *Mei '40-maart '41* [May 1940-March 1941]. In two parts. (Rijksinstituut voor Oorlogsdocumentatie.) The Hague: Martinus Nijhoff. 1972. Pp. xv, 495; vii, 497-992.

The fourth volume of de Jong's "official history" begins with the establishment of the German administration after the Dutch capitulation on May 14, 1940, and ends with the protest strike of February 1941 in Amsterdam. De Jong chose this period as a unit because its main theme is the failure of the German attempt to win over large segments of the Dutch people to voluntary collaboration with the New Order that Hitler was confident of establishing during his "summer of glory." The February strike of 1941 was a clear and vigorous demonstration of the failure of this policy. For that reason it forms the logical conclusion of the present volume.

The broad outlines of German policies and Dutch responses to these policies have been reasonably well known to students of the period, but de Jong furnishes details and insights that lend richness and depth to the story: How Seyss-Inquart, the *Reichskommissar*, first sought to implement with a certain degree of suppleness and flexibility the assignment given to him by Hitler—to win over the Dutch peo-

ple to the New Order; how the Dutch, after the initial despair and numbness following invasion and defeat, began to rally in defense of traditional values such as loyalty to the House of Orange and the retention of "Dutch" traditions; how the Dutch Nazi party became the target of patriotic condemnation; and how the initial proclivity toward cooperation with the occupying power changed into rejection as a result of a number of German political actions, such as the German support of the Dutch Nazi party (NSB) and the beginning of the persecution of the Jews.

Within these broad outlines this reviewer found many new insights. The chief of these perhaps was the extent to which the Dutch secretaries-general (with only one or two exceptions) wholeheartedly cooperated with the German administration, because they unreservedly agreed with the German demands that law and order be maintained at all costs and that the country should go back to work for its own sake and, if necessary, for that of the German war effort. What is particularly striking is that the two secretaries-general concerned, in this period, even wholeheartedly supported the use of the Dutch police in investigations of resistance activities, with the result that the main resistance group of 1940 (*De Geuzen*) and the main espionage agent dispatched from London (van Hamel) were tracked down by Dutch police.

This cooperative attitude of the secretaries-general is accentuated by the contrast with the firm position taken by the Dutch commander in chief, Winkelman, immediately after the capitulation, particularly in respect to the use of Dutch industry for German military orders; it is also highlighted by the early awareness in the summer of 1940 of the need for a firmer stand on matters of principles evidenced by a number of prewar political leaders.

De Jong adds a great deal to the known picture in his portrayal of the changing positions of traditional party leaders and of the internal development of the Netherlands Union, the unity movement founded during the summer of 1940 in an attempt to thwart the claims to power of the NSB. He brings out the fact that at first Seyss-Inquart and some of his associates hoped that the Union might provide a mass basis for cooperation with Germany, until it

became abundantly clear that the Union was primarily seen by its followers as an organization meant to fight the NSB and indirectly the Germans.

In general, de Jong's description of the workings of the German administration depicts the *Reichskommissar* as a stronger figure than some previous observers have indicated, even though de Jong admits that most of Seyss-Inquart's immediate subordinates had independent bases of power in the Reich satrapies of SS, party, and government. De Jong claims that the understanding between Himmler and Seyss-Inquart was close, even though in Holland Seyss-Inquart steered a careful course between the conflicting interests of the SS and NSDAP. De Jong portrays the military commander General Christiansen even more negatively than previous descriptions, pointing out that he almost never used the potential authority inherent in his position to moderate the political course of the *Reichskommissariat* or of the police. Along this line, this reviewer noted with special interest that Bormann's representative in the *Reichskommissariat*, Schmidt, who sometimes has been portrayed as a "moderate," was in effect the chief instigator, within the German administration, of the Dutch Nazi excesses that directly led to the February strike.

One of the outstanding features of the present volume of de Jong's work is the excellence of the biographical portraits he draws, particularly those of his Dutch compatriots. General Winkelman, the secretaries-general, some of the chief political leaders, and even the German masters are described with a sensitivity and insight that, particularly in the case of the Dutch, it would be hard to excel. De Jong also continues to be able to view individuals and events with a finely balanced judgment that incorporates the world of 1940 and 1941 as it looked then to actors and victims, as well as the "wisdom of hindsight" that has to be applied with such care in dealing with Nazi policies and actions.

Perhaps there is a price to pay for all this richness and detail: at times it seems that the "broad picture" of the period does not come through quite as clearly as it might have in a shorter treatment. Perhaps the author's summary of the first seventeen chapters of the book

at the beginning of the final climactic chapter on the February strike is the result of de Jong's recognition of this problem. Be that as it may, this volume, like the preceding ones, is an invaluable aid to an understanding of this chapter of Dutch history.

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LEO TANDRUP, edited with an introduction and commentary by. *Svensk agent ved Sundet: Toldkommissær og agent i Helsingør Anders Svenssons depecher til Gustav II Adolf og Axel Oxenstierna, 1621-1626* [A Swedish Agent at the Sound: The Dispatches of the Customs' Commissar and Agent Anders Svensson at Elsinore to Gustavus II Adolphus and Axel Oxenstierna]. (Skrifter udgivet af Jysk Selskab for Historie: Number 26.) Aarhus: Universitetsforlaget. 1971. Pp. 694. 85.50 D. kr.

SIGMUND GOETZE. *Die Politik des schwedischen Reichskanzlers Axel Oxenstierna gegenüber Kaiser und Reich*. (Beiträge zur Sozial- und Wirtschaftsgeschichte, number 3.) Kiel: Kommissionsverlag Walter G. Mühlaus. 1971. Pp. xiv, 410. DM 20.

HANS LANDBERG et al. *Det kontinentala krigets ekonomi: Studier i krigsfinansiering under svensk stormaktstid* [The Economy of the War on the Continent: Studies in the Financing of War in the Period When Sweden Was a Great Power]. (Scandinavian University Books. Studia Historica Upsaliensia, 36.) Stockholm: Lärmedelsförlagen. 1971. Pp. xii, 506. 80 S. kr.

The three books refer to Sweden's involvement in the Thirty Years War. The first is a source publication, which consists of a short biography of Anders Svensson, a summary and brief evaluation of his reports, and 550 pages of his dispatches. These are factual and dry, chiefly covering news about Danish policies, military affairs, diplomacy, and occasionally internal power struggles. They include news Svensson gathered about faraway events: wars in Poland, France, the Empire, or about Spanish-English negotiations. A few deal with prices, pestilence, famine, mail service, and—surprisingly and unfortunately only too little—with economic issues such as traffic in the Sound.

The question arises whether instead of the publication of a complete text, an *Inventar* ("Calendar") of the contents of the dispatches (as given in one instance by Tandrup himself, p. 581 n. a) might not have served as well, guiding the historian interested in details to

the original manuscripts. Why print reports full of news that is better known from other sources (the siege of La Rochelle or of Breda; war in Livonia; etc.), rumors that are often wrong, events that are hardly memorable, and lengthy diplomatic formulas as found in all other ambassadorial dispatches? Not all source material is valuable, and good judgment must be exercised in selecting what should be published. The editing is, however, careful, comprehensive, and informative.

The book by Goetze begins, chronologically, where Tandrup leaves off. It offers no source material or new information but is an analysis of Swedish aims in Germany. Goetze emphasizes that not religion but the centralizing policies of the Habsburgs and their maritime plans induced Gustavus Adolphus to intervene. What was right for other countries (Spain, France, England, Sweden) was to be denied to Germany. The king's aim was to finalize Sweden's *dominium maris Baltici*, annex Pomerania, and maintain the "German liberties," i.e., internal division. Like Philip of Macedonia in Greece, Gustavus wanted to erect an *imperium Macedonicum* over Protestant Germany and, possibly, seize the imperial dignity. His plans drove him to ever new military ventures, to plundering and dispossessing Germans in order to compensate Sweden's mercenary officers and soldiers. Naked force, applied *jure belli*, led not only to the destruction of the very liberties he professed to protect but to endless suffering. Oxenstierna, who himself grabbed Mainz, tried after Gustavus's death to maintain the king's aims, at least in northern Germany, while extricating Swedish troops from south Germany. Yet even there he sought to preserve Sweden's dominating political influence and to force the south German princes to pursue the war in his interest and to assume the costs of war. Eventually his policy succeeded, insofar as Sweden retained in the Peace of Westphalia all that really mattered to her in north Germany.

Goetze's well-written and thoughtful work is a valuable correction for the conventional interpretation of Sweden's role in the Thirty Years War. What his book may lack in economic analysis is, at least for certain periods, excellently provided in the third book, which contains three essays.

The first essay, by Hans Landberg, deals with financing war preparations against Poland in 1655; the second, by Lars Ekholm, with financing Gustavus Adolphus's early campaigns in 1630-31; the third, by Roland Nordlund, with financing the war after Gustavus's death when mutiny threatened in 1633. All three show that credit was the crucial factor, notwithstanding Sweden's basic assumption that war had to feed itself. While still preparing for war, with gains only in the future, Sweden herself had, of course, to raise the means. Extra taxes in Sweden were difficult to impose, but royal estates alienated by the nobility were confiscated (*Reduktion*), income from copper and iron exports diverted for mobilization purposes, new tolls levied on shipping, and troops quartered in allied lands. Still this yielded not enough, and therefore rich noblemen, like Leijensköld, de la Gardie, Königsmarck, and other military entrepreneurs had to advance vast sums, and forced loans were imposed on merchants. In 1655 King Karl Gustaf, just as earlier King Gustavus Adolphus and Oxenstierna, personally took a hand in financial matters, whose favorable outcome depended on future victories.

All three essays, accompanied by many statistics, trace in detail the financial transactions. None of them contains complete balance sheets because surviving public accounts are incomplete, and private accounts of men like Leijensköld and Königsmarck are nonexistent. We cannot even guess what the profits (including indirect rewards) or losses were. Costs of armaments and naturalia are rarely treated in the essays. But we learn much beyond what earlier investigators (Heckscher, Redlich, and others) could tell about the activities of financial agents, their demands for securities, the techniques of the loans, the mechanics of money transfers and money drafts, and the bookkeeping methods and budget tricks. We learn where money came from (for example, Amsterdam via Hamburg) and where it went, and we see how a victorious war began to "feed itself." Yet even then, new credit was constantly needed. Confiscations did not suffice—nor did seizing of lucrative offices, appropriating salt and mining rights and other privileges, securing subsidies from France and Holland, or shifting war costs onto the shoulders of foes and allies. The

last measure especially, treated extensively in the third essay ("Krig genom ombud" [War by Proxy]), was unsatisfactory because it made Sweden dependent upon her allies and threatened her overlordship. She was driven ever deeper into war by financial as much as military and political needs, into conquering, seizing, pillaging.

Nilsson's excellent summary of the three essays would have gained even more in usefulness if yet another investigation of war finance could have been added: financing the last phase, demobilization. This task offered new, and perhaps the most complex, problems since future gains by force could no longer be expected. But the scholarly treatment by the three authors—chiefly descriptive and less analytical—adds another dimension to our understanding of Sweden's military actions, politics, and finance in the Thirty Years War.

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*Lærde brev fraa og til P. A. Munch* [Learned Letters from and to P. A. Munch]. Volume 1, 1832-1850, edited by GUSTAV INDREBØ and OLUF KOLSRUD; volume 2, 1. januar 1851-30. september 1859, edited by GUSTAV INDREBØ and OLUF KOLSRUD, completed by TRYGVE KNUDSEN and JENS ARUP SEIP; volume 3, 1. oktober 1859-7. mai 1863; Tillegg, 23. mai 1830-2. november 1862, edited by TRYGVE KNUDSEN and PER SVEAAS ANDERSEN. Oslo: H. Aschehoug & Co. (Vols. 1 and 2); Universitetsforlaget (vol. 3.). 1924; 1955; 1971. Pp. 474; 485; xi, 591.

With the publication of a third volume of "Learned Letters from and to P. A. Munch," the University of Oslo has completed a task begun as long ago as 1919. The first volume appeared in 1924 and contained 253 letters and documents dating from 1832 to the end of 1850. During this period Munch made his historical debut, brought out his early publications, developed new ideas and theories in Old Norse linguistics, and advanced to a professorship at the university. (The first letters to and from C. T. Rafn, among others, are found here.)

The second volume, covering the period from 1851 to October 1, 1859, did not appear until 1955; the task of its editors was a hard one. These were the years when Munch did his best work and had the most difficult time of his life. He felt superior in gifts and accomplish-

ments to his colleagues, especially those in administrative positions, and could not keep this hidden from them. Worse yet, as far as they were concerned, he went out and proved it. As a good historian and a good Norwegian, he made friends and enemies—about one of the former to five of the latter. But he made and kept the friendship of the crown prince, later king of the Dual Kingdom of Sweden and Norway, Charles XV John. It was partly by the aid of the prince that in the summer of 1857 he set out southward looking for historical material on Norway and the Middle Ages in foreign libraries and archives. There must have been many a sigh of relief at his going from those whom he had advised, criticized, opposed, and corrected. The latter half of this volume, which takes him through Copenhagen, Berlin, Vienna, and Venice to Rome and the Vatican Archives, is most valuable.

In Rome he made the acquaintance of Father A. Theiner, secretary of the Papal Archives. Even to Munch what happened seemed almost a miracle. Within a year he was helping Theiner with dating and cataloging; in turn he was aided in his search for new things and had permission to copy all he could find on Norway. No scholar from a land of heretics had ever before been so favored; it would be long ere one was so honored again. To this gnarled Norwegian the hand of friendship was extended and firmly grasped.

When the news got about, numerous other historians from the Protestant lands strove to profit by his success and access. Not one succeeded; even Ranke was shown the door. Munch found, copied, and sent north a tremendous mass of documents, not only to Norway but to Sweden and Denmark as well. Yet his life was a hard one. When Oslo thought of him, it deemed life in Rome easy and living there cheap. He had his family with him, so to stay-at-home bureaucrats it seemed quite natural to cut down on his stipends; eliminate his position as professor, thus saving his salary; and send him his pittance by most irregular and unusual routes and people. At times Munch seems not to have known where the next day's living was to come from, let alone the next week's. Yet in the fine frenzy of the scholar, he kept finding, copying, collating, and sending home invaluable material.



Volume three, published in 1971, ends on May 7, 1863. It contains an appendix of material, dating back to 1830, that was unknown or unavailable when the earlier volumes were edited. There is also a highly satisfactory series of indexes: one of places where the unprinted letters are found and the printed ones are available; one of persons mentioned; a registry of publications; another of places and of matters dealt with; and last of all, a list of corrections, faults in publication, and necessary additions to each volume. The editors and the university might well say *Laus Deo!* for they have done a good work well.

These volumes throw light on much of the mid-nineteenth-century work in Scandinavian history, linguistics, and folklore. There is much on publication, teaching, and travel. It is not easy to transport oneself into an age when the mail between Stockholm and Oslo only went once a week; when it took fourteen days for a letter from London to reach Oslo, and twelve for one from Paris; when a package from St. Petersburg might lie at the customhouse in Oslo for three weeks because no one happened to be going in the direction of the university. There is also much on personalities and problems, for there are things that seem to be eternal in the academic experience. Munch in his day had more than his fill of two of these: the continuous battle with the bureaucrat, whose mind never rises above petty regulations; and the infighting in the ranks of the profession, where *schadenfreude* at the mishaps of a colleague looms larger than pride in one's own accomplishments. Yet there were compensations. When he left for the south on his greatest journey, he was sent from man to man, from place to place, and found courtesy, aid, and consideration everywhere, beyond what he had known or come to expect. There, he was a prophet with honor. Munch was a great man in his own way. This publication honors the university that brought it about; it memorializes a historian who brought it honor; most of all, it honors a man who was an honor unto himself.

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MAGNE SKODVIN. *Norden eller NATO? Utenriksdepartementet og alliansespørsmålet, 1947-1949* [Scandinavia or NATO? The Foreign Ministry

and the Alliance Question, 1947-1949]. Oslo: Universitetsforlaget. 1971. Pp. 355.

In the mid-1960s, Norway's membership in NATO came in for a new and rising wave of public discussion in the newspapers, politically oriented journals, and argumentative books. Some of the most active participants in this discussion were clearly aiming at the separation of Norway from NATO when the North Atlantic Treaty came up for renewal in 1969. In that context the Norwegian government decided to commission Professor Magne Skodvin of the University of Oslo to prepare an account, based on official documentary materials, of how Norway's decision to join NATO was made. The manuscript was to be ready by the fall of 1968, but for various reasons it was delayed.

*Norden eller NATO?* must be regarded as the single most significant book in print about any aspect of Norway's postwar foreign policy. It deals with the most crucial issue Norway has faced since World War II, and it deals with it on the basis of every scrap of documentation concerning that issue which is on file in the Norwegian Foreign Ministry. No sources were withheld from Skodvin, and no restrictions were put on him regarding their use. On the basis of this extensive material, plus other official records and a number of documents declassified for Skodvin's use by other governments, he traces the decision-making process up to that point in mid-February 1949 when Foreign Minister Halvard Lange reached his decision to recommend that Norway join NATO. It is an immensely detailed account, yet it never loses the reader's interest or attention. Here, for the first time, the full inside story of the abortive negotiations for a Scandinavian Defense Alliance is told, as seen from Oslo. Step by step we follow Lange from Karlstad to Copenhagen, to Oslo, to London, and on to Washington. Skodvin makes it crystal clear what information and considerations led to Norway's ultimate choice.

Skodvin's assignment was confined to the decision-making process within the Norwegian Foreign Ministry. He does not go one inch beyond that. Once Lange's decision has been made, Skodvin winds up the account in three brief sentences. One can only regret that he could not have gone further. Other historians must now examine the decision-making proc-

esses within the ruling Labor party, in the government, and in Parliament during the remaining weeks until the treaty was signed in early April. It should be an exciting assignment.

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JÖRG RAINER FLIGGE. *Herzog Albrecht von Preussen und der Osiandrismus, 1522-1568*. Bonn: [the author]. 1972. Pp. 1078.

Whereas Gottfried Seebass, in his study of Andreas Osiander, and Walther Hubatsch, in his biography of Duke Albert of Prussia, have recently aroused the interest of Reformation scholars in two neglected Reformation leaders, Jörg Rainer Fligge has given us a detailed account of the bitter Osiandrian controversy that involved both of these men and threatened to split Lutheranism at a critical time in its development. In his doctoral dissertation, written under the supervision of his adviser Walther Hubatsch of the University of Bonn, Fligge attempts the difficult task of evaluating Osiander's work both in Nuremberg, where he was the leading clergyman during the reformation of that city, and in Königsberg, where he went at the invitation of Duke Albert as professor, preacher, and administrator.

Using the large number of printed works by Osiander, the plethora of polemical literature of this time, and the abundant archival materials in Göttingen (the depository of the Königsberg documents), Nuremberg, and Stuttgart, as well as manuscripts in libraries and archives elsewhere, Fligge provides us with an objective, detailed account of the Osiandrian controversy, not only to the death of Osiander in 1552 but to the end of the polemics concerning it in 1567. This is not a historical account for the general reader but a chronological analysis of the theology of both Osiander and Duke Albert; of the reaction of the Königsberg theologians to Osiander's disputed doctrines of justification, the essence of God, and the presence of Christ in the believer; of the arguments of Osiander's supporters and opponents; of the attempts of Albert to restore religious unity in Prussia and the Holy Roman Empire; and of the decline of the controversy and the victory of Lutheran orthodoxy.

Fligge explains Albert's loyalty to Osiander

by referring to a friendship that dated back to Osiander's religious discussions with the former grand master of the Teutonic Knights during the Nuremberg diets of 1522-24. But Fligge also shows that both men developed their theological views during their early studies in Scholasticism, Neoplatonism, and mysticism, from which they appropriated their emphases on essence and accidents and unity under God, the supreme essence. Albert, he points out, was not a mere dilettante but a well-educated lay theologian who took seriously his responsibilities as a Christian ruler with relatively little interest in social or economic matters. He served not only as an ecclesiastical prince concerned with administrative affairs but as a paternal guide in doctrinal matters, writing theological tracts, confessions, public prayers, and hymns for his people. The book has an exhaustive bibliography, a good index, and helpful illustrations.

HAROLD J. GRIMM

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REINHOLD AUGUST DORWART. *The Prussian Welfare State before 1740*. Cambridge, Mass.: Harvard University Press. 1971. Pp. xi, 328. \$12.50.

This book is a very detailed description of the endeavors of the Brandenburg princes, especially between 1640 and 1740, in numerous fields that the author regards as concerns of "the welfare state." It ranges widely from sumptuary laws to care for the poor; from regulations for the Jews to schools, universities, and academies; from Berlin architecture and public works to sanitation, medical education, and fire protection. Professor Dorwart has taken great pains not to limit himself to his immediate sources, mostly the vast material of printed ordinances; he has broadened his approach to include a description of conditions in the different fields. The result is a compendium that furnishes a great deal of useful information, as I have experienced.

More questionable is the main thesis of the author—the uniqueness of the efforts of the Hohenzollern—and his implicit claim that he deals with the whole of the Prussian state from the sixteenth to the eighteenth centuries. In fact, this book is an analysis of policy for the electorate. Material from outside Brandenburg has been very rarely used. Moreover, it is seen exclusively from above, as if only the princes

counted and as if they could operate in a vacuum. In addition, even rather undetermined rulers like Joachim II or Frederick III are presented in a harmonizing way, as almost on par with the great elector or Frederick William I. Estates, municipalities, guilds, and law courts are mentioned only accidentally. The whole story is presented as if the final stage of enlightened absolutism had already been reached at least a century earlier. Otto Hintze once remarked that a comparison of the Magdeburg Polizeiordnung of 1688 with the Instruction for the General Directory of 1723 reveals in numerous details the transition from "staendische Territorialstaat" to enlightened despotism. Not a trace of the former, however, can be found in Dorwart's presentation.

Similarly, the position of the prince as *summus episcopus* and his influence in Church affairs even after the dynasty had converted to Calvinism are not mentioned. Hence the concern of the rulers between 1650 and 1740 with schools is interpreted as an attempt to establish a uniform, state educational system. It should be stated, however, that Dorwart pays special attention to the influence of Pietism on Frederick William I—which is perhaps the first time that the penetrating studies of the late Carl Hinrichs and his school have borne fruit in a presentation in English. This is not the least of the merits of this informative book.

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ERNST BARNIKOL. *Bruno Bauer: Studien und Materialien*. (Forschungsinstitut der Friedrich-Ebert-Stiftung and the Internationaal Instituut voor Sociale Geschiedenis.) Assen: Van Gorcum & Comp. 1972. Pp. xii, 574. 98 gls.

Bruno Bauer was perhaps the most important of the Young Hegelians. All of his more prominent colleagues in that movement—Hess, Marx, Feuerbach, even Stirner—have already been the subject of full-length studies. But the treatment of Bauer has been surprisingly sparse: a few articles, sections in two general books on the Young Hegelians, and a short German doctoral thesis that is almost inaccessible. With this publication, Bruno Bauer finally gets his due: an exposition of his intellectual history that is the result of a lifetime's scholarly effort.

When Ernst Barnikol died in early 1968, he had already been working on Bauer for some forty years. He had not, however, managed to complete his work, and the present *Studien* were edited posthumously by two of his colleagues. Most of the work consists of twenty-five studies covering the entire period of Bauer's intellectual activity; this is followed by a hundred or so pages reproducing archival material exclusively from the early years focusing on Bauer's difficulties with the authorities from 1840 to 1842. The whole is done with a meticulous attention to detail and an unrivaled knowledge of the archival material.

Forty years is a long time, and it might not be everyone's choice to spend it on a fairly peripheral figure such as Bauer. But any student of nineteenth-century German cultural history must be grateful for having the material so readily accessible. For although Bauer may be peripheral, he is interestingly typical of a whole current of German thought in the nineteenth century. Bauer was the central figure in the secularizing movement of the Young Hegelians and the intellectual mentor of Marx for several years. Barnikol's studies throw light on the genesis of Marx's ideas. This was the period that originally interested Barnikol, but he continues his studies into Bauer's critical liberal comments on the 1848 revolution and further to his conservative support for Bismarck's policies. He also investigates Bauer's pro-Russian views and his general cultural pessimism that shows him to be a precursor of Nietzsche and Spengler. In his evolution from a radical thinker in the Vormärz to a conservative under Bismarck he typified so many German intellectuals. Ernst Barnikol has charted this progression admirably; his editors have done an excellent job on his manuscripts, and historians will be grateful for the resulting definitive work on this minor, but typical, figure.

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KURT RIEZLER. *Tagebücher, Aufsätze, Dokumente*. Edited and with an introduction by KARL DIETRICH ERDMANN. (Deutsche Geschichtsquellen des 19. und 20. Jahrhunderts, number 48.) Göttingen: Vandenhoeck & Ruprecht. 1972. Pp. 766. DM 142.

With the appearance of this volume, one of the

most important sources on German politics during the First World War becomes available to the historical community. In 1914 Kurt Riezler was a foreign service officer attached to the staff of Chancellor Bethmann Hollweg, whose adviser and confidant he became. Riezler kept a diary of his wartime experience but, before his death in 1955, left instructions that it should be destroyed. From that fate it was rescued by the intervention of his friends, notably Theodor Heuss, the president of the Federal Republic, and by the historians Walter Goetz and Peter Rassow, who were consulted by Heuss and recommended preservation and publication. Riezler's brother, who was appalled by the acerbity of some of the references in the diary to persons still living, resisted that advice, and it was only after his death, when Riezler's daughter, Mrs. Howard E. White, permitted K. D. Erdmann to use the diary in connection with a study of Bethmann that appeared in 1964, that any part of its contents became public. The Erdmann essay aroused so much interest among historians that there was an insistent demand for access and publication, to which Mrs. White finally yielded in 1968, authorizing Erdmann to prepare this critical annotated edition.

Since Bethmann Hollweg's own papers were destroyed in the course of the Second World War, the Riezler diary provides us with the best evidence that is available concerning the chancellor's mood at critical moments during the first world conflict: the deep pessimism concerning the spiritual state of the German people, for instance, that was one of the motivating factors in his decision to risk war in the hope of breaking out of the ring of encirclement; the mixture of horror and fascination that Russia inspired in him, which perhaps distorted his judgment in July 1914; and the strain of fatalism that deepened as the war proceeded and appears to have disarmed him in his running fight with the soldiers. Riezler's notes on his frequent conversations with Bethmann are also enlightening on such subjects as the evolution of Germany's war aims, the chancellor's views on the postwar position of Belgium and Poland, and his calculations at different times about the possibility of negotiated peace.

Finally, the diary, despite its hurried and

fragmentary style, is more effective than many more systematic accounts in recreating the atmosphere of wartime politics, in which the enormity of the issues at stake, the constantly shifting situation on the war fronts and the lack of precise knowledge concerning it at any particular moment, and the disorganization of the government and the number of competing agencies vying for position made rational decisions virtually impossible and put power in the hands of the most confident and unscrupulous party, the soldiers. It was this that defeated Bethmann, this and the German people's lack of political sophistication. "*Gesamttragik des deutschen Volkes*," Riezler wrote after the chancellor's fall, "the blind faith in power. Who makes the gesture gets it. Bismarck slavery. Bülow theater. Hollow facade" (p. 448). And later, in April 1918, Riezler wrote that "the *milites gloriosi* are all-powerful, supported by their successes, the unmentionable military piety of the people, the propaganda machine of the *Vaterlandspartei*, etc. . . . Civil servants and journalists wholly befogged by four years of censorship, lies, and dissimulation. The great *ruere in servitium* begins, as Bethmann used to say, after Tacitus" (pp. 459-60).

Riezler's own thinking about Germany's wartime problems and her postwar position was elaborated in 1916 in a series of articles that appeared in the *Europäische Staats- und Wirtschaftszeitung*, a selection of which Professor Erdmann has included in this volume. After Bethmann's dismissal, Riezler returned to the foreign service, first in Stockholm, where his negotiations with Bolshevik agents helped prepare for the Brest-Litovsk conference, then in Moscow, where he narrowly escaped death in the *attentat* that killed Graf Mirbach, and finally in the political department of the foreign ministry. He retired from the service in June 1919 in protest against the government's signing the Versailles Treaty.

The Stockholm and Moscow phases of Riezler's career are treated in his diary, which is supplemented by the inclusion of a number of official dispatches and memoranda and an account of the Mirbach assassination that he gave to a correspondent of the *Berliner Tageblatt*. His later activities—particularly his distinguished service as head of the *Kuratorium*

of the University of Frankfurt from 1928 to 1933 and his professorship at the New School for Social Research in New York after Hitler's accession to power—Professor Erdmann discusses in a comprehensive and satisfying biographical essay, which also includes an analysis of Riezler's principal scholarly works in the fields of political science, philosophy, and esthetics.

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HAROLD J. GORDON, JR. *Hitler and the Beer Hall Putsch*. Princeton: Princeton University Press. 1972. Pp. xii, 666. Cloth \$19.50, paper \$8.95.

Hitler's attempt to stage a putsch in Munich in November 1923 has not lacked historians. There is, however, room for an up-to-date, comprehensive account in English that offers a detailed chronology and puts the episode into historical perspective. Professor Gordon's volume is the outcome of prodigious industry and of a combing of archives that is unmatched by any of his predecessors. It is written in the snappy, gladiatorial style familiar from his earlier works. No future historian of Bavaria or of Germany will be able to ignore it, but he may well find it hard to accept many of the author's interpretations.

Gordon's general conclusion is sound: that the putsch, far from being a milestone on a march to power, was the end of the period of latent civil war since 1918 (p. 615); that the putsch left Hitler in undisputed control of the previously fragmented Right-radical movement (p. 603); that Hitler was further strengthened because outside Bavaria the Right-radicals lost most of their freedom of action (p. 604); and that Hitler's failure restored to Bavaria stable parliamentary conditions that lasted longer than almost anywhere else in Weimar Germany (p. 547)—a fact of which Gordon might have made more.

The central part of the work, however, is a reconstruction, as painstaking as it is polemical, of the events of November 8–9 and the background to them. His sections on the composition of the Nazi party and the organization of the army, though detailed, do not add greatly to the accepted picture. This is true

even of his emphasis on Nazi recruitment from the Left and/or the working class, which is surely old news by now. The section on the ever-shifting host of "partiotic bands" is more helpful, and events bore out his verdict that the moderately conservative and monarchist bands were more numerous and more influential than the radical ones. The section on the organization of the police, its political role, and the personality of the head of the *Landespolizei*, Colonel von Seisser, renders a major service: their choice of roles was even more decisive in November than that of the army.

The section on the Bavarian state crisis, culminating in the beer hall fracas, covers over two hundred pages. As a blow-by-blow account of the maneuverings and the fighting, and in particular of the twenty-four hours of the putsch itself, it could scarcely be bettered. If its overall effect is ultimately not satisfying, that is because the narrative does not elucidate the motives and *arrière-pensées* of the admittedly devious triumvirate whom Hitler sought to push into a coup—Kahr, Lossow, and Seisser. Gordon is inclined to assume that none of the three was prepared to go along with Hitler from the moment they were "hi-jacked" at their Bürgerbräu meeting, and that they delayed showing their hand only for tactical and technical reasons. This hypothesis is now impossible to disprove, but the documentation of Deuerlein, Carsten, and Hofmann, on whom Gordon is very hard, suggests that alternative interpretations should be dismissed less cavalierly.

Gordon is able to take this line in part because he emphasizes the antagonism between the Bavarian authorities and the Nazis, playing down, though not ignoring, the overlap in aims and methods and the authorities' antagonism to Berlin and the Republic. This, in turn, rests on an evaluation of the Nazis as a rabble of hotheads who had more in common with the SDS of Columbia University than with the "blue-white" establishment. If it is true that the Nazis were not conservative because there was "not a single institution that Hitler and his circle wished to conserve" (p. 5), much of what Gordon argues follows. But if not?

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LUTZ NIETHAMMER. *Entnazifizierung in Bayern: Säuberung und Rehabilitierung unter amerikanischer Besatzung*. [Frankfurt am Main:] S. Fischer. 1972. Pp. 710. DM 68.

Denazification in the American zone of occupied Germany was a monumental failure. Anyone who still harbors doubts on this score would do well to read Lutz Niethammer's massive and authoritative monograph, a somewhat enlarged version of a dissertation completed at Heidelberg under Werner Conze in 1971. Based on the records of the U.S. military government, the denazification tribunals, and the German governments in the U.S. zone, as well as on the papers and personal testimony of many key participants in the denazification program, *Entnazifizierung in Bayern* is not only the first comprehensive study devoted exclusively to this controversial subject but also the first book that presents both the American and the German side of the story.

Proclaimed at Yalta and Potsdam as one of the principal objectives of Allied postwar policy, denazification was intended to rid the conquered Reich of all vestiges of National Socialism. Implicit in the Allied declarations was the view that all but the most nominal Nazis and supporters of the regime should be pinpointed, purged, and punished. In the final analysis, however, only the American occupation authorities, strongly influenced by political pressures in the United States, took this concept seriously enough to act in accordance with it. But, as its proponents quickly discovered, denazification was more easily proclaimed than achieved. After twelve years of Hitlerian rule, Germany was more thoroughly "nazified" than most outsiders had realized. Whether out of conviction or out of expediency, millions of Germans had joined the NSDAP; many more had become members of affiliated party organizations. The sheer magnitude of the task alone made denazification by purge problematical. Moreover, as Niethammer's representative sampling shows, the mass arrests, internments, and dismissals that characterized the early stages of the program affected mostly the small fry, while many ranking Nazis and many of those involved in actual criminal acts managed to evade the net.

And yet Niethammer argues convincingly that denazification was not necessarily doomed

from the outset. There was in fact no single, well-thought-out plan that had the support of all those concerned with the program. The lack of unanimity on goals precluded uniformity in procedures. But just as there were many points of conflict and confusion, so too there were numerous opportunities for transforming denazification into a constructive and potentially successful operation. How and why these opportunities were missed are questions that the author examines with admirable thoroughness and objectivity. One major problem, Niethammer contends, was that the Americans sought to achieve fundamental political reform without significant social change. But their chosen instrument, the "personnel purge," was at once too sweeping and too restricted to elicit the support of the indigenous political forces whose cooperation was essential to the success of the denazification program. For those on the German right, the U.S. effort was too radical; for those on the German left, it was not radical enough. The conflicts engendered by these differing views, their day-to-day effects on the denazification process and on the internal politics of Bavaria, and their long-term effects on the political and social development of postwar Germany are meticulously dealt with in this book.

In the end the more conservative forces had their way. When, in 1946, the military government transferred the denazification program to the local German authorities, it was gradually transformed from a process designed to purge and punish into one designed to rehabilitate and reinstate. To be sure, Niethammer, after studying several hundred individual cases, concludes that things got somewhat out of hand; and, therefore, the rehabilitation process went considerably further than its German proponents had initially intended. In fact, well over ninety per cent of those brought before the German tribunals were adjudged to be "followers," and thus in effect amnestied. In the final analysis, then, denazification satisfied no one—except of course the real culprits, for whom it served as a smoke screen under whose cover they not only escaped retribution but also managed to creep back into public life.

This volume is a tragic monument to a misguided and mismanaged policy; it is also a triumphant monument to historical scholar-

ship. If it is not widely accepted as the definitive account of denazification under the American occupation, I shall be surprised.

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ALAN PALMER. *Metternich*. New York: Harper and Row. 1972. Pp. vii, 405. \$12.50.

This new biography should render obsolete the score of books of the same kind and format produced since Von Srbik's famous blockbuster. The narration follows closely the thread of chronology, cleverly weaving political achievements with events of private life and glimpses of Metternich's ideology and character. The ordinary reader will be delighted with the smooth swiftness of the story and the sprinkling of witticisms, while the professional historian will be gratified to find, whenever he may be curious, footnotes referring to sources and various authorities. The latter include, in addition to the well-known classics of the ample literature on Metternich, most of the recent publications, among which the most meaty is the correspondence with Wilhelmina von Sagan, published by Maria Ullrichova in 1966; among the notable books used by the author are those by Henry Kissinger, Paul Schroeder, Enno Kraehe, Arthur Haas, Donald Emerson, Manfred Botzenhart, and David Ward. Unfortunately, Mr. Palmer seems not to have been aware of the existence of my own recent contribution, *Metternich et la France après le Congrès de Vienne* (1968-71), nor of Irby C. Nichols's *The European Pentarchy and the Congress of Verona* (1971). Therefore his account shows some weaknesses in this direction; especially when interpreting Metternich's maneuvers in the congresses of Aix-la-Chapelle, Troppau-Laibach, and Verona, not to mention a number of petty details that cannot be discussed here. This is the more to be regretted as, on the whole, the book deserves to, and will probably, remain for a long while the standard one-volume biography of the famous statesman.

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WOLFGANG ROSAR. *Deutsche Gemeinschaft: Seyss-Inquart und der Anschluss*. Vienna: Europa Verlag. 1971. Pp. 441.

This book by a student of Ludwig Jedlicka exemplifies the trend among historians of modern Austria to free themselves from the influence of prewar polemics. Focusing on the oft-maligned quisling of Austria, Rosar draws a portrait of the "Anschluss man," an ideological type common to Austria's middle-class leadership before 1938. Based on careful research in German and Austrian archives, Rosar's findings often concur with Seyss-Inquart's own Nuremberg testimony.

Central to Rosar's interpretation is a strong emphasis on the generational kinship between men whose Weltanschauung was shaped by World War I and its immediate aftermath. Members of the "front generation"—including Dollfuss and Schuschnigg—shared a *Grossdeutsch* patriotism and differed less over the desirability of *Anschluss* than the tempo at which it should occur. This kinship, Rosar contends, explains the relationship of trust that existed between Seyss-Inquart and the leaders of the Fatherland Front. Also important was the strategy that Seyss developed as a member of the *Deutsche Gemeinschaft* (a nationalist fraternity to which Dollfuss also belonged), based on the reconciliation of the Catholic and anti-clerical nationalist camps. The complex relationship between Seyss, the Fatherland Front, and National Socialism is carefully analyzed. Cautious by temperament, Seyss opposed violence, but he also believed that Austria could slowly move toward union with Germany while time would temper the Nazi's revolutionary élan. At the time, Rosar argues, this optimism did not appear unfounded since before March 12, 1938, neither the German diplomats, the party, nor Hitler himself ever developed a coherent strategy for *Anschluss*. Particularly valuable is Rosar's account of party rivalries in Vienna and Berlin and their influence on the *Anschluss* movement. In the end Seyss-Inquart was certainly used by Göring and others, but the notion that he was merely a tool, Rosar demonstrates, oversimplifies the truth.

Unfortunately, the final chapter lacks an adequate summation, and we must be satisfied with the rather weak conclusion that, while inwardly Seyss never accepted *Gleichschaltung*,

"*reservatio mentalis* alone does not absolve responsibility for events." There is no attempt to relate Seyss's experience as an *Anschluss* leader to his later activities as an occupation authority. Moreover Rosar's list of secondary sources is thin—notably missing are Dieter Ross's *Hitler und Dollfuss* (1966) and Jürgen Gehl's *Austria, Germany, and the Anschluss* (1963). But these weaknesses do not undermine the value of a book that suggests fresh approaches to the history of interwar Austria.

HARRY R. RITTER

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KLEMENS VON KLEMPERER. *Ignaz Seipel: Christian Statesman in a Time of Crisis*. Princeton: Princeton University Press. 1972. Pp. xvi, 468. \$17.50.

Four decades after his death Ignaz Seipel has found a biographer who has grasped his significance as an Austrian and European statesman and, at the same time, recognized his personal limitations. Professor Klemens von Klemperer of Smith College, author of *Germany's New Conservatism* (1957), has mastered this complex task. He has done such a thorough job of searching for relevant material in Austrian and German governmental and Church archives and in private collections of "Seipeliana," that there will be no need for another Seipel biography for many years to come.

On the whole, this is a fine piece of scholarly writing; it might have had greater human appeal had he given more color to the chapter on Seipel's youth and his formative years in the priesthood. But a fair, well-balanced picture of him as a statesman emerges as one reads on. Klemperer's judgment on Seipel and his collaborators is usually discerning; only the author's condemnation of Felix Frank (pp. 323-24), the Austrian minister in Berlin, does not seem called for to me.

To Americans, Seipel represents a rather unfamiliar type: the statesman in a soutane. Actually, Seipel would have made a spectacular prince of the Church; he was considered repeatedly for high positions in the hierarchy, and in his last months, already very ill, he toyed with the dream of becoming archbishop of Vienna and cardinal. But for all his religious devotion and his thorough grounding in

the moral teachings of his church, he was a "Christian statesman of the school of a Richelieu," as Klemperer correctly observes (p. 267). Seipel's Socialist adversaries even called him, after the bloody riot in Vienna (July 1927), the "Prelate without mercy."

His turn to the right in those fateful days is somewhat puzzling, for he had started out as a spokesman of the liberal wing of Austrian Catholicism. Although a monarchist at heart, he had served the new democratic republic loyally as leader of the Christian Socialist party and as chancellor. Because of his able diplomacy, Austria gained in 1922 financial stability by a large loan under the auspices of the League of Nations. From then on, he was an influential figure in European power politics. He was not enthusiastic about close ties with Germany, but he did not rule out the *Anschluss* at a later date. In an authorized interview of February 1926, to which Klemperer refers (p. 305) because it aroused considerable disturbance in Paris, Seipel quoted to me Gambetta's words that one should always think of it, but never talk about it. The lasting impression he made on me in that conversation coincides with the picture drawn by Klemperer: a calm, dignified, friendly, yet somewhat remote personality, a statesman of wide horizons and with a clear perception of Austria's place in Europe.

In October 1926 Seipel returned to power, but his second chancellorship was much less successful than the first. There was a fundamental change in his attitudes. He failed to strengthen the democratic basis of the republic, largely because of his unbending hostility against the Socialists, a feeling their leader Otto Bauer reciprocated. After Seipel's sudden resignation in April 1929 he turned further and further to the right, embracing the support of the fascist *Heimwehr* and advising his German political friends to take the Nazis into the government. Clearly he had reached the end of his road. Klemperer is right when he concludes that "Seipel was not a villainous, but a tragic figure in history."

FELIX E. HIRSCH

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*La fine del potere temporale e il ricongiungimento di Roma all'Italia: Atti del XLV Congresso di storia del Risorgimento italiano*



(Roma, 21-25 settembre 1970). (Istituto per la storia del Risorgimento italiano. Biblioteca scientifica, Atti dei Congressi, volume 13.) Rome: the Istituto. 1972. Pp. xix, 677.

Under the indefatigable leadership of Professor A. M. Ghisalberti, the annual congresses of the Istituto per la Storia del Risorgimento have for some time focused on appropriate (usually centennial) anniversaries. The forty-fifth congress, held in September 1970, was a culmination of the series, held to commemorate the annexation of Rome by the Italian kingdom. The event that in 1870 disgusted Mazzini, embittered Catholics, and won excommunication for Italy's leaders was celebrated in 1970 in an atmosphere of good feeling. It opened on the Campidoglio with greetings from the president of the Republic and closed four days later with a quotation from Pope Paul—perhaps the most splendid, most truly international, and least controversial of the Risorgimento *convegni*. The much-studied Roman question, once a classic of historical dispute, has apparently lost its power to divide.

The first long paper, by Father Roger Aubert, "L'Eglise face au problème de Rome," was followed by a general discussion and then by separate papers on attitudes toward the temporal power in Great Britain (Noel Blakiston), France (Jacques Gadille), Germany (Rudolf Lill), Italy (Giacomo Martina), Latin America (Carlos M. Rama), the Slavic countries (Angelo Tamborra), and Austria (Adam Wandruska). Father Aubert's essay, balanced and suggestive, places the intransigent determination of Pius IX against the background of the very diverse traditions and perspectives within the Church, and Aubert calls for closer research into the varieties and shifts of Catholic opinion. His point is reinforced by the papers on the separate countries, for in each case national traditions of politics and political thought, specific conflicts between Church and society, and particular social tensions dominate the controversy. What now attracts historians more than the issue of papal sovereignty is the insight that controversy can provide into the social fabric it once threatened to tear. Having put the question, these Olympian surveys stop short of the answers; the pleas of Pius IX were just one more strain in the domestic life of separate nations.

A related view is developed by Alberto Acquarone in a handsomely woven and subtle essay on Italian politics and the Roman problem. Taking as his theme the concern for a moral as well as political regeneration that ran throughout the Risorgimento, Acquarone underscores every faction's intense concern with Rome and the durability of the compromise solution painfully launched in 1870. In the following session, noted experts discussed in separate papers the position on the Roman question taken by the major powers: Germany (Karl Otmar Frhr. v. Aretin), Great Britain (Max Beloff), Italy (Ennio di Nolfo), Austria (Frederick Engel-Janosi), France (Pierre Guiral), Spain (Jesús Pabón, in 150 pages), and the United States (Dragan R. Zivojinović). At the end of the conference Adam Wandruska quoted the contemporary comment by Gregorovius about the taking of Rome: an event that in other circumstances would have upset the whole world passed as a little episode in a greater drama. These informed surveys of separate countries confirm the extent to which the Franco-Prussian War and domestic strife everywhere overshadowed events in Italy.

The conference closed with papers by Fiorella Bartoccini, a preview of her sensitive study of life in papal Rome; by Vittorio Frosini on juridical aspects of the Roman question; and a concluding statement by Ghisalberti in which scholarly and rhetorical traditions of Risorgimento study are elegantly reconciled. The papers in this volume, like the ceremonial occasion that sponsored them, allow few surprises; but they will, with their broad perspectives and bibliographic notes, prove useful to any student who wishes to reopen for himself questions that many once thought could never be resolved.

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S. J. WOOLF, editor. *The Rebirth of Italy, 1943-50*. (Centre for the Advanced Study of Italian Society, University of Reading.) New York: Humanities Press. 1972. Pp. vi, 264. \$9.50.

The old dictum "publish or perish" should not be taken literally by neophytes in the profession. "Publish and perish" is quite possible,

if the book is a poor one. Authors of *The Rebirth of Italy, 1943-50*, please take note.

The writing of recent history is, we all know, fraught with many dangers, but they are compounded when nine different persons express their views on nine different themes, when a period of seven years is chosen for analysis that has so many years after it that everyone knows how the conundrums were answered, and when the authors seem miffed at the way things turned out.

One saving grace of the book is that it has a central theme, a theme that is especially well stated by the editor in the concluding chapter of the volume. "The 'failure' of the Resistance was bitterly noted within months of the final liberation of Italy and soured the political activities of a significant portion of the partisan generation. There was no revolutionary transformation of Italian society, the parliamentary system seemed a mere restoration of that following the First World War, and the state remained deeply marked by fascist and pre-fascist traits, features which were accepted, even accentuated by the now dominant Catholic party—a monarchial republic of priests . . ." (p. 213).

In addition to having a common theme the authors appear to have a common feeling that Italy had a chance to go left after the war and that the missing of the chance was a national misfortune. The first chapter, by Professor Quazza of Milan University, explains how the Resistance failed to be a revolution, and the last chapter, by Professor Woolf, director of the Centre for the Advanced Study of Italian Society at Reading University, England, describes how the entire evolution in this period worked against the realization of left-wing goals. The other authors have their own variations on this theme, blaming the Church, big business, foreign pressure, and/or lack of political sophistication for the fact that Italy created a democratic republic.

In any cooperative endeavor of this kind, some chapters are inevitably better than others. I was favorably impressed by "The Rebirth of the Party System, 1944-48," for the author is convincing in showing how conflicting ideologies and power groups on the left split their forces and how the Christian Democratic party was able to hold its divergent elements to-

gether in such a fashion that it could become the rallying point of the moderates. On the other hand, I was particularly unimpressed by "Economic Policy in the Reconstruction, 1945-51," for it deals almost exclusively with the problem of inflation, damns the policies of Luigi Einaudi out of hand, and condemns Italy's efforts to bring its foreign trade into balance instead of "milking the golden cow," meaning getting more American aid.

In spite of all the arguments and evidence presented here, I could not help but have my opinion reinforced by what I read—that Italy created a democratic republic because that was the form of government which the majority thought would best serve its interests. Italy allied itself with the Western powers, rather than with the Eastern, not because of the intervention of the Church, the pressure of the United States, or the machinations of a small coterie of politicians, but because it believed that its security would thereby be best assured. Clearly there is a history of Italy in the postwar period that is different from that seen through red prisms.

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JOHN O. IATRIDES. *Revolt in Athens: The Greek Communist "Second Round," 1944-1945*. With a foreword by WILLIAM HARDY MCNEILL. Princeton: Princeton University Press. 1972. Pp. xiv, 340. \$11.50.

John O. Iatrides has written an adequate summary of what has been revealed by British, American, and Greek sources since 1945 about the Allied Powers and their involvement in Greek affairs during the Second World War. Given the special place of Greece in the current reassessment of the origins of the cold war, the failure of this book to penetrate to the essential issues of the "Second Round" of the Greek civil war is particularly disappointing. The absence of a critical historiographical introduction to a highly controversial subject weakens the revisionist position suggested by the author's documentation—which he himself has been too shy to emphasize. The considerable polemic literature that has appeared during the last generation has yet to be subjected to the serious criticism it deserves. Iatrides reviews at length the complicated relations,

dominated by the question of the return of the unpopular Greek king to his throne, between the British government, the Greek government in exile in the Middle East, the Greek king in London, and, occasionally, the Communist-dominated resistance (EAM-ELAS), which by 1944 was in virtual control of Greece. A notable contribution of the book is its examination of the peripheral role of the United States and especially the inclusion of the perceptive and often prophetic reports of its ambassador, Lincoln MacVeagh, to the exiled government.

Like the sources he heavily relies on, Iatrides fails to give adequate notice to the character of the EAM-ELAS coalition and, in general, to the political situation in Greece. While the exiled politicians were consumed with Churchill's firm resolve to return the king to his throne, thereby assuring Greece's place within the British sphere, the EAM-ELAS managed to establish a remarkably effective working government under the Axis occupation. The author's objective was to examine the causes of the battle of Athens, which had been precipitated, soon after liberation and the landing of British troops, by a violent confrontation between unarmed civilians and the Greek police. The immediate issue had been the British demand that the EAM-ELAS forces unilaterally disarm. Iatrides's conclusions are that both sides were unsure of each other's position and their own immediate objectives and that all sides found themselves in a bloody struggle no one wanted. This position, misses the point and, furthermore, contradicts the evidence Iatrides has cited at length. Churchill, it is clearly shown, never hesitated for a moment in pursuing, despite "all costs," his objective of a liberated Greece under British auspices. On the other side, Iatrides introduces a document, the one new piece of evidence in the study, that helps to confirm what has been suspected all along: that the Communists never intended to take power by force and that they were in fact committed to a "strategy of legalism." The substance of this captured document should have been the subject of this book. The key to understanding the Greek civil war cannot be found in London, Cairo, or Washington alone, but in Athens and the Greek countryside. Both the EAM and the Greek Communist party it-

self were amalgams of political, social, and regional groups that tried in vain to reach an accommodation with the British and later the Americans; their story has yet to be told.

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ȘERBAN PAPACOSTEA. *Oltenia sub stăpînirea Austriacă (1718-1739)* [Oltenia under Austrian Rule (1718-1739)]. (Academia de Științe Sociale și Politice a Republicii Socialiste România, Institutul de Istorie "N. Iorga." Biblioteca Istorică, number 23.) Bucharest: Editura Academiei Republicii Socialiste România. 1971. Pp. 342. Lei 22.50.

This monograph deals with the history of that portion of Romania's territory which was separated from Wallachia by the Austro-Turkish treaty of Passarowitz (1718) and incorporated within the Austrian Empire until 1739. The province, Oltenia or Little Wallachia, was handed back to Wallachia by the treaty of Belgrade.

The abundance and exceptional value of the documentation concerning Oltenia left by the Habsburg authorities has been in part exploited by Romanian historians for several decades. However, a great deal of the material hitherto unresearched was investigated and analyzed by the author. Added to previous studies, the present investigations have allowed him to describe the whole period under its complex, ample, and unitary aspects. Șerban Papacostea pays particular attention to the population problem, and implicitly to Austrian demographic policy; interprets the economic life of Oltenia, pointing out the dominant agrarian character of the region; analyzes the status of the classes and the categories within each class; and presents the fiscal structure, the administration, and the judiciary with rich supporting documentation. Linking all these he stresses the confrontation between the efforts of the Viennese court to subordinate the province and the struggle of the Oltenian boyars to save their autonomy and preserve their power. He also demonstrates the forms of the class struggle of the peasantry, and its consequences. The peasantry, caught between two exploiting tendencies, had recourse to mass immigration, thereby temporarily succeeding in upsetting the network of the fiscal and social exploiting institutions. This compelled the

Austrian authorities to a constant re-adaptation of governmental solutions.

The analysis of the centralizing system imposed on Oltenia by the Austrians, a system very different from the traditional boyar state prevailing in the Romanian principalities at the time, opens up new perspectives on terms of historical research, for example, a new interpretation of a series of reforms introduced by the Phanariot princes of Moldavia and Wallachia after the re-incorporation of the Oltenian territory. Papacostea has completed a historical monograph, in its full sense, that focuses the attention of foreign scholars as much on the author as on the problem treated.

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N. M. LEBEDEV. *Pestel': Ideolog i rukovoditel' dekabristov* [Pestel': The Ideologue and Leader of the Decembrists]. Moscow: Izdatel'stvo "Mysl'". 1972. Pp. 341.

It is reasonable to assume that any writer who undertakes to publish a new study of P. I. Pestel would be mainly motivated either by the uncovering of new sources or by an opportunity to arrive at original conclusions. Lebedev offers neither one nor the other. The author presents a true illustration of firmly established Soviet semantics with all its amorphous terminology. The work is another example of the common literary pattern with its standardized censure, approvals, and deductions so often found in Soviet historical literature.

Much has been written on Pestel, from A. I. Herzen and G. V. Plekhanov to more recent writers such as N. M. Druzhinin or B. E. Syroechkovsky. To this imposing list of publications N. M. Lebedev adds another biography of the "Decembrist ideologue." It is a lengthy monologue written with the confidence of a "party-liner" and overlaid with carefully chosen references, beginning with the epigrammatic quotation from Marx and followed by frequent citations from Lenin. The narrative is strewn with colloquialisms and hollow phrases that add little to the illumination of the secretive, intriguing personality of Pestel. The writings of Pestel or his testimony cast insufficient light either on his leadership or on his role as the ideologue of revolution. The effusive gusto

of Lebedev's style lacks methodical analysis, is void of analytical depth, exposes partiality, and frequently needs supporting evidence. A single instance may suffice. The author regards the appointment of M. M. Speransky to the investigation commission of the Decembrists as a "personal tragedy" for the aged statesman. The emperor, presumably, named Speransky to test his monarchical loyalty. This has been said by other writers; Lebedev repeats this version, yet it still lacks specific proof.

All in all, Pestel still remains the complex enigmatic personality he always has been. He is often referred to as the Russian Robespierre, as the pioneering advocate of a state dictatorship, and even as the earliest ideologue of the Soviet system. All this may be speculated upon, but the pedestrian, day-to-day account offered here presents little convincing proof. In the end, Pestel still stands as the enigmatic, rebellious aristocrat that he has been for nearly a century and a half in historical literature.

ANATOLE G. MAZOUR  
Stanford University

H. MONTGOMERY HYDE. *Stalin: The History of a Dictator*. New York: Farrar, Straus and Giroux. 1972. Pp. xv, 12-679. \$12.95.

Using a technique that has served him well in writing biographical studies of literary figures (for example, Henry James and Oscar Wilde), Mr. Hyde has undertaken the more formidable task of writing a biography of Stalin in accordance with the principle *se non e vero, e ben trovato*. If a story or anecdote appeals to him he uses it, even though it may come from a source known to be a forgery (the pseudo-Litvinov *Notes for a Journal*) or a work of dubious authenticity (the so-called Khrushchev memoirs, against which, as Mr. Hyde disarmingly informs us, Robert Conquest warned him). A number of colorful but improbable details have been gleaned from a biography of Stalin written by Yves Delbar (1953), which in turn was based in part on materials supplied by Gregory Bessedovsky, the slippery former Soviet minor diplomat whom Bertram D. Wolfe identified as the principal concocter of the Litvinov forgery.

Diligent as well as indiscriminating, Mr. Hyde on occasion has turned up interesting,

unhackneyed material, notably in the field of Soviet foreign policy during the thirties and the Second World War, periods for which British diplomats and memoirists have contributed some vivid firsthand observations of Stalin. Unfortunately, Mr. Hyde makes little attempt to use these nuggets as the basis for a coherent analysis of Stalin's character or to relate them causally to his career.

The book is worth reading for the occasional quotation one may have missed and for its engaging narrative flow, but it adds little to our knowledge or understanding of Stalin and Stalinism. In analytical depth it lags far behind the old but still unsurpassed biography of Stalin by Boris Souvarine (1939), which has recently been reprinted (1972), though without the extensive bibliographical notes that constitute one of the most valuable features of the original French edition (1935).

ROBERT M. SLUSSER

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A. A. GROMYKO *et al.*, editors. *Dokumenty vneshnei politiki SSSR* [Documents on the Foreign Policy of the USSR]. Volume 16, 1 ianvaria–31 dekabria 1933 g. [January 1–December 31, 1933]; volume 17, 1 ianvaria–31 dekabria 1934 g. [January 1–December 31, 1934]. (Ministerstvo Inostrannykh Del SSSR.) Moscow: Izdatel'stvo Politicheskoi Literatury. 1970; 1971. Pp. 919; 878.

Since publication of the series began in 1957, the *Documents on the Foreign Policy of the USSR (DVP)* has established itself as an essential source for the study of Soviet foreign relations and the history of diplomacy between the world wars. Volumes 16 and 17 continue the valuable contribution made by their predecessors.

Together the volumes present 891 documents (plus statistics on Soviet foreign trade in 1933) in a convenient chronological format. The value of these materials, more than four-fifths of which were previously unpublished, is amplified by extensive notes and by the inclusion of 116 related documents of foreign origin. Lists of the Soviet and foreign documents and indexes by topic and country also facilitate the reader's use of the materials.

Volumes 16 and 17 follow the pattern set for the *DVP* of focusing on the conduct of Soviet diplomacy by the Commissariat of Foreign Af-

fairs (Narkomindel). Fifty-two per cent of the Soviet documents are reports to the Narkomindel from representatives abroad or memoranda of meetings with foreign diplomats. One-fifth are dispatches from the Narkomindel to its representatives. Communications to other governments constitute twelve per cent of the total; public policy statements, eight per cent; and international agreements, seven per cent. Half of the remaining six documents are reports by the Narkomindel to the Central Committee of the Communist party or to the Council of People's Commissars (Sovnarkom). There are, however, no policy directives by the Sovnarkom or the Communist party and no suggestions of dissent within these bodies about foreign policy. The problems of Soviet historiography are further reflected by the omission of Karl Radek's many important articles. There are also no materials bearing on the Communist International.

Despite these limitations, the two latest volumes of the *DVP* add measurably to our knowledge of Soviet foreign affairs during 1933 and 1934. The development of formal relations with the United States is the subject of 75 documents. Of particular note are several, but apparently not all of M. M. Litvinov's reports about his negotiations with Franklin Roosevelt in November 1933. The *DVP* also provides the fullest Soviet publication to date of the Roosevelt-Litvinov agreements, although Litvinov's explanation of Soviet economic espionage laws is not included. The tangled and dangerous Far Eastern situation is well covered with more than 130 documents, which permit a more detailed understanding of the Soviet response to Japanese aggression. Historians of German-Soviet relations will benefit from extensive archival materials, including two economic agreements and reports of numerous meetings with German political leaders. Nearly one-fifth of the volumes are assigned to relations with France and Britain, but there are some strange omissions: for example, the temporary commercial treaties of 1934 with these two states. Still more curious, 105 documents concern relations with the four Scandinavian countries, nearly as many as pertain to the League of Nations, the Disarmament and World Economic Conferences, and Poland, Latvia, Lithuania, and Estonia combined.

Its shortcomings notwithstanding, the *DVP* is a major source for historians of Soviet foreign policy. To ignore it would be only less rash than to neglect other sources.

ROBERT HIMMER

West Virginia State College

#### NEAR EAST

S. M. STERN *et al.*, editors. *Islamic Philosophy and the Classical Tradition: Essays Presented by His Friends and Pupils to Richard Walzer on His Seventieth Birthday*. Columbia: University of South Carolina Press. 1973. Pp. viii, 549. \$25.00.

It has been a long time in coming, and it comes, alas, expensively, but this *Festschrift* is a rich and rewarding collection. Richard Walzer, a classicist trained by U. von Wilamowitz and Werner Jaeger and an Arabist from the school of G. Bergsträsser, has been the most knowing student of the Greco-Arabic translation literature in philosophy over the last thirty years; here he is celebrated by thirty-three colleagues and students, some of them classicists, some Arabists, and some philosophers. The articles on Greek philosophy by Ackrill, van den Bergh, Charles Kahn, the Kneales, Theiler, Wehrli *et al.* will probably receive notice elsewhere, and so I shall here remark only on the Arabists' contributions.

First there is an important article by Franz Rosenthal (pp. 337-49) wherein he has collected, translated, and briefly studied all the references in the Arabic translation literature to the enigmatic Aristotelian commentator called in Arabic *'llynws*. Rosenthal has had no more success in identifying him than others (Aelianus? Albinus? Elias? Apollonius?), but now all the evidence is exposed in a convenient and useful form. In a somewhat parallel effort, A. A. Ghorab (pp. 77-88) has extracted and translated the citations of Aristotelian commentaries, some of them lost in Greek, by Alexander of Aphrodisias, Themistius, and Porphyry to be found in al-'Amirī's (d. 992) important ethical work *al-Sa'ādah wa al-Is'ād*.

J. N. Mattock gives us (pp. 235-59) the first full translation, from Kraus's edition, of the Arabic epitome of Galen's *On Traits of Character* (*Peri ēthōn*), a work that Walzer himself has studied in the past. Georges Vajda has like-

wise translated (pp. 473-89) the chapter on the vision of God from the *Kitāb al-Muhtawī* of the Karaite theologian Yūsuf al-Baṣīr and shows its dependence on parallel passages in the contemporary Mu'tazilite theologian 'Abd al-Jabbār (d. 1025), a dependence that extends far beyond this particular chapter.

Two articles are devoted, appropriately, to Farabi. Martin Plessner has edited (pp. 307-14) the newly discovered preface to Farabi's *risālah* on medicine from Istanbul (MS Emanet Hazinesi 1730), together with the section of Maimonides' Arabic commentary on the first aphorism of Hippocrates (MS Hunt 427), where the Jewish physician cites Farabi textually. F. W. Zimmermann's essay is a detailed study (pp. 517-46) of Farabi's enumeration of opposed propositions in his commentary on the *De Interpretatione* (Kutsch-Marrow, pp. 64-65), where Farabi's "amazing collection of oddities and obscurities" is viewed against the ambiguities of the Syro-Arabic translations of that passage in Aristotle.

George Hourani and Alfred Ivry here offer summary prolegomena to their larger studies of the ethics of 'Abd al-Jabbār and the metaphysics of al-Kindī. Hourani (pp. 105-16) is sound and careful as always, and Ivry's contribution (pp. 117-40) is the best short study I know of Kindi the philosopher. H. V. B. Brown has attempted (pp. 35-48) further to disentangle Ibn Sīnā's quarrel with the *Maghribīyūn*—here, with Pines, the Christian Peripatetics of Baghdad—by studying their respective views on the function of nature as a creative agent, consciously as in Alexander and the Baghdadis, or unconsciously with Rāzī and Ibn Sīnā.

Susanne Diwald writes (pp. 49-62) on the complex doctrine of the internal senses elaborated by the Brethren of Purity, with some interesting remarks on their connection with Manichean theories on the subject. There is a substantial and illuminating treatment of the Imamite Shi'ite attitude toward the 'Uthmanic recension of the *Qur'an* by A. Kohlberg (pp. 209-24), who finds in the Shi'ite sources a far more nuanced—and ambiguous—position than the Sunni polemicists would have us believe.

M. Schwarz takes up (pp. 437-66) the notion of "acquisition" (*kasb*), which played such a central role in the debates on ethical determination and free will in Islam, and traces its ev-

olution in literary and theological texts from the pre-Islamic "performance of an action when a judgment of value is involved," thence to the action itself, and finally to classical *kalām's* understanding of *kasb* as the relation between the agent and the action. The late S. M. Stern has assembled and instructively commented upon (pp. 437-66) the fragments of a chronological work by the ninth-century historian Abū'Isā ibn al-Munajjim. Our loss is underscored once again by this article and by the promise of what was to come (compare pp. 220, 447) from this gifted scholar.

There is more besides, even in Islamics: Louis Gardet's intelligent meditation on Mu'tazilism (pp. 63-76); Albert Hourani's study of the modern founder of the Naqshabandi order, Shaykh Khālīd (d. 1827) (pp. 89-104); the analysis by Angelika Kleinknecht, who also prepared the Walzer bibliography (pp. 5-16), of the curious exercise in Qur'anic logic known as *al-Qistās al-Mustaqīm* (pp. 159-88); and M. C. Lyons's brief, and not entirely happy, excursion into comparative psychology (pp. 225-34).

Taken all in all, which likely only the recipient and reviewer does, this is an impressive collection and one that enhances, by the quality of its contributions and the care in its making, the scholar whom it seeks to honor. *Ad multos annos!*

F. E. PETERS  
New York University

D. M. DUNLOP. *Arab Civilization to A.D. 1500*. (Arab Background Series.) [London:] Longman; [Beirut:] Librairie du Liban; distrib. by Praeger Publishers, New York. 1971. Pp. x, 368. \$15.00.

Due to its multilateral nature, *Arab Civilization* may be viewed from many sides and by various scholars. Anthropologists, sociologists, philosophers, artists, political scientists, as well as historians, have the right to undertake research dealing with this topic. Needless to say, each scholar would look at it from the point of view of his own specialization. D. M. Dunlop is a well-known historian of the Middle East and possesses a good knowledge of the original classical Arabic sources—a commendable achievement for a non-Arab scholar specializing in Islamic history. In this book, therefore, he discusses certain aspects of Arab civilization

as a historian, albeit a historian with a good taste for other cultural features such as literature, philosophy, geography, science, medicine, and some famous women in Islām. As a matter of fact, these are the main topics that Professor Dunlop discusses in this book besides the two main topics dealing directly with Islamic history, namely, "The Arabs and the Arab World to 1500" (chapter 1) and "History and Historians" (chapter 3). As expected, these two chapters (1 and 3), in which the author as a historian deals directly with his discipline, represent the best and most informative portions of the book. This does not mean that the other chapters are not informative. On the contrary, as the author himself mentions in the foreword, throughout the whole book there is always "something fresh to say" (p. ix).

This book is not written for popular reading; nonetheless it is stimulating and challenging to scholars and advanced students acquainted with the problems of research in Islamic history and civilization. A glance at the references cited in the footnotes is enough to convince the reader that an all-out effort has been made in this book to examine the literary sources of classical Islam as thoroughly and as exhaustively as possible. As is often the case in such endeavors, however, all features of Arab civilization cannot be included in a single volume or by any one scholar. Accordingly, it would be unfair to criticize this work for what has been left out or minimized. Suffice it to say that what the author set out to do was done well and deserves the respect and admiration of his colleagues.

Throughout this book the author's originality and "fresh ideas" are readily discernible. Even if some readers might not agree with some generalizations presented here, the fact still remains that in our field of research there are no laboratory tests to provide any sort of measurement for intellectual judgments. This situation is more accentuated in matters dealing with classical Islam simply because there are no primary sources at hand. Aware of this problem, Professor Dunlop closes his book with this statement: "An overriding consideration, however, which makes many generalizations about the Arab empire mere personal opinions, is the absence of archives which, except for such documents as happen to be included in

the works of historians or others, are almost totally lacking" (p. 267).

WILSON B. BISHAI  
Harvard University

DEREK HOPWOOD, editor. *The Arabian Peninsula: Society and Politics*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 320. \$11.50.

In 1968 Britain announced that after a century and a half she was about to lay down her political responsibilities in the Persian Gulf. Middle Eastern specialists reacted to this news in characteristic fashion, and scholarly conferences featuring papers by an array of genuine (and a few not-so-genuine) specialists on Arabia and the Gulf became something of a fad. This volume is the record of one of these conferences—in this case one held early in 1969 under the joint sponsorship of the School of Oriental and African Studies of the University of London and the Middle East Centre of St. Antony's College, Oxford.

In the technical sense, the book benefits from skillful editing by Derek Hopwood, Oxford's Middle Eastern bibliographer. The work consists of fourteen articles organized around five themes: bibliography, history, political development and international relations, sociology and culture, and economics; it also contains three informative—if slightly dated—statistical tables and a useful index. Although there are some unfortunate efforts included among the individual papers, the majority are, at the very least, workmanlike summaries, and a few stand out as original, important contributions. J. C. Wilkinson, an Oxford historical geographer, has written an imaginative yet solid discussion, "The Origins of the Omani State," based upon extensive knowledge of little-known Arabic materials. R. D. Bathurst's well-documented study produces a closely knit explanation of Oman's emergence as an Indian Ocean maritime power and the effects of this development upon the country's internal political history up to the early eighteenth century. The Oxford sociologist, P. A. Lienhardt, in "Some Social Aspects of the Trucial States," has written an interesting piece, which focuses upon the status and role of women in contemporary eastern Arabian society. Also, Frank Stoakes, a Manchester political scientist, has

provided a thought-provoking analysis of current social and political change in eastern Arabia.

Unfortunately, the lively bibliographical article by Hopwood that opens the volume is too narrow in scope to provide an essay broad enough in concept to undergird this book, which aspires to furnish scholars with a sophisticated introduction to the Arabian scene. The book's foreword expresses the hope that it "will not only provide students of the Middle East with an introduction to the social and economic and political problems of Arabia and the Persian Gulf, but also generate wider interest in further research and study of the area." In my judgment, the first-mentioned hope is not realized because the work is a qualitatively uneven mosaic displaying large gaps in coverage. Moreover, much of the information, analysis, and speculation it presents became dated during the long interval between the original appearance of the papers and their ultimate publication. The second, more modest and realistic purpose may well be achieved because of the excellence of some of the individual contributions.

ROBERT G. LANDEN  
University of South Carolina

M. A. COOK. *Population Pressure in Rural Anatolia, 1450-1600*. (London Oriental Series, volume 27.) New York: Oxford University Press. 1972. Pp. 118, 3 maps. \$20.00.

It has become traditional for Ottomanists to deplore both the state of the art, which is singularly underdeveloped, and the state of the archives, which impedes development. In this brief but brilliant essay, M. A. Cook offers an example of the kind of work that will ultimately end this circular dilemma and make it possible to write the social and economic history of the Ottoman Empire with less diffidence and more precision.

The stated purpose of this study is to test the thesis advanced by F. Braudel (*La Méditerranée et le Monde méditerranéen à l'époque de Philippe II* [Paris, 1949; rev. ed. 1966]) that emergent overpopulation resulted in a decrease in grain exports from the eastern Mediterranean in the late sixteenth century. In fact, the question of Mediterranean trade is not really



considered. As the author indicates, there are no reliable measurements of Ottoman grain production and exports. Moreover we note that increased land values, land expropriation, and the use of marginal lands could indicate growing profitability of agricultural production rather than overpopulation.

It is the second aspect of Braudel's thesis, the demographic problem, that is of primary concern. Through a meticulous examination of *Mühimme* surveys for three districts in Anatolia, the author attempts to utilize fiscal materials to establish indices of population growth and population pressure. He then presents a provocative essay on the possibilities of a correlation between rural disorders in Anatolia, specifically the Celali uprisings, and emergent overpopulation. While we question the elimination of certain categories from the survey, especially *vakf* and *mülk* lands, the limitations of the materials and the methodology are clearly indicated; the boundaries of the problem are rigorously defined, and every effort is made to optimize the validity of the data. The results of the study are statistically inconclusive. The limited materials available, the ambiguity of the data, and the inadequacy of the resource base make the evaluation and analysis of social and economic developments problematic. This is not to criticize but to agree with Mr. Cook. We cannot proceed to articulate change in the Ottoman Empire without further research of the quality reflected in this volume.

DEENA R. SADAT  
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Camden

WILLIAM L. CLEVELAND. *The Making of an Arab Nationalist: Ottomanism and Arabism in the Life and Thought of Sati' al-Husri*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1971. Pp. xvi, 211. \$8.50.

Mustafa Sati' al-Husri (1880-1968) is certainly a worthy subject for an intellectual biography. An Arab of an Aleppo family, his early career was spent as a loyal member of the Ottoman elite; teacher, provincial administrator, educational reformer, and publicist promoting the multinational Ottoman bond. Then, when World War I shattered the Empire, he left Istanbul and gave his loyalty to the Arabs, serv-

ing as educational planner but, most important, becoming one of the earliest and most influential spokesmen of Arab nationalism. Al-Husri's life and ideas, therefore, are deserving of study on several levels: as a spokesman of Ottomanism, as an ideologue of Arabism, and as a case study of the readjustment of identity that many Arabs of his generation had to undergo with the dissolution of the Ottoman Empire.

Professor Cleveland's work gives most of its attention to the second phase of al-Husri's activities, that of advocate of Arabism. Here the book is excellent. Al-Husri's predominantly linguistic concept of nationalism, his use of historical arguments to prove both his theory and its applicability to the Arabs, his attacks on narrower concepts of nationalism, the minimal place given to Islam in his writings, and finally the instrumental aspects of his teachings with their emphasis on selective education in the service of nationalism and on the necessity of inculcating the younger generation with the virtues of solidarity and sacrifice—all this is summarized lucidly from a comprehensive examination of al-Husri's writings and then analyzed critically and judiciously. While the intellectual weaknesses and the practical, "manipulative" goals of al-Husri's doctrines are noted, there is also a scrupulousness of analysis that avoids squeezing the man into intellectual categories where he does not fit. Al-Husri was a complex man, if not a terribly sophisticated thinker; Professor Cleveland's treatment does justice to that complexity.

Al-Husri's Ottoman phase and his shift from Ottomanism to Arabism receive less attention. While the few pages summarizing his pre-World War I thought are valuable, one suspects that much more could be said about his early career. The ideas offered on al-Husri's reorientation in loyalty after the war are fascinating, however. For this shift Professor Cleveland seems to suggest largely an explanation of expediency; that al-Husri, who knew, but rejected, linguistic theories of nationalism in his Ottoman phase (for example, he criticized Ziya Gökalp's ideas on Turkism in 1911), came to see the linguistic bond as the only possible basis of Arab identity after the shattering of the Ottoman Empire. This analysis (fairly, if not totally, convincing) raises important ques-

tions about the depth and meaningfulness of modern linguistic-historical Arab nationalism. Was (is) belief in Arabism subject to change, as al-Husri's belief in Ottomanism itself gave way to Arabism after World War I? Was (is) the espousal of an eternal Arab nation but a rationalization for the more practical position that Arab unity is the only way to Arab survival in the modern world, as seems to have been the case with al-Husri? This work does not address these questions directly, but its probing of the thought of Sati' al-Husri provides fertile material for the entire subject of the commitment of nationalists to their nation.

JAMES P. JANKOWSKI  
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Boulder

JACOB M. LANDAU. *The Hejaz Railway and the Muslim Pilgrimage: A Case of Ottoman Political Propaganda*. Detroit: Wayne State University Press. 1971. Pp. 294. \$15.95.

Jacob Landau of Hebrew University is to be commended for providing us with a transcription and an English translation of *al-Sa'āda al-nāmiyya al-abadiyya fī'l-sikka al-ḥadīdiyya al-ḥijāziyya* (*The Increasing and Eternal Happiness—the Hejaz Railway*) by Muḥammad 'Arif Ibn al-Sayyid al-Munir al-Ḥusayni'l-Dimashqī (Library of Istanbul University, Arabic MS 4780). An introduction, a map, a glossary, facsimiles of the frontispiece and two pages of the manuscript, five pages of photographs, and an index increase the volume's usefulness.

As the title he chose for this volume indicates, Professor Landau is principally interested in 'Arif's manuscript as an example of Muslim-oriented Hamidian political propaganda aimed at the Arabs. His introduction deals with the late nineteenth- and early twentieth-century phenomena of Ottoman railroad construction, particularly the Hejaz Railway, and of Ottoman propaganda among the Arabs. It also contains information concerning the manuscript and its author in which Professor Landau calls attention to the valuable description of the pilgrimage provided by 'Arif who "wrote with the knowledge of an insider" (p. 27).

An examination of the text suggests that it

was this detailed account of the pilgrimage that 'Arif wrote first. In it he describes the caravan route and its stations, the Bedouins along that route, the ceremonies in Istanbul and Damascus attending the departure and return of the pilgrimage caravan, the caravan's mail service, the religious rituals in Mecca and Medina, and the rationale for the pilgrimage.

In 1900, while in Istanbul, 'Arif prepared and presumably presented to Sultan Abdül Hamid II a work intended to refute the arguments raised against the construction of the Hejaz Railway. After a preface in which he frankly announces this intention, 'Arif has inserted his account of the pilgrimage (sections 1–10) with an occasional reference to the proposed railroad seemingly added, usually at the beginning or the end of a section. This is followed by several sections (11–17) devoted entirely to extolling the potential benefits and advantages of the Hejaz Railway to Muslims in general and to the Syrian and Hejazi Bedouins in particular. Among these are "populating the country, restoring life to the servants [of Allah], serving the Two Shrines [Mecca and Medina], assisting those desirous to visit both of them, promoting . . . commerce, bolstering . . . agriculture, and maintaining the political balance of the . . . Arab lands" (p. 42).

This volume thus supplements our factual knowledge of the Syrian-Hejazi pilgrimage at the turn of this century, as well as giving us a pro-Ottoman Arab's views on the Hejaz Railway.

RICHARD L. CHAMBERS  
University of Chicago

HASSANEIN RABIE. *The Financial System of Egypt, A. H. 564–741/A.D. 1169–1341*. (London Oriental Series, volume 25.) New York: Oxford University Press. 1972. Pp. xii, 242. \$21.00.

This extensively documented study is a detailed reconstruction of the sources of revenue (chapter 3), financial administration (chapter 4), and the *iqṭā'* (an administrative grant of revenue, usually based upon land) system (chapter 2), as well as a survey of the monetary changes (chapter 5) during the Ayyubid dynasty and the first century of Mamluk rule in Egypt. The

breadth of the medieval Arabic material and the Geniza documents that Professor Rabie has culled for their isolated and often obscure data is reflected in his extensive bibliography, the numerous footnotes, which are conveniently placed with the text, and the introductory chapter, which surveys the value of the various types of sources. Only an examination of the traditional chronicles is missing.

The caution with which Rabie uses his source material is immediately apparent in the body of the work. In each chapter the author brings together all the available information within appropriate subsections, for example, the size and value of the *iqṭāʿ*, taxes on vices, confiscations, tax farming, and so forth. Not only is there new data, but their cumulative effect is to give us the most detailed account of any financial system for medieval Islamic history.

There are two main weaknesses in the work. The author has been tempted to allow his data to speak for itself. On topics such as the importance of the cadastral survey of 1315, the changing role of the wazirate, or the reasons for the decrease in the supply of Egyptian gold currency under Saladin, Rabie's statements have applications far beyond the specific topic being discussed. But, too often, the reader must draw his own conclusions, whether it be about the impact of a particular tax or monetary change or about the general structure and dynamics of Ayyubid-Mamluk financial systems as a whole.

Rabie's caution in avoiding possibly imprecise Western terminology such as fief and feudalism is commendable, but I believe he went too far. For example, Rabie writes that "the *ajṇād al-ḥalqa* resorted to *nuzūl* to have their *iqṭāʿ*'s changed to pay" (p. 56). The first Arabic term is defined on page 40, note 2, which, unfortunately, is not listed in the index under *ajṇād al-ḥalqa*; the second term can only be translated by context; and the third should be left as *iqṭāʿ*. This illustrates the second and more serious problem, the lack of a glossary or translation of all the Arabic terms employed in the index and text. Ultimately it will have to be the specialist or a most patient and careful reader who will be able to extract the extensive and fundamentally important information that

Dr. Rabie has so carefully gathered and systematically organized.

JERE L. BACHARACH

University of Washington

JANET L. ABU-LUGHOD. *Cairo: 1001 Years of The City Victorious*. (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1971. Pp. xiv, 284. \$25.00.

It is generally recognized today that Cairo is one of the world's great cities, the largest by far on the African continent or in the Middle East. What is seldom appreciated is that in the fourteenth and fifteenth centuries, before shifting trade routes ended its role of entrepôt in East-West trade, its population was at least double that of any city in Europe. Mrs. Abu-Lughod's study details the later vicissitudes of the medieval city, through its long decline under Ottoman and Mamluk rule, and its more recent status as a kind of *casbah* or *medina* contrasting with the new Europeanized quarters constructed along its western edge on the former floodplain of the Nile. Since the author's main concern is sociological, she does not attempt to rehearse Cairo's long record of distinguished contributions to Islamic theology, law, and science; the thousand-year-old mosque-university of al-Azhar is scarcely mentioned. Within the restrictions she has set for herself, however, Mrs. Abu-Lughod does a skillful job of translating the raw data of her sources, particularly the census returns of 1947 and 1960, into informative and readable prose. The prospective reader should not be put off by the battery of tables and charts nor by the note on methodology, tactfully postponed to an appendix. There are helpful maps and more than a hundred instructive illustrations, many of them from photographs by the author. The results are a handsomely produced volume and a series of well-delineated portraits of the immensely varied districts constituting this fascinating city. Everywhere Mrs. Abu-Lughod seeks to convey the nature of the inhabitants, from the well to do in the high-rise apartments of Garden City and Zamalek along the Nile to their neighbors, the slum dwellers of overcrowded Bulaq (where the population density exceeds 100,000 per square kilometer!),

and from the middle-class and bureaucratic families of the planned communities of Helio-  
polis and Nasr City to the tens of thousands of  
Egyptians, many of them evacuees from the  
Suez Canal zone, who have made makeshift  
homes among the tombs and shrines of the gro-  
tesque Cities of the Dead.

JOHN B. CHRISTOPHER  
*University of Rochester*

JACQUES BERQUE. *Egypt: Imperialism & Revolution*. Translated by JEAN STEWART. New York:  
Praeger Publishers. 1972. Pp. 736. \$38.50.

"You strive towards a sublime goal, but you  
never reach it. And who then will reach it? An  
agonizing question, faced with which Fate  
veils her face. . . ." (*Faust*, part 2, quoted in  
the author's preface, p. 26). To these lines,  
Jacques Berque might well have added the  
Angel Chorus at the end of *Faust*, part 2:  
"Gerettet ist das edle Glied/Der Geisterwelt  
vom Bösen,/Wer immer strebend sich bemüht/  
Den können wir erlösen." (Saved is this noble  
soul from ill,/Our spirit-peer. Who ever/Strives  
forward with unswerving will,—/Him can we  
aye deliver. . . .)

These latter lines from Goethe sum up the  
"message" of this magnificent book, full of  
hope despite the sordidness of the past and  
prophetic of what could happen if the lessons  
of the past continue to be ignored. "Can it be,"  
writes Berque (pp. 29–30), "that the new being  
now struggling to emerge from the ordeal  
amidst violence and misunderstanding is really  
recreating the synthesis of which Western man  
had lost all hope? In any case, that is what he  
must needs do. The alternative is his down-  
fall." The French edition of this book was  
published in 1967, that most fateful year for  
Egypt and the entire Arab world. The revolu-  
tionary generation who created Nasser's Egypt  
did not profit from these lessons; nevertheless  
the "message" is still a valid one and the hope  
still remains.

The central question posed by Berque is:  
"Under what circumstances, according to what  
correlations, does liberation result dialectically  
from dependence?" (p. 25). He has focused on  
Egypt during the period between the British  
occupation in 1882 and Black Saturday of  
1952, on the eve of the July revolution. He

effectively demonstrates that in this dialectical  
process, colonization and decolonization "are  
not so much successive phases in time as rival  
phases. . . ." Failure to see colonization and de-  
colonization as rival phases has led other histo-  
rians to deal consecutively with events that  
were in fact occurring simultaneously, tending  
thereby to disguise the inner reality of Egypt-  
ian history and to attribute meaning to events  
that have no meaning without reference to this  
inner reality. Berque does not make the same  
mistake. For this reason, his book provides the  
most comprehensive picture of this period of  
Egyptian history yet to be published. Further-  
more, it points to the direction that must be  
taken in all future works on modern Egypt and  
other areas of colonization and decolonization.  
It is not an easy assignment, certainly not for  
those who lack that special perception Berque  
has shown in all of his books dealing with the  
modern Arab world—a perception that can  
only come from long acquaintance with and an  
appreciation of the Arabs.

Lacking the range of monographic materials  
available to social historians of Western socie-  
ties, Berque relies heavily on his training as an  
Orientalist and sociologist to extract from  
many different sources (literature, newspapers  
and journals, archival materials, personal ob-  
servation, and innumerable conversations with  
Egyptians) the evidence required for the crea-  
tion of a social history of Egypt during a pe-  
riod of total disruption. In this period every-  
thing deemed permanent in Egyptian society  
fell under attack; no adequate substitutes re-  
placed what was destroyed. Western concep-  
tions introduced in the process of colonization  
were indiscriminately accepted by the Egyptian  
ruling elite and by Egyptian intellectuals, an  
acceptance often skin-deep and consequently  
uncreative.

With a broad brush and in lavish colors Pro-  
fessor Berque paints a picture enormously ex-  
citing in its variation. He begins with a de-  
scription of Egyptian society just before the  
age of colonization, a society already undergo-  
ing a process of change but nonetheless retain-  
ing its traditional contours. The period of col-  
onization interrupted the organic evolution of  
Egyptian society by the introduction of new  
factors. Whatever good British rule brought to  
Egypt must be balanced against the disruptions

it caused. Deluded by their accomplishments, the British never properly assessed their shortcomings. In the end, they created conditions that produced a mood of violence among the Egyptian peasantry, that led to the growth of a nationalist movement among the intellectuals, and that made the revolution of 1919 by all strata of Egyptian society an inevitability.

The period between the wars turned out to be a period of lost opportunities, not so much because of the three-cornered political struggle conducted by the palace, the British, and the Wafd, but because of the failure of all to come to grips with the essential problems already crowding in. One could recognize already the economic decadence, the population explosion, the inequities in the distribution of wealth, the growing alienation of the young, the emergence of political extremism, and the decline in the influence of the religious authorities, especially of the more moderate elements. For a brief moment, a reversal of these trends appeared possible with the ascent to the throne of the young king, with the Anglo-Egyptian Treaty of 1936, with the end to the capitulatory regime made possible in 1937 by the Montreux conference, and by the entry of Egypt into the League of Nations.

World War II shattered all illusions. It was not solely the continuation of the British occupation that made everything go wrong; by this time, the process of decolonization was nearly irreversible. Rather, it was the inadequacies, opportunism, and greed of the ruling clique. Their failures were monumental and the consequences of these failures predictable. Professor Berque's judgments are their harshest at this point. He spares no one.

The immediate postwar era produced no improvement. Rather, things went from bad to worse. Egyptian reversals over the Sudan and in Palestine, a cholera epidemic, the degeneration of the king and of the politicians, the exigencies of cold war politics—all combined to produce the explosions of early 1952 with which the author concludes his book. The process of colonization and decolonization had moved into a new phase. An opportunity for a new beginning would soon be available to those elements produced by a vastly changed Egyptian society. Professor Berque does not carry his analysis into the revolutionary era,

nor does he state directly in the body of his book his judgments as to its accomplishments. It is clear, however, that he does not consider the struggle against the colonizers the central problem of the future. More important would be the redirection of the decolonizing efforts along lines more deeply rooted in the Egyptian soil. "Is it sufficient to adopt, after having endured, the hypotheses of Western civilization as the sole form of progress? Rather, should not every civilization go forward in its own way to discover the technological stages of the future?" (p. 26). "He [the Egyptian] must reconcile his specific personality and his universal humanity, both within himself and in the eyes of the Other. While deeply rooted in the soil, he must aspire towards a supremely rational future. He must ensure that this crisis of his personality and that of society as a whole correspond to one another in purpose and in significance, and join in commitment to a single end. He knows that he must belong to the world, or cease to be" (p. 30).

Although this book is brilliantly conceived and brilliantly executed, it suffers from a few defects that might easily have been avoided. The form of indexing the work proves an obstacle to easy reference rather than an aid, and the absence of a full bibliography at the end of the book is deplorable. Numerous references are made to sources within the body of the work without any indication of where they may be examined. It is possible that Professor Berque was not always in a position to disclose their location, but even that fact would have proved useful to know. But to carp on the lack of adequate scholarly paraphernalia or on the errors that inevitably occur in so precocious an attempt of synthesis seems out of place in a review of so eminent a book by so distinguished an author.

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JACQUES COULAND. *Le mouvement syndical au Liban (1919-1946): Son évolution pendant le mandat français de l'occupation à l'évacuation et au Code du travail*. Preface by JACQUES BERQUE. Paris: Éditions Sociales. 1970. Pp. 453.

This is an attempt to chart the evolution of labor unionism in Lebanon during the French

Mandate, from 1919 to 1946, the year of the French evacuation and the passage in Parliament of the *Code du Travail*. The book discusses the manner in which the various communities in this nation of confessional and ethnic mosaic have participated in or reacted to the dynamics of the workers' struggle and demands; to what extent and how Lebanese labor unionism was inspired by external models, be they European or Egyptian; how much progress toward the transformation of Lebanese and Near Eastern societies resulted from the sophisticated debate that labor unionism sparked; and in what way such debate was more conducive to change than the usual type of intercommunity bickerings.

Couland's work is a serious research endeavor, not only into public and private archives and the news media but also utilizing the wealth of oral testimony of historical witnesses. *Le Mouvement Syndical au Liban* suggests a constant interplay between two types of institutions—the unions, a kind of association established by workers determined to protect their professional interests, and the Mandate, a type of colonial domination with international blessings. The former is concerned with antagonistic social groupings within a global society, the other between two global societies—one dominated, the other dominating. These two institutions, existing on different levels and with different aims, interfered on a common ground, that of a definite socioeconomic formation—industrial capitalism.

Jacques Couland has discussed the evolution of unionism in Lebanon historically, using the following stages: (1) When the French occupied Lebanon in 1919, unions were nonexistent, and the working class was still embryonic and scattered. Lebanon was not yet a political entity. Autonomy, on a confessional basis, for Mount Lebanon nestled as it is in an Arab-Islamic region was antieconomic. Yet in 1921 the *Parti du Travail* was constituted. (2) 1926 ushered in a period where transitional domination was replaced by a permanent one. Economic difficulties and the Syrian revolution created a favorable climate for spontaneous labor strikes; the "drivers" were reorganized, and their strikes were impressive because of the important role these chauffeurs played in the regional economy. Notables and nationalist leaders sympa-

thized in an attempt to enlist their leaders and thus enlarge their clientele. Similarly, the typographers' association transformed itself into a union directed by workers but seeking alliances with notables and political leaders. (3) The world economic crisis worsened the situation by 1936 and the pace of industrialization slowed down. On the eve of World War II a federation of labor was at hand—the *Comité d'Union Syndicale* with a quasi-legal status. (4) Progress in unionism was interrupted during the war. The first phase (1939-41) of the war manifested itself as a police repression against both the nationalist and labor movements. In its second phase (at least until 1943) it changed direction. Free France abolished the Mandate—courting labor in Lebanon, just as in France, as a means of combating Germany. The unions were reconstituted around the *Comité d'Union des Syndicats*.

The history of labor unions in Lebanon thus appears from this impressive study to follow the vicissitudes of the political and economic history of that nation, itself conditioned by the dialectic of the relationship between colonizer and colonized. *Le Mouvement Syndical au Liban, 1919-1946* is indispensable to all students of the Near East and of Lebanon. I recommend it explicitly and hope that it will soon be translated into English.

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#### AFRICA

ATTILIO GAUDIO. *Allal El Fassi: Ou l'histoire de l'Istiqlal*. Preface by JACQUES BERQUE. Paris: Éditions Alain Moreau. 1972. Pp. 365. 35 fr.

The title is misleading, for this book is not a history of Morocco's Istiqlal party; it is nonetheless a valuable contribution to the presently insufficient body of knowledge available to the West concerning a historically and strategically significant part of the African continent. In it Attilio Gaudio, an Italian journalist who has been a friend and confidant of Allal El Fassi for twenty-five years, presents a comprehensive compendium of the contributions of El Fassi to Istiqlal party doctrine and policy. His account is presented substantially in El Fassi's own words, for Gaudio relies upon numerous

(and frequently extended) quotes from El Fassi's writings and speeches. The book, written in French, contains 240 pages of adequately documented text, an additional 125 pages of annexes, and 17 documents, many of them translated from Arabic. Unfortunately, there is no index.

Allal El Fassi, a pioneer in the Moroccan nationalist movement, has exerted an important influence on the policy of the Istiqlal party since its organization in 1944, and as president of the party since 1960, perhaps a dominant influence. His has been an idealistic vision of the participation of Moroccans in a democratic society guided by the principles of Islam. According to El Fassi, nationalism, as the rejection of European colonialism, is a stage through which Morocco must pass before joining a much larger Islamic brotherhood. El Fassi advocates the Arabization of Moroccan culture, specifically education; the rejection of Western capital investment and influence; the nationalization of the mining, banking, insurance, and other industries; and the redistribution of land to the actual cultivators—in short, the Arab socialism of the Middle East proper.

The weakness of the book lies in Gaudio's failure to account for El Fassi's interaction with other leaders of the Istiqlal party, like Ahmed Balafrej. El Fassi's attitude toward government was theoretical, not practical. Almost certainly his participation would have been welcomed in any of the cabinets formed in the period between the achievement of independence in 1956 and the proclamation of a state of exception by King Hassan II in 1964; except for an eighteen month period between June 1961 and January 1963 he preferred to remain "in opposition"—the sort of opposition to which the king referred following the army mutiny at Skhirat in July 1972 as a "spirit of criticism" rather than the "critical spirit" needed in the nation.

El Fassi's fixation on his concept of the ideal not only obviated the personal contribution he might have made toward the development of government in an independent Morocco, but fragmented and weakened the Istiqlal party as well. Traditionalists, Berbers for the most part, alienated by the apparent Istiqlal willingness in 1954 to accept a monarch other than the exiled Mohammed V, fashioned in the Liberation

Army an effective force for independence. This force was later politicized in the *Mouvement Populaire*, led by Majoub Aherdane and Mohammed El Khatib. Modernists, led by Mehdi Ben Barka, Abderrahman Bouabid, and Abdallah Ibrahim, split off into the *Union Nationale de Forces Populaires*. Ahmed Balafrej threw his lot with the monarch, although he retained his party membership. Others, frustrated by El Fassi's visionary and unproductive leadership, dropped out of political life. But, whether positive or negative in influence, Allal El Fassi remains an important figure in North African politics; he deserves to be better understood, and Gaudio's book contributes to that understanding.

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HENRI BRUNSCHWIG, with the assistance of GEORGETTE LAGARDE and JAN VANSINA. *Brazza explorateur: Les traités Makoko, 1880-1882*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Documents pour servir à l'histoire de l'Afrique équatoriale française. Second series, Brazza et la fondation du Congo français, number 2.) Paris: Mouton & Co. 1972. Pp. 298. 59 fr.

This is the second volume in a three-volume series of documents entitled "Brazza et la Fondation du Congo Français." The first volume, which appeared in 1966 under Brunschwig's direction, covered Brazza's first mission to Equatorial Africa between 1874 and 1879. The third volume, which was published in 1969 under the editorship of Cathérine Coquéry-Vidrovitch, dealt with Brazza's third mission from 1883 to 1885. The present volume contains materials on his second mission, 1880 to 1882. It is divided into three sections: the travel diaries of Brazza, the correspondence between him and the French Committee of the International African Association, and official documents. Two of the three notebooks in section 1 have already been published in the *Cahiers d'études africaines* (no. 1 [1965], 5-56; no. 2 [1966], 157-227). They cover, respectively, the negotiation of the treaty with the Makoko (January 22 to March 15, 1882) and the trip from Franceville on the Ogowe River into the Congo Basin (June 22 to August 16, 1880). These notebooks reveal that Brazza

consistently sought to extend French political control into the Congo Basin and that he negotiated his treaty with the Makoko during palavers lasting nearly two weeks. The two notebooks are reproduced here together with the notebook on Brazza's return via the Niari-Kwilu (August 28 to October 3, 1880). Jan Vansina's excellent notes on African peoples and geography contribute much to an understanding of the diaries, as do Georgette Lagarde's biographies of the French participants.

The past fifteen years have seen a re-examination of the origins and course of the late nineteenth-century scramble to partition tropical Africa. In his earlier studies Brunschwig has contributed a good deal to a review of the French role. Through the editing of these documents he makes it possible for larger numbers of students to examine some of the important French sources for these developments. In addition he allows us to study the role of Brazza himself, who did so much to extend the French empire into Equatorial Africa and to introduce a rule that was to prove so disastrous for millions of blacks even within his own rather brief lifetime.

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JOHN S. GALBRAITH. *Mackinnon and East Africa, 1878-1895: A Study in the 'New Imperialism.'* (Cambridge Commonwealth Series.) New York: Cambridge University Press. 1972. Pp. viii, 253. \$17.50.

In this scholarly and eminently readable work, John Galbraith attempts to shed new light on the process and politics of the late nineteenth-century British expansion in East Africa through an examination of the ill-fated career of the Imperial British East Africa Company and the activities of its founder, William Mackinnon. Galbraith is ultimately successful, and historians of Africa and the British Empire alike will benefit from the new material he has collected. He shows in painful detail the imperialist foot-dragging of the Foreign Office in the 1880s, wanting to preserve British interests in East Africa, but anxious to maintain a cordial relationship with Germany, and above all reluctant to commit any of the taxpayers' money. Galbraith shows convincingly that it was the fear of a German takeover in Bu-

ganda—thus threatening the British presence in Egypt and the Sudan—that finally sparked the decision to act.

Under these circumstances, the chartering of a private company seemed an ideal solution. Mackinnon and his friends could simultaneously develop the commercial potential of East Africa and carry out the strategic and political goals of the Foreign Office by establishing the British presence in Buganda and the upper Nile. In the end, of course, they could not. The company failed in both its political and commercial objectives, and its failure was probably inevitable. East Africa lacked the kind of resources that could have yielded significant profits in the short run, and the "precipitate rush into Buganda" fatally overextended the company's means. Galbraith argues, however, that in the long run strategic considerations outweighed commercial gain in Mackinnon's thinking as much as in Salisbury's. While the company directors were originally attracted by the promise of commercial profit, their motivations ultimately derived from a sense of patriotic imperialism: "The directors believed that . . . the greatest dividends would not be in personal profit but in securing to Britain territories which could be valuable economically and strategically" (pp. 238-39).

Galbraith's book, then, is a welcome addition to our knowledge about British imperialism in Africa, and it takes its rightful place alongside earlier studies of Rhodes and Goldie, of company rule in western and southern Africa, and of the politics of European expansion. Some will regret that Galbraith did not say more about the African part of the story. We learn nothing about the operations of the company on the ground, for example, or its interaction with Kikuyu and Ganda. African rulers such as Mwanga and Barghash lack the reality of historical actors in this account: they exist only in correspondence preserved in P.R.O. files. It is perhaps unfair to fault a scholar for not adventuring beyond the accepted confines of his discipline, yet in this sense the complete history of the Imperial British East Africa Company remains to be written. This narrowness of scope is the major disappointment in an otherwise promising book.

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## ASIA AND THE EAST

CARMEN BLACKER *et al.* *Tradition and Modernization in Japanese Culture*. Edited by DONALD H. SHIVELY. Princeton: Princeton University Press, for the Conference on Modern Japan of the Association for Asian Studies. 1971. Pp. xvii, 689. \$14.50.

The last decade and a half has been an enormously fruitful period for Anglo-American scholarship on modern Japan. Much of this contribution to our increased understanding of Japanese society, culture, and history can be attributed to the maturation of a postwar generation of scholars supplied with adequate research funds (funds that are unfortunately now drying up); but another factor of great significance has been the widespread acceptance, if often only implicitly, of a more sophisticated conceptual framework for treating modernization. This paradigm, refined in the literature of the postwar social sciences, has provided a set of shared perspectives and concepts of considerable usefulness. The series "Studies in the Modernization of Japan" has been important both in presenting some of the best of recent research and in pointing the direction for further efforts.

The present volume, the fifth and penultimate in the series, is in some ways the most intriguing, for it is an explicit attempt to extend this view of modernization to culture as understood by those who would defend the autonomy of the arts and the humanities. The attempt, however, is only partially successful. There are hints of the frictions generated by such an endeavor in Professor Shively's preface: "It would divert us from our primarily humanistic interests to reopen problems of definition and theory in the studies of modernization. But to attempt to relate cultural changes to modernization taking part in other sectors of a society is to venture into an area that has been investigated very little" (p. xv). John Rosenfield, in an interesting tour of Western-style painting in the first decades after the Meiji Restoration of 1867, feels constrained to warn us that art is more than merely a "social or economic phenomenon" (although he does offer some insights into how they are interrelated). Valdo H. Vigliemo, in his admirably written essay on Nishida Kitaro (twentieth-century Japan's fore-

most philosopher), raises the issue more directly by admonishing those who suffer from "uncritical faith in the social sciences, particularly social psychology," and insisting that we come "to grips with the true thrust of Nishida's thought, *qua* thought" (p. 508n.). William Malm's "The Modern Music of Meiji Japan," despite some tantalizing glimpses of music as a part of popular culture, basically refuses to confront the issue at all by concluding that "if modernization means 'conforming to present [*sic*] usage, style, or taste,' then only parts of the Meiji musical scene can be called truly modern" (p. 300). The point, of course, is not that Vigliemo's admonition about uncritical faith is gratuitous, but rather that the facile generalizations and fallacies of reductionism to which he and others (including myself) would object can be corrected only by either confronting them directly or pointing the way to an alternative paradigm.

On the other hand, there is much of interest to be learned from these discussions of painting, music, and philosophical thought; and, moreover, the volume does include a number of excellent articles that make better use of the conceptual tools implied in the title. Our level of sophistication is raised substantially by those addressed directly to the triadic interplay between traditional culture, Western influence, and modernization (as distinct from Westernization): for example, Eugene Soviak's careful, concise analysis of the impressions of Western civilization brought back by the 1871 Iwakura Mission; Michio Nagai's and Donald Shively's articles on "Japanization" as a reaction to government policies perceived as politically authoritarian as well as culturally threatening. Our understanding of the social pathology and crises of identity associated with modernity is further enhanced by the contributions of Donald Keene on the cultural effects of the Sino-Japanese War of the 1890s and Carmen Blacker on millenarian religious movements, and by the treatment of the lives and works of leading (if, as the editor tells us, not necessarily "representative") literary figures by Professors Brower, Hibbett, McClelland, Ortolani, and Seidensticker. Again, however, there are signs in the latter group of ambivalence in their approaches to the subject matter; one of the few solid points regarding modern fiction upon

which there is a common consensus among these literary scholars is the change in diction and grammar to incorporate popular speech. The final article by Roy Andrew Miller attacks the problem of linguistic modernization with precision as well as wit. His conclusion suggests that the Weberian concept of rationality and modernity is far too limited to encompass the complexities of change in the Japanese use of respect or honorific language.

In sum, the quality of the individual monologues is high, and the editor labors energetically in his preface and introductions to bring the group discussion back to the common themes. If at times one feels frustration at the absence of more sharply focused dialogue, it may be because too much was conceded in the ground rules at the outset.

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S. L. VAN DER WAL, editor. *Officiële bescheiden betreffende de Nederlands-Indonesische betrekkingen, 1945-1950* [Official Documents Concerning Dutch-Indonesian Relations, 1945-1950]. Volume 1, 10 aug.-8 nov. 1945; volume 2, 9 nov.-31 dec. 1945. (Rijks Geschiedkundige Publicatiën, Minor Series, numbers 36 and 37.) The Hague: Martinus Nijhoff. 1971; 1972. Pp. xxiv, 616; xix, 628.

The severely plain title of this series, "Official Documents Concerning Dutch-Indonesian Relations, 1945-1950," should not mislead the reader. Its subject is not some episode of foreign relations but the tragedy of the Dutch effort, amid the storm of the Indonesian revolution, to restore their lost Netherlands Indies. Nor are its documents confined to foreign relations, though the many-sided diplomacy of the struggle is thoroughly covered here. Professor van der Wal also ranges widely through private letters in Holland, newspaper stories, and especially the reports of Indies officials to survey the larger setting in which the political perceptions and policies of the leading Dutch authorities were formed and changed.

It is an ambitious enterprise. The two present volumes—each with six hundred pages in fairly small type, along with detailed notes and two indexes—cover only four and a half months of the five years. Even allowing for some quieter periods later it will take a good

dozen more such volumes to complete the story. The task is in the best of hands; in Professor van der Wal are combined not only the command of subject familiar from his earlier collections on prewar Indies politics and education, but also a formidable capacity for work, the establishment standing so important in a politically sensitive enterprise, and the historical imagination necessary for a grand design.

The 576 documents in these two volumes, plus a hundred more included in the footnotes, are selected from numerous Dutch and Indies departmental archives. The largest category is communications between Lieutenant Governor-General van Mook and Colonial Minister Logemann. But much space is also devoted to cabinet meetings in The Hague; correspondence with the Dutch ambassadors in London, Washington, and Australia; conferences and correspondence connected with Lord Mountbatten's Southeast Asia Command that was charged with reoccupying the Indies after the Japanese surrender; and Indies officials' field reports, recommendations, and memos on contacts with Indonesians. About five per cent of the documents are in English, the rest in Dutch. Language alone is likely to deter all but specialists, which is unfortunate because there is much here of importance for students of post-war British, United States, and Australian policy and of decolonization generally.

For students of recent Dutch and Indonesian history the series will be, quite simply, indispensable. The present volumes have a special appeal for, covering a period whose strong natural story-line has been enhanced by deft editing, they are absorbing and at times exciting reading. By the end of these decisive months, after much noisy clashing of gears, the Dutch apparatus is in working order and has settled on a basic policy of trying to push moderate Indonesians into autonomy within the Dutch Kingdom. The reasoned doubts of many high Indies officials (volume 2), including van Mook, have been set aside, as much by van Mook himself as by The Hague; the poignant appeal of an Indonesian Indies official (Raden Singer, volume 1, pp. 521-23) has been ignored; the tragedy can now unfold over the years.

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## UNITED STATES

SAM BASS WARNER, JR. *The Urban Wilderness: A History of the American City*. New York: Harper and Row. 1972. Pp. xvii, 303. \$12.50.

ARTHUR MANN *et al.* *History and the Role of the City in American Life*. (Indiana Historical Society Lectures, 1971-1972.) Indianapolis: the Society. 1972. Pp. 65. \$1.50.

These two volumes, similar only in their focus on the urban experience, testify to the vigor and variety of American urban history. Warner's book skillfully examines the entire range of city development and its human and social consequences in the United States. The second volume, a series of three lectures sponsored by the Indiana Historical Society in 1972, probes selected aspects of urbanism and urban history.

Despite (or perhaps because of) a somewhat unorthodox organization and a personalized treatment of subject matter, Warner's *The Urban Wilderness* is a creative and sophisticated urban history. It draws upon an impressive array of writings in history, demography, political science, economics, law, planning, housing, and medicine and health care. Fundamentally, the book examines the forces, innovations, and institutions that have shaped our urban society, provides a framework for understanding current urban problems, and advocates a variety of democratic planning tools to achieve humane and open urban communities.

It is impossible in a short review to do justice to the book's many and varied interpretive themes. One chapter elaborates the total planning of seventeenth-century New England towns; yet this "Puritan folk planning" was static in the face of a changing society, and orderly communities gradually eroded under the twin pressures of population growth and economic competition. A second chapter examines the American tradition of land usage, a tradition that viewed land as private property rather than a social resource. Thus, because of misplaced land-usage priorities, the urban gridiron, the federal township system, zoning, city planning, and federal highway construction have all contributed to today's "disordered, inhumane, and restricted city." In another section, Warner establishes a rough chronological framework for understanding the development of the modern American city. For each of three fifty-year periods (1820-70, 1870-1920, and

1920-70), he shows how transportation and technology shaped the city's internal and external economy, its land uses, its business and corporate structure, its job and residential structure, and its human and societal problems. Archetypal cities (New York, Chicago, and Los Angeles, respectively) are used to illustrate the general patterns for each period. Additional chapters trace the emergence of what Warner calls the "urban cultural consensus" and the simultaneous persistence of class, racial, and socioreligious differences; the inadequacies of urban housing, both past and present; and the failure of health delivery institutions to provide effectively for the whole population.

Beyond these stimulating and suggestive "present-oriented historical essays," as Warner calls his chapters, the author also advocates a variety of "solutions" for present urban problems. These range from national, regional, and community planning to democratic socialism and include such proposals as the following: the elimination of racial and class segregation and discrimination; government entrance into the housing field on a large scale to guarantee decent housing for all Americans; transportation planning combined with a "full-employment, living-wage policy" that would permit every family to own a "nonpolluting" automobile (Warner opposes the investment of billions of dollars in new mass transit facilities, preferring utilization of the existing highway network instead); democratic control of the huge bureaucratized workplaces in which Americans work; and a health delivery system that remedies actual medical needs and provides preventive services as well. The American public, Warner contends, must demand "that government and business serve the goals of a humane society." Such proposals, of course, have been advocated before, but they seem all the more reasonable considered in a historical framework. Specific ways, however, of achieving these basic goals go unmentioned and will, one suspects, remain elusive.

The book is enhanced by more than one hundred photographs that illustrate key themes of the urban experience; more than just supplemental, they are an integral part of the text and buttress the writing in a significant way. *The Urban Wilderness*, in short, is one of the most important general studies in

urban history yet published—an imaginative use of history to foster understanding of the troubled, urbanized society in which we live.

Like Warner's wide-ranging survey, two of the three essays in the second book under review also afford penetrating insights into the history of urban America. In "The City as a Melting Pot," Arthur Mann reviews the three traditional theories of ethnic assimilation in the United States (melting pot, Anglo conformity, and cultural pluralism) and posits a new and perhaps more useful model for making sense of our "multi-ethnic" society. In "Four Stages of Cultural Growth: The American City," Neil Harris explores a neglected dimension of urban history and proposes a periodization for the development of urban culture. The third lecture, "An Urban Historian's Agenda for the Profession" by Sam Bass Warner, Jr., is a very personal document that describes the many-sided crisis confronting historians and suggests some alternatives. For different reasons, each of these volumes is worthy of the attention of historians concerned with the American city.

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Florida Atlantic University

RICHARD SLOTKIN. *Regeneration through Violence: The Mythology of the American Frontier, 1600-1860*. Middletown, Conn.: Wesleyan University Press. 1973. Pp. viii, 670. \$25.00.

Since Henry Nash Smith's *Virgin Land* revealed the riches concealed in the mythology of the American frontier, a handful of literary scholars have explored aspects of the subject with impressive, if not spectacular, results. With the publication of this magisterial volume mythological investigation takes on a new dimension. Richard Slotkin, an assistant professor of English at Wesleyan University, has produced a book of monumental scholarship, enormous sophistication, and lasting significance. Few who read his 600-odd pages will dispute that he has convincingly demonstrated the influence of frontier mythology as a major force shaping the nation's literary outpourings between 1620 and the 1850s, or that the traditions evolved from the hunter-hero myth that he describes so expertly provide a structuring

metaphor for a study of the evolution of the national character.

His analysis of the nation's writing, both popular and literary, that substantiates these generalizations is terrifyingly thorough and utterly convincing. The Puritan's first contact with the New England wilderness, he shows, revealed especially in Thomas Morton's *The New English Canaan* and in the captivity narratives of the Pequot and King Philip's wars, made God, not man, the mythological hero, rescuing passive captives from the strokes of evil by grace, just as He regenerated their souls by salvation. By the end of the seventeenth century, as Indian conflict intensified, Puritan mythology revealed a lessening faith in righteousness as a tool for frontier conquest; man must debase himself to the level of his devil-like foes as he exorcised them from the forests, much as he might exorcise his own soul of sin. The wilderness was still a haunted land, but frontiersmen no longer dreaded its terrors so intensely as they mastered its mysteries.

Professor Slotkin sees a major turning point in mythogenesis with the 1716 publication of Benjamin Church's *Entertaining Passages Relating to King Philip's War*; for the first time Church made himself, not Providence, the hero of a captivity narrative, meeting his Indian foes as men, not demons. As other writers adopted this pattern a secularized view of the frontier emerged, based on rationalism and a deepening understanding, and with it a concept of the Indian as an idealized child of nature. Still another step toward the evolution of the hunter-hero was taken by authors who used Braddock's defeat to glorify American above British heroes. By the time of the Revolution, the ingredients essential for America's ideal mythological hero had been isolated.

The author who created this ultimate hunter-hero was John Filson when he added an appendix on "The Adventures of Col. Daniel Boone" to his 1784 edition of *The Discovery, Settlement and Present State of Kentucke*. Boone stepped from the pages of this fanciful tale as the archetypal frontier hero, the man who made the forest safe for democracy, concentrating within himself all the attributes of the hero-types that had emerged in colonial writing. From that time on the Boone myth was to serve as a barometer of the nation's cul-

tural progress, revealing in Boone's changing character alterations in the national consciousness. It even donned sectional garb, with New Englanders painting him as a debased Jacobin, Southerners as a disguised aristocrat, and Westerners as a realistic pioneer.

Against this background, Professor Slotkin argues, the Boone myth emerged during the 1840s as a distinctive literary form in the works of Cooper, Thoreau, and Melville. In the latter two particularly he finds the hunter-hero mythology generative, used in *Walden* to state in epic form the captive narrative in which the adventure impulse becomes a major force in the struggle to release the hero from forces imprisoning his mind and spirit, in *Moby Dick* to assemble the gallery of frontier myths with which Melville was familiar into a colossal hunt where all the elements of the frontier-hero myth are developed to their archetypal extremes.

No brief summary can do justice to the subtleties and complexity of Professor Slotkin's thesis; this is a book that should be read in its entirety by all with an interest in American writing, American violence, and the national character. The task is not to be undertaken lightly, for the author's multisyllabic vocabulary, sprinkled with the specialized terminology of the mythologist, and his leaden seriousness even when dealing with potentially humorous situations (he re-tells Thorpe's "Big Bear of Arkansas" tall-tale without cracking a smile) do not make for easy reading. Yet the reward is worth the effort. For those with the patience and skill to master the no-nonsense prose and thread their way among the bypaths that lead to a few dead ends, this is one of the few books that can be called definitive.

RAY ALLEN BILLINGTON  
*Huntington Library*

DAVID D. HALL. *The Faithful Shepherd: A History of the New England Ministry in the Seventeenth Century*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1972. Pp. xvi, 301. \$11.95.

Determining the effect of the New World on transplanted European institutions has long been a staple of American historical scholarship. In yet another work of the genre, David

D. Hall chronicles and interprets the development of the New England ministry from its English beginnings to the closing decade of the seventeenth century. His analysis deals with the birth and maturation of congregational patterns of doctrine and polity, the evolving relationship between Church and state, and the practice of evangelism among the clerics. The intent of the study is to discover how each of the first two generations of clergymen defined the role and status of the ministry and how their definitions mirrored the nature of the colonial experience.

The depth and richness of the written record left by the clergy in Massachusetts mean that the work is written largely from sources produced by Bay Colony clerics, but whenever possible material from churches in Connecticut is added to illuminate or strengthen interpretive assertions. His investigation also relies heavily on the writings of Continental and English clerics, incorporating material from theologians ranging in diversity from John Calvin to Edmund Calamy. Political, economic, and personal sources are ably employed along with the usual sermons and polemics to place the ministry in proper focus.

Although Hall concentrates on the evolution of colonial religion during the first twenty years of settlement, asserting that "within the context of Puritan history the change seemed predetermined," the progression he discovers is circular rather than linear, with the second generation of clerics forced by circumstances to modify their role and resurrect aspects of the rhetoric and doctrine their predecessors expounded in the first few years after their arrival in America. Within the compass of the thematic return to aspects of an earlier faith, several less vital issues are treated with considerable imagination. The conflict between advocates of an evangelical and a sacerdotal ministry, disputes over the nature of congregational church membership, and the problems of intrachurch membership are all interpreted in ways that might well cause revision of long-held conceptions on several aspects of New England's ecclesiastical history.

In any study that is dependent on discerning and evaluating causal factors, the essential task is to communicate the quality, intensity, and interrelationships of the factors in a manner

that is comprehensible and consistent. Hall is able to do this with skill. The conflicts between Church and state as well as disagreements between individual ministers and ministerial cliques are treated in a way that effectively communicates the preoccupations of the colonials to readers exclusively attuned to secular justifications for the use of power and the manipulation of men. But a certain measure of the empathy created with the clerics of three hundred years ago is achieved only by slighting the spiritual commitment that was central to the ministerial decision-making processes. The divines seem driven by considerations bound closely to authority, status, and security while matters of faith, duty, and a sense of devotion are included merely as peripheral items.

*The Faithful Shepherd* is a valuable addition to the historical literature of early America. Controversial interpretation is the stuff that induces more investigation, and surely by providing carefully constructed perspectives that will generate debate and give impetus to further research, Hall's work is an important step toward gaining a deeper understanding of the colonial period.

B. R. BURG  
Arizona State University

ITALA VIVAN. *Caccia alle streghe nell'America puritana*. (Nuova collana.) [Milan:] Rizzoli. 1972. Pp. 751. L. 7,500.

Italian scholarship has recently made notable contributions both to colonial American history (Giorgio Spini's *Autobiografia della giovane America* in 1968) and to the history of witchcraft (Carlo Ginzburg's *I benandanti* in 1966); but Itala Vivan's attempt to combine these two specialties is a failure that will be useless to American readers. Her thick volume has been built from a wide range of scholarship, much of it excellent: her debts to G. L. Burr, Perry Miller, and Kai Erikson, for example, are considerable and fully acknowledged. But she never consulted the voluminous unpublished sources on her subject; her book acquires its bulk largely through copious translations of important published sources. The author is not so much a historian as a belletrist, as the number of entries under Nathaniel Hawthorne in the index testifies. And while

she is concerned to tell Italians something more about a period not well known to them, the first century of American history, she has chosen to tell it as a long tale of Puritan repression. There is an eighty-page excursus on "Preti feroci" cracking down on dissenters from Morton of Merry Mount through the Quakers; worse yet, there are several quick asides to Sacco-Vanzetti, the late Senator McCarthy, and even Bobby Seale.

The most important redeeming feature of this book is its useful stress on the wilderness and especially on the American Indians in shaping the special contours of magical and witchcraft beliefs in colonial America (pp. 12-13, 53-76, 95, 123, 133, 162, 222, 259, 344, for example): here is a major reason why witchcraft in New England was somewhat different from old England. But even its illuminating discussion of the "demonic" redskins is a small reward from a large book that all too often merely substitutes new clichés for old.

E. WILLIAM MONTER  
Northwestern University

COTTON MATHER: *The Angel of Bethesda*. Edited, with introduction and notes, by GORDON W. JONES. [Worcester, Mass.:] American Antiquarian Society; Barre, Mass.: Barre Publishers. 1972. Pp. xl, 384. \$25.00.

In his own day Cotton Mather was accorded considerable status, but historians, physicians, and scientists in the nineteenth and early twentieth centuries relegated him to the dark ages. The era of debunking in the twentieth century, however, had a reverse effect upon Mather's image, and he has since emerged as a pioneer in medicine and science and a liberal in theology. If the process of brightening continues, it may well lead to a counter-reaction. Be that as it may, we now have the first complete edition of Mather's chief medical manuscript, "The Angel of Bethesda." Avid as he was for publication, it is surprising that he did not secure a publisher for it. The manuscript is long, however, some 410 pages or more, and it was not completed until the close of his life. By this time, too, he had already quarreled with most of Boston's physicians over the inoculation issue. For whatever reason, the manuscript languished for over two hundred years.

Mather's manuscript was dismissed by earlier

historians and lay readers largely because of its theological overtones. The manuscript is divided into sections, each of which deals with a specific disorder. In every case Mather begins with a short sermon in which he stresses the wrath and mercy of God and beseeches his readers to recognize their own unworthiness. Then follows a description of the ailment, and a list of the various therapeutics. Compared to the physicians of his day, Mather was surprisingly moderate in his prescriptions and demonstrated remarkable good sense. Yet he was a product of his times, and valid remedies are interspersed with calls for such items as crabs' eyes, warm urine and honey, sheep dung in white wine, and an infusion of hog dung and nettle-juice. In making these recommendations Mather was fully in accord with accepted medical practices.

Mather shrewdly observed that venereal disease was occasionally hidden under the diagnosis of phthisis (pulmonary tuberculosis), and he constantly urged his readers to avoid excesses—in drugging, diet, and bleeding. He was familiar with the current medical literature and well versed in the various medical theories. To his credit, he was one of the first to espouse the animalcular thesis, an early expression of the germ theory.

Dr. Gordon Jones has written a good introduction, carefully annotated the text, and made this rare medical treatise available. In so doing, he has contributed to our understanding both of Mather and his period.

JOHN DUFFY

*University of Maryland*

J. A. LEO LEMAY. *Men of Letters in Colonial Maryland*. Knoxville: University of Tennessee Press. 1972. Pp. xvi, 407. \$13.95.

This permanently valuable book is diminished by summary, for its importance lies less in its argument than in its evidence. With the zealous detective work of older literary scholarship but using the latest scholarly resources, Lemay meticulously pieces together hundreds of unfamiliar bits of information that compose a new picture of early Southern writing. The combination of old and new is significant because *Men of Letters* is the first local pre-Civil War literary history to appear in several decades.

Just after the older scholarship succumbed to the New Criticism, technological helps like Xerox and microprint made possible extraordinary extensions of the scope and depth of early American literary history. But while much new material became accessible, the type of literary history that might have used it went out of fashion.

The chronological-biographical format of *Men of Letters* allows this accumulated material to be fully presented. Covering Maryland literary life from Andrew White to Thomas Bacon, the book draws on wills, college registers, land records, English magazines, and Maryland newspapers, to settle many long-elusive points of fact, attribute many anonymous poems, provide rich bibliographical notes to guide further research, and fill in many details about early Maryland publications, publishing, and writers. In sketching the lives of such mere presences as Dr. Alexander Hamilton and James Sterling—poets one has always known of, while knowing little about—Lemay adds a new dimension to one's mental picture of the colonial Southern literary situation. Ultimately the richness of evidence itself is a major argument: that belletristic activity in the early South was self-aware and extensive, enlivened by celebrities, gossip, clubs, a marketplace, and other features of metropolitan literary life.

Lemay's interpretations of his evidence are challengingly fresh and bound to provoke argument. Several attempts to relate Maryland literature to later, larger ideas in American culture rest on assertions that one writer or work "foreshadows" or "prefigures" or "anticipates" some later writer or theme—language that hints at influence where only similarity is evident. *The Sot-Weed Factor* becomes an archetypal frontier put-on, satirizing not Maryland but the narrator's view of it, and English notions about colonial life. If so, one wonders, is the narrator's disdain for Quakers also a satire on English views of Quakers? Lemay's answer is that Cook exaggerates some elements of Maryland life just enough to satirize Maryland; he exaggerates others so much that they satirize the people who would think them true. But does either category contain Cook's description of the plantation feast of "delicious Meats" and "Turkies wild Luxurious Chear"?

To raise such questions is to demonstrate the

value of this book to the study of early American literature. There was nothing before it to ask such questions of. By reconstructing a large and often skillful body of writing in the framework of the older literary history, Lemay creates a new field of inquiry. One must read his book to have an adequate picture of the colonial literary scene.

KENNETH SILVERMAN  
New York University

DAVID S. LOVEJOY. *The Glorious Revolution in America*. New York: Harper and Row. 1972. Pp. xvi, 396. \$15.00.

Nine years ago Michael Hall, Lawrence Leder, and Michael Kammen stimulated new interest in a comparative treatment of late seventeenth-century colonial politics with their collection of primary sources entitled *The Glorious Revolution in America*. Now in a similarly titled study David S. Lovejoy has provided a long-awaited assessment of those critical upheavals that disrupted the American colonies from Bacon's Rebellion in 1676 to the major revolts in New England, New York, and Maryland in 1689. His interpretation is decidedly neo-Whig, which should provoke a lively debate among scholars of the period.

It is Lovejoy's thesis that by the 1670s and 1680s, the colonists "had come to some hard conclusions about the English empire and the meaning of their life in America," and that they "reacted to events and ideas sometimes in a fashion similar to that of their grand and great grandchildren in the 1760's and 1770's" (pp. xv, xvi). These hard conclusions were the result of a keen awareness and defense of individual and charter rights under assault by an intruding imperial bureaucracy. Lovejoy graphically describes England's new assertive role in commercial, political, and religious aspects of colonial life following the Restoration. Then, proceeding colony by colony, he recounts the conflicts over sovereignty, the application of English laws, legislative privileges, and individual liberties. He discusses with admiration the assembly debates, petitions, and grievances upon which he rests much of his argument for the ideological origins of the Glorious Revolution in America. To borrow Thomas J. Wertenbaker's phrase, Lovejoy por-

trays these rebels as torchbearers of the American Revolution.

This book properly reminds us of the widespread discussion of political rights and liberties in late seventeenth-century America, but it prompts many disturbing questions as well. Does Lovejoy not perhaps overemphasize the role of ideas with insufficient attention to the social bases of these revolutions? Were not these turmoils also power struggles over who should rule at home? Although the author observes the presence of factions, any internal divisions appear clearly less significant to him than the collective colonial response he detects to the new English imperial system. Finally, how did the majority of colonists, who were not directly involved, perceive these struggles, and what was the impact of the disturbances on society at large?

Most of the secondary literature on these revolutions fails to address these concerns satisfactorily; that literature remains quite uneven, largely narrative in nature, and generally restricted to a discussion of political events on the provincial level. For this comparative synthesis, Lovejoy has had to rely very heavily on his own extensive research, but this too has been largely in those published primary materials and official provincial records that afford little information for a broader social analysis of the period. It has been a considerable task simply to establish a reasonable chronology and account of the uprisings, which the author has done very well in this straightforward political history with its primary attention to the articulated issues of contention.

Answers to the troublesome questions and a testing of Lovejoy's emphasis on an ideological struggle may rest in the fragmented, widely scattered, and largely unpublished local records that historians are mining so profitably for their community and demographic studies. Such records for Maryland would suggest some important modifications of Lovejoy's thesis at least for that colony, where in 1689 rebels turned toward, not from, English rule in their revolt against Lord Baltimore. Men united against the proprietor in the political struggles of the 1670s and 1680s bitterly opposed each other in the final revolution. Career profiles of both factions suggest that significant social and political characteristics, not primarily positions



on political principles, distinguished the two groups and helped to influence their alignment. These fragmented county records do, however, support Lovejoy's picture of a more politically sophisticated and mature society than is usually assumed. Although the revolution rendered the colony without normal provincial government for three years, county institutions were little affected as opposing parties cooperated sufficiently at that level to ensure basic stability and continuation of both essential and routine political activities.

Maryland may be the exception, but much recent literature has demonstrated the importance of social analysis and the value of further research in the largely untapped local records. More intensive studies of these revolutions are still needed. Meanwhile, Lovejoy has provided a fine narrative of the period and a most provocative comparison of these important revolutions, a comparison that should challenge all students of the colonial political process.

DAVID W. JORDAN  
Grinnell College

IRA ROSENWAIKE. *Population History of New York City*. (A New York State Study.) [Syracuse:] Syracuse University Press. 1972. Pp. xvii, 224. \$12.50.

The main concern of this book is to present "the statistical dimensions of three centuries of change" in the population of New York City as they appear in the unusual number of censuses available for this town. In practical terms this means a focus on the actual size of the population of the city at various points in time as well as on the number of persons of different racial, ethnic, and religious backgrounds who inhabited New York throughout its history. In addition the author explores the reasons for the growth of population. He emphasizes the role of migration, not only because the movement of people in and out of the city was the main cause of its demographic change, but also because evidence on the effects of natural increase (the excess of births over deaths) is somewhat limited for the period before 1900.

Few historians will be surprised by the conclusion that one ethnic or racial group has continually been replaced by another throughout New York's past. Students of urban history,

however, will appreciate the detailed documentation of the rates and patterns of turnover. Among the more specific findings that should be of interest are the extraordinary loss of native-born Americans to the rest of the United States, the extent of the movement to the suburbs that began about 1830, and the fact that poverty and ghettoization were not universally associated with high birth and death rates.

In the end, however, this book may be of restricted value to many historians because of its narrow conception of the limits of demographic history. Surely some discussion of the age and sex structure of the city's population could have been added to the comments on size and ethnic and racial composition. Rosenwaike notes New York's economic attractiveness to immigrants. Yet he ignores the possibility that, by bringing in unusually large numbers of adult males, migration may have been as much a cause as an effect of economic development.

The book has merit. The author has done well what he set out to do. But it seems clear that if demographic history is to hold an interest for many historians, future work on New York (and other cities) must ask other questions of the censuses (as Thernstrom and others have done), and it must exploit more fully the noncensus sources that shed light on population change. Only with the most narrow view of demographic history could one claim, as the dustcover does, that this is "the definitive reference work on the demographic history of our nation's largest city."

ROBERT V. WELLS  
Union College

MAXWELL WHITEMAN. *Copper for America: The Hendricks Family and a National Industry, 1755-1939*. New Brunswick: Rutgers University Press. 1971. Pp. viii, 353. \$12.50.

Mr. Whiteman traces the business and personal history of the Hendricks family through four stages of the American nonferrous metals industry: the colonial and early national periods, when a vast carrying trade brought most of America's copper, tin, spelter, lead, and antimony from England; a period during and following the War of 1812, when South American imports were substituted; the years of uninterrupted search for native American ores and the

development of mills along the Eastern seaboard; and, finally, the exploitation of the copper deposits of the Great Lakes region and the gradual westward shift of processing and manufacturing facilities after 1840. In each stage members of the Hendricks family were leaders of the industry; firms bearing their name were active in the copper trade as late as 1939.

The bulk of Mr. Whiteman's narrative relates to the years 1755-1869, to the mercantile house founded in New York by Uriah (1737-98), and to the Soho Copper Works founded in Belleville, New Jersey, by his son Harmon (1771-1838) and carried on by Uriah II (1802-69). Extensive information is provided on the mechanics of the import and inland trades that supplied the raw-material requirements of America's coppersmiths, brassfounders, tin-smiths, plumbers, and pewterers (including such notable Hendricks customers as Paul Revere, Robert Fulton, and James P. Allaire). Similarly, the corporate history of the first successful copper-rolling mill in the United States and the rise and fall of a market for American-made copper ship-sheathing and locomotive and boiler parts are fully covered. As merchants and manufacturers the Hendrickses were nonferrous metals specialists and are important as pioneers in that industry; they do not, however, seem to have been innovators in general business administration.

In addition to these business details, the social life of the family shares center stage. Upon his arrival from London in 1755 the first Uriah associated himself with New York's Spanish-Portuguese congregation, Shearith Israel. In 1762 he married into the prominent Gomez family. Thus identified with the earliest Jewish settlers of New York City and affiliated with the same synagogue for over two hundred years, the Hendrickses played active roles in the cultural and religious life of New York's Sephardic community. And because kinship and business were so intertwined in this community, Mr. Whiteman's careful unraveling of genealogical ties is most welcome.

It would appear that the large Hendricks family collection in the New-York Historical Society is a treasure trove of early American industrial and religious history. Mr. Whiteman's organization and use of these materials is a good first step toward their full exploitation by

business, technological, and cultural historians.

JAMES P. BAUGHMAN  
Harvard University

CLIFFORD K. SHIPTON. *Biographical Sketches of Those Who Attended Harvard College in the Classes 1764-1767*. (Sibley's Harvard Graduates, volume 16.) Boston: Massachusetts Historical Society. 1972. Pp. 598. \$20.00.

Dr. Shipton begins volume 16 of *Sibley's Harvard Graduates* with this statement: "After forty years and thirteen volumes there is nothing new for the editor of this series to say by way of introduction." But being a pastmaster in the art of the biographical sketch, he has a wealth of information, entertainingly written, to contribute to the historian and the genealogist. The four Harvard classes in this volume totaled 197 young men, a cross-section of the professional and business communities of New England, with a few other occupations (farmer, surveyor, tavernkeeper) for good measure. Except for four students from other British colonies and two from Europe, they were natives of New England, mostly of Massachusetts, thus attesting to the provincialism of Harvard College in the pre-Revolutionary period.

These sketches of Harvard graduates reveal very little about them individually as undergraduates, for the operations of the college were officially recorded with a formality and brevity that tantalize the historian; collateral evidence is also sparse. Nevertheless, the repetitious nature of the data has its peculiar value, supplied in quantity, whether the information pertains to roommates or social status, to theses debated or disciplinary action taken, to student pranks or student membership in the Association for Suppression of Vice. Dr. Shipton's sensitivity for the apt quotation is well illustrated by the Butter Rebellion of October 1766, spearheaded by Daniel Johnson, class of '67, who, summoned before the faculty, stated "that the only Reason of their going into those Measures, was to procure *better Butter*."

One-third of the graduates became ministers of the gospel. By this period the occupational range of Harvard men was widening: 20 per cent became merchants, 16 per cent physicians, 12 per cent lawyers, and 11 per cent schoolmasters; there were a few unfortunates by reason of instability, changing times, or just hard

luck. The Revolutionary crisis, which began to quicken within a few years of their graduation, lends interest to the careers of these New Englanders, 24 of whom served in the American armed forces and 32 of whom became loyalists. Although only a few won reputations beyond their own region (e.g., Manasseh Cutler of the Ohio Company and Jonathan Loring Austin of the secret foreign service for the Continental Congress during the Revolution), among the total lot were two governors of Massachusetts (Caleb Strong and Increase Sumner), a noted educator (the Reverend Enos Hitchcock), and a bishop of Massachusetts (the Right Reverend Samuel Parker). Most picturesque was the French physician, Dr. Peter De Sales Laterrière, whose romantic and adventurous life has the makings of a fascinating historical novel. Some of these worthies appear among the thirty-one illustrations in the book.

The historian of eighteenth-century America should not overlook the rich crumbs of evidence on many aspects of life of this period, personal and social, interspersed throughout the biographies. Here is an excellent case of the whole being greater than the sum of its parts. The only shortcoming is the index, which focuses too exclusively on personal names and places of birth and residence of the graduates. Those rich crumbs are hidden among the 550 pages.

LESTER J. CAPPON

*Atlas of Early American History*

DONALD L. ROBINSON. *Slavery in the Structure of American Politics, 1765-1820*. (The Founding of the American Republic.) New York: Harcourt Brace Jovanovich. 1971. Pp. xii, 564. \$13.95.

The author, a political science professor, describes his book of eleven chapters and lengthy "Notes" as "a study of the impact of slavery on the founding of the United States as a national political community" (p. 3). In excellent detail he presents developments from the beginnings of slavery in English America through conventional periods such as the American Revolution, the Confederation, the Philadelphia Constitutional Convention, and the early presidential administrations. There are two chapters each for the Revolutionary era and the Constitutional Convention, another on "The At-

tempt to End the Importation of Slaves," and one in which the central concerns are the Northwest Ordinance and the Missouri Compromise. In the area of foreign affairs the chief topics are Jay's Treaty and the revolt in Santo Domingo led by Toussaint L'Ouverture.

Professor Robinson's book bears the marks of careful scholarship. Although he stresses that "slavery and racial prejudice" were "seeds of the sectional clash of 1860" that "were in the soil at the nation's founding" (p. 444), it is doubtful that any close students of American history will find any of his information new or startling. Most of his interpretations have long been available, especially in the works by such black historians as George Washington Williams, W. E. B. Du Bois, Carter G. Woodson, Lorenzo Greene, Benjamin Quarles, John Hope Franklin, and others.

Two of Robinson's statements on the Constitutional Convention are revealing. He writes: "The careless language of the fugitive slave clause reveals better than anything else in the Constitution that the fundamental problem for blacks in the union was that . . . white Americans in general did not hold them in just regard" (p. 230). He makes it clear, once again, that, in the areas of both domestic and international relations, slavery was one significant aspect of the Federalist-Antifederalist divisions.

At times the discussion of slavery is lost sight of, in the interest of that other part of the book's title, the structure of American politics. Although everywhere the plight and activities of free blacks were intimately tied to that of their brethren in bondage, free blacks are practically never mentioned. Probably of greater importance, however, is that here, once again, the voice of the slave is silent. No evidence is presented that any of the slave narratives were consulted. Although the author's excellent "Notes" (pp. 451-543) indicate familiarity with numerous sources that reveal the impact of the thought and deliberate acts of blacks, use of this material is not reflected in the text. Robinson points out that his central concern is "to record and to judge the ways in which the founders of the American political system dealt with" the institution of slavery, yet at another place he indicates that his goal is the larger one of "writing something about the black presence and its significance during the found-

ing of the American political system" (p. xi, xii).

Another possible defect of this book—but by no means peculiar to it—is that Robinson writes as though the defense or toleration of slavery by many white Americans was due solely to rational factors. It may be time for all scholars to consider whether the Founding Fathers and their immediate successors as national leaders were victims of such irrational forces as Joel Kovel points to in his book, *White Racism: A Psychohistory* (1970).

Robinson points out that, except on the subject of the slave trade, at the Constitutional Convention the South held to "absolute intransigence on the subject of power to abolish or regulate slavery" (p. 211). Because of such things as greed for profits, power, and social prestige, too few persons were willing to listen to James Madison's lament that the "distinction of color [had been] made, in the most enlightened period of time, a ground of the most oppressive dominion ever exercised by man over man." Many black Americans still lament that almost two centuries of enlightenment since 1787 have not been enough to end all of the "oppressive dominion."

EARL E. THORPE

North Carolina Central University

LETITIA WOODS BROWN. *Free Negroes in the District of Columbia, 1790-1846*. (The Urban Life in America Series.) New York: Oxford University Press. 1972. Pp. ix, 226. \$7.95.

Letitia Brown's meticulous study of the laws and customs governing slavery and manumission and the treatment accorded free Negroes in the District of Columbia before 1846 throws new light on the reasons for the steady growth of a self-respecting community of freedmen and a corresponding shrinkage in the percentage of slaves. Mrs. Brown discusses several circumstances that encouraged this development in the area carved out from the two states with the heaviest concentration of Negroes in the country in 1790: the ideological climate of the region in the post-Revolutionary period when belief in the rights of all men still ran strong in Virginia and Maryland; the "fluid quality" of the social structure taking form in the new

capital city; the "confusion, the loopholes, the oversights in the law, the scattered, episodic, uncoordinated physical development of the Federal facilities"; and the "good fortune" of having "in positions where actions could implement ideas" men who were sympathetic to Negro freedom (Massachusetts-bred Judge Cranch, for example, who presided over the Circuit Court of the District of Columbia for fifty years).

The volume of litigation initiated by slaves to establish their claims to freedom and by freedmen to defend other rights may well astonish readers familiar with the curbs imposed on Negroes by neighboring states in 1806 and after. In the District, unlike Virginia, manumission was never restricted or conditional on the freedman's leaving the jurisdiction within twelve months, and, unlike Maryland, no municipal ordinances or congressional acts barred incoming free Negroes from obtaining permanent residence.

Two appendixes supplement the discussion of free blacks' material progress, one listing their occupations in 1835 before a race riot led the city to limit them largely to menial jobs, the second naming Negro property owners and the assessments on their holdings at five dates between 1824 and 1846. Although, as the final chapter notes, "In many ways Washington crystallized into a Southern city as the Civil War neared, . . . elements of its earlier heritage and unique function persisted. . . . The special urban setting in the District with its tradition of contractual labor . . . continued to encourage enterprise and to supply enough employment for subsistence." If in this and other chapters the text seems excessively detailed at times, the preciseness of the resulting information is well worth some extra verbiage.

CONSTANCE McLAUGHLIN GREEN  
Washington, D. C.

JAMES THOMAS FLEXNER. *George Washington: Anguish and Farewell (1793-1799)*. Boston: Little, Brown and Company. 1972. Pp. xii, 554. \$15.00.

In this final volume Flexner is so immersed in Washington that at times he seems to be standing beside the man and reading his inmost

thoughts. Thus the account of Washington's death reads as if Flexner had been in the room at the time. Flexner's story is that of an aging man whose memory was failing and who made political mistakes that he would not have made when younger. The nadir was reached in 1798 when President John Adams appointed Washington to create an army in case of war with France. The quarrel over the appointment of subordinate generals leads Flexner to the sad conclusion that Washington blew up a "medium sized disagreement, into an issue that threatened to convulse the nation. And at various moments in the controversy the brilliant pragmatist seems to have lost contact with reality. . . . Not even the most resplendent hero is immune to the passing years."

There are numerous errors: John Beckley was clerk of the House of Representatives, not a congressman; Samuel Chase, not Thomas Chase, was appointed to the Supreme Court; state legislatures elected United States senators in all the states, not in "most states"; the "radicals" did not "often" create unicameral legislatures but created only two: Pennsylvania and Georgia.

There are good things, however. One is the account of Washington's desire to free his slaves. Another describes his efforts to sell some household goods and two horses to the incoming president, John Adams. Adams was a farm boy, too, and he had no intention of paying £1,000 for what he said were old horses. Flexner's account of Gilbert Stuart's portraits of Washington and of Washington as an art collector is fascinating. While Washington bought prints of the usual semidraped nymphs, he really wanted "realistic" paintings of the American landscape. Flexner's conclusion that Washington was the grandfather or great-grandfather of the Hudson River school of painters will intrigue some readers and startle others.

Thus in this, as in the previous volumes, one will find much of interest and value, but one will not find an adequate discussion of the public issues with which Washington was involved, or even many clear statements as to what those issues were. But this is not a history of the times. It is the history of a man written with sympathy and an undeviating belief in

Washington's greatness, even when he slips from the high pinnacle upon which the author has placed him.

MERRILL JENSEN  
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Madison*

ELWYN A. SMITH. *Religious Liberty in the United States: The Development of Church-State Thought since the Revolutionary Era*. Philadelphia: Fortress Press. 1972. Pp. xiv, 386. \$10.95.

The first two parts of this volume constitute a major contribution to American religious and intellectual history. Perhaps for the first time one can find between the covers of a single book powerful reinterpretations of the evidence surrounding a theme that was central to both Protestant and Catholic experience. Professor Smith's clarification in the first section of what he calls "the separatist tradition" of Church-state thought in the new nation rests upon his solid grounding in Reformed theology. He makes plain, for example, the significance of both natural law and eschatology in the ideas of Isaac Backus, and shows far more clearly than Sidney Mead or Wesley Gewehr did the interplay between popular Presbyterianism and the republican philosophy of James Madison. His powerful chapter on "The Moral Government of God" displays a familiarity with both the details of theological speculation in New England and the experiences of pastors who tried to bring that speculation to bear upon their immediate challenges. The discussion of the mid-nineteenth-century controversy between Protestants and Roman Catholics explains and to some extent justifies the Protestant point of view, thus preparing the reader to consider seriously the Catholic arguments set forth in the second part of the volume.

The six chapters dealing with the Catholic tradition, from John Carroll to John Courtney Murray, are somewhat more diffuse. Bishop John England, of Charleston, South Carolina, receives the thoughtful appreciation that is his due. With judicious borrowings from Thomas T. McAvoy, Smith is able in two chapters to make crystal clear the issues in public aid to parochial education that McAvoy, Robert Cross, and others have left unclear. He relies

too heavily, I think, upon Ray Allen Billington's work on mid-nineteenth-century nativism and neglects Vincent Lannie's study of Archbishop John Hughes's role in the school controversy.

In the final section Professor Smith provides a reasonably lucid account of the arguments of lawyers and Supreme Court justices dealing with Church-state issues, especially in the twentieth century. The style and substance of this section is more technical and theoretical than the earlier parts of the book, but its clarification of precisely how and why the metaphor of a "wall of separation" between Church and state in America has repeatedly confused lawyers as well as the public is a splendid achievement. I, for one, believe the links between the "constitutional tradition" Smith describes here and the various religious traditions will become clearer when someone produces an equally careful study of the attitudes and beliefs of American Jews.

The three sections constitute, to some degree, separate narratives of the history of Church-state thought, each overlapping the other in time. One may question this organization on the grounds that no one of these traditions was in fact at any given period developing without continuous dialogue with the others. The task of making clear what has been an impenetrably foggy historical swamp was the primary one, however; and if a choice had to be made, Professor Smith was wise to make the one he did, in preference to the pursuit of a reconstruction of the interplay of the various traditions in each period. He has managed to give to both the political theory and the history of American religion a clarifying set of insights.

TIMOTHY L. SMITH

*Johns Hopkins University*

GEORGE D. GREEN. *Finance and Economic Development in the Old South: Louisiana Banking, 1804-1861*. Stanford: Stanford University Press. 1972. Pp. xiii, 268. \$8.75.

To relate the institutional history of banking to economic development is an exacting task, requiring the historian to grapple with economic theory as well as to confront more familiar problems of data and interpretation. But to write banking history of the antebellum

United States is especially difficult because of the extreme decentralization of governmental authority over banking which prevailed in that era. Each state had its own peculiar (usually changing) policy "mix" ranging from outright prohibition or tight regulation of banking firms to permissive, or promotional, measures that encouraged proliferation of banks and laxity of operational practices. Moreover, both the First and the Second Bank of the United States lent additional complexity to the institutional framework and the dynamics of banking's impact on the direction and pace of economic change.

Professor Green, trained in both economics and history, uses his knowledge of the two disciplines to good effect in this case study of banking policy, practice, and economic impact in one state. After summarizing the principal economic theories that are relevant to an understanding of how banking interacts with the process of economic change, Green offers a summary account (cast in a matrix of business-cycle history) of how Louisiana banks functioned in mobilizing capital, providing a paper money supply to the economy, extending short-term and intermediate-term commercial credit, and providing long-term loans on the collateral of real property. He relates the development of banking to the state's public finance, provides full analysis of the legislative history of major legislation of 1842 and 1853, and, more generally, seeks to delineate the "ties to the outside world" that linked Louisiana banking (and the commerce that flowed through New Orleans) with the mercantile and financial institutions of the Anglo-American trading network.

The most original and striking feature of Green's study is the explicit effort to evaluate the rationality of Louisiana's public banking policies and bank practices in light of economic theory. Although he encounters difficult data problems, some of them quite unsatisfactorily resolved (e.g., consider the slender basis for estimating "income from Louisiana commerce," omitting even income from marketing imports, at page 197), one must welcome his efforts to develop a one-state statistical series that can aid in assessing the central problem of the book—the impact of banking on development. His main conclusions are that in the 1820s and

1830s financial intermediation by banks "accelerated economic growth" (p. 2), that from 1840 to the early fifties restrictive public policies hampered Louisiana's growth, and that after 1853 more expansive banking once again served as a stimulus to the "real economy" (pp. 4, 56, 163 ff.).

Green argues, too, that neither banking nor perpetuation of the slave-labor system substantially diverted Louisiana from the path of optimal development, in the long run, based on regional comparative advantage. But here he falls prey to the tempting prospect of assaulting historiographical straw men, taking aim at the "primary emphasis" of works that do not pretend to take adequate account of how Louisiana, with its great New Orleans entrepôt, deviated from the "Southern" norm. He also fails sometimes to deliver what is implicitly promised: for example, to demonstrate that the decline of the relative trade position of New Orleans can be explained rigorously by reference to inadequate credit facilities as well as to the broad impact of the transportation revolution; or, that Richard Easterlin's data and hypotheses can be interpreted quite differently than is customary (see the logical cul-de-sac at page 179, note 20).

However that may be, scholars are indebted to Professor Green for a bold, explicit introduction to the interpretive issues in the light of relevant theory, and for a thoroughly researched, thoughtful, and well-written case study of one state's banking system.

HARRY N. SCHEIBER  
*University of California,  
San Diego*

CARROLL SMITH ROSENBERG. *Religion and the Rise of the American City: The New York City Mission Movement, 1812-1870*. Ithaca: Cornell University Press. 1971. Pp. x, 300. \$10.50.

NATHAN IRVIN HUGGINS. *Protestants against Poverty: Boston's Charities, 1870-1900*. Foreword by OSCAR HANDLIN. (Contributions in American History, number 9.) Westport, Conn.: Greenwood Publishing Corporations. 1971. Pp. xiv, 225. \$10.00.

ALVIN W. SKARDON. *Church Leader in the Cities: William Augustus Muhlenberg*. Philadelphia: University of Pennsylvania Press. 1971. Pp. 343. \$15.00.

PAUL A. CARTER. *The Spiritual Crisis of the Gilded Age*. DeKalb: Northern Illinois University Press. 1971. Pp. xiii, 295. \$8.50.

The chief danger of a lengthy review that deals with more than one book is that it will to an inordinate degree express the reviewer's opinion on a general unifying topic and neglect the books under review. This threat becomes especially strong when, as in this case, the editor of the *AHR* has assembled a group of books whose interrelationships are both numerous and significant. But I intend to restrain myself.

First to be discussed are three excellent monographs, all of which are revised doctoral dissertations that demonstrate again the invaluable nature of such publications. I feel no inclination to make the familiar kind of condescending remarks, because each of these works exhibits the results of thorough research, careful documentation, thoughtful generalization, and felicity of style to a greater degree than many supposedly more mature monographs. Each of them makes substantial contributions to historical knowledge in an area of great importance to our understanding of the vast social and intellectual problems that beset nineteenth-century America. May their numbers increase!

Carroll Rosenberg's study of the New York City Mission movement and its impact on charitable activities provides an important new approach to the process by which socially influential New Yorkers became aware of poverty as a major problem and then organized themselves to do something about it. She describes how older views were transformed and new kinds of energy unleashed by the great revivals of Charles G. Finney and how this in turn led to city-wide evangelistic campaigns. Her intensive studies of the New York Tract Society and over a score of later charitable organizations reveal how efforts to reach all New Yorkers gradually led many earnest men and women to a discovery of the desperate condition of the poor and then increasingly to go beyond the distribution of tracts and Bibles to the offering of material assistance. As awareness of these needs deepened, less specifically evangelistic agencies were founded, but into these as well the pious aims of revivals continued to operate. As she brings the narrative down to 1870 one learns of an entire era in social reform, the emergence of pro-

fessional forms of social work, a growing realization of the importance of a blighted environment, and also the rise of serious efforts to enlist the legislative power of government, especially in the improvement of housing. In the end one is led to concur with her assertion that the earlier period was "as much the Age of Finney as that of Jackson" as well as with her larger conclusion that the Second Great Awakening had a powerful animating effect on New York's charities and that religious attitudes continued to be important even after the Civil War, when secular tendencies of many sorts were more pronounced. The author's precision of statement, use of evidence, and penetrating interpretations make the book an important contribution to a little-studied aspect of New York's history.

Nathan I. Huggins's carefully researched and sharply reasoned account of the essentially Protestant philanthropic agencies and the social and moral attitudes that dominated Boston's organized response to poverty appears now almost as if it were designed as a sequel to Carroll Rosenberg's work. Not only does he study the next half century, in which the "Charity Organization Movement" took over the less coordinated efforts of the antebellum period, but he follows up important themes developed in the New York study. Among these are the tendencies to professionalization and secularism that were so pronounced in Boston. Huggins, in fact, emphasizes the degree to which warmly Christian motivation that animated the advocacy of Channing and the pioneering labors of Joseph Tuckerman became almost imperceptible in a theorist like Edward Everett Hale. In contrast to the situation in New York and many other parts of the country, moreover, one cannot but note the relatively small influence in Boston of evangelists like Finney and the overwhelming importance there of the Unitarian establishment. The basic task, of course, for Huggins as for Rosenberg, was to provide a basic account of the way new agencies were founded and how they adapted themselves to increasingly heavy burdens. And in the 1870-1920 period, when governmental aid was still insignificant, this meant the development of greater efficiency and hence much closer cooperation. Be that as it may, Huggins's interpretation of changing attitudes toward the

poor as the problems of urban poverty became more serious and complex are unusually discerning.

Pervading Huggins's entire account, however, is another theme, which Rosenberg necessarily also dealt with, the immense degree to which all of the humanitarian endeavors in both cities were conditioned by a settled conviction that their effectiveness depended on a distinction between the worthy and the unworthy poor, a view that grew sharper and more culturally oppressive with the passage of the decades as problems of urban poverty deepened and as the poor came to be of non-Protestant background. Huggins by no means ignores organizational history, nor does he unfairly demean the motives and achievements of many earnest men and women in the movement. Indeed his intellectual history of the entire philanthropic impulse is remarkably sensitive. Yet his whole discussion is pointed to an observation in his epilogue that has a very contemporary ring. "The reformers," he says, "were unable to see a simple truth that has struck some present-day social thinkers: the problem of the poor is that they have no money."

Skarden's biography of William Augustus Muhlenberg (1796-1877) provides an important account of how one enormously active church leader with deep humanitarian commitments and even deeper concern for Christian nurture and the life of the Protestant Episcopal Church wrestled with the intellectual and theological tensions of his day and at the same time struggled (within the limits of his ecclesiology) to make the Church itself an instrument of social amelioration. Muhlenberg's youth was spent in Philadelphia, but after a brief pastorate in Lancaster he came to the New York area in 1827 and remained there until his death. During this half century he had two major careers, first as founder and head of Flushing Institute, a preparatory school for the sons of the rich that became a model for many other such institutions, a number of them founded under Episcopal auspices. After 1845, however, as rector of the Church of the Holy Communion in lower Manhattan, his outlook became considerably enlarged. He made his a "freechurch" in which the owners of pews would not hold inordinate power, and he even withheld his parish from membership



in the diocesan convention wherein pew holders were dominant. He also made his church a center of devout worship and liturgical innovation despite his strong antipathy for the Oxford Movement. In this same spirit he sought unsuccessfully to formulate a compromise between the Catholic and Evangelical wings of the Episcopal Church. With regard to specifically urban problems he is remembered for several efforts: the founding of an infirmary and tenement building and then of St. Luke's Hospital. As a means of staffing these and other similar institutions he founded a sisterhood of deaconesses "for ladies of quality who have passed the first bloom of life and are still single."

Skardon's biography goes far beyond existing scholarship on Muhlenberg's eventful career. Heretofore our knowledge has depended almost entirely on the adulatory *Life and Work* (1880), which Muhlenberg himself committed to his chief coworker, Sister Anne Ayres, with instructions that she thereafter destroy all of his personal papers. Since she did both tasks with extreme thoroughness, it took nearly a century and extremely zealous research efforts to produce a biography that would significantly enlarge our knowledge of Muhlenberg's many-sided career. But that time, fortunately, is now come.

As one moves in this review toward a consideration of the "spiritual crisis of the Gilded Age," it is natural to wonder how Muhlenberg responded to the many new intellectual dilemmas that pressed upon his later years. These, of course, were questions with which Anne Ayres could not effectively deal, but even with the flood of new information that Skardon brings one must say that Muhlenberg brought relatively timorous succor to the troubles of the age—whether on Reconstruction in the South, poverty, Biblical criticism, or Darwinism. Yet his constant wrestling with the issues reveals their depth and range and underlines the need for a comprehensive view of religious problems in postbellum America.

Paul A. Carter's work on the spiritual crisis of the Gilded Age, unlike the works so far considered, is not, strictly speaking, a monograph; nor is it a historical survey of the period. His approach is basically topical, with each of his ten chapters dealing in one way or another with

the religious, moral, and social aspects of the many problems of the age. One chapter uses Henry Ward Beecher's sensational career as a window on the times. Another uses the writings of Lew (Ben-Hur) Wallace, Ambrose Bierce, Elizabeth Phelps Stuart, and Margaret Deland to illuminate various crosscurrents of doubt and conviction. Other chapters tell of encounters with Darwinism, Biblical criticism, Eastern religion, racism, slums, immigration, the social Gospel, Catholic-Protestant conflict, and the new type of urban revivalism. Over two dozen drawings and cartoons of the day and very frequent allusions to or comparisons with dilemmas of the later twentieth century add liveliness to the account. The author's animated style as well as his well-selected quotations also heighten the reader's interest in the period. Over forty pages of highly substantive annotation make fully manifest the author's long and deep immersion in this field, though unfortunately they are not placed on the page where they could easily be read in context.

Taken as a whole the book is a serious and valuable contribution to our understanding of the period in American history that still remains less adequately studied and interpreted than any other. One of the strongest features of Carter's study is his clear depiction of crisis—the depth of the anxiety of the age, the diverse questions faced, and the way in which many of the age's answers adumbrate those twentieth-century problems that he treated in his earlier book on *The Decline and Revival of the Social Gospel* (1954). The chief shortcoming of the work is that its emphasis on the texture and color of the age (often revealed through minor writers) precludes detailed exposition of more eminent thinkers, such as Josiah Royce, William James, Borden P. Bowne, W. G. Sumner, Albion Small, and Henry George. Yet this work fills an important need, and it would serve excellently as a college text on a vital period of transition if it were issued in a paper edition.

As I stood back from the century of rampant growth, drastic social change, and deepening crisis that each of these books has in its way illuminated, I was tempted to state a considerable number of impressions that have had their validity strengthened. Yet I will restrict myself to two. One pertains to the continuing hold of

Protestant convictions and values despite powerful pressures of secularization. The other has to do with the remarkable degree to which the changes in Protestant thinking seemed to intensify rather than weaken the ways in which the Protestant ethic encouraged the growth of an unregulated capitalistic society. Both of these considerations, not surprisingly, emphasize the importance of the social as well as the religious aspects of history that each of these four authors has been pursuing.

SYDNEY E. AHLSTROM  
Yale University

JAMES P. BAUGHMAN. *The Mallorys of Mystic: Six Generations in American Maritime Enterprise*. (The American Maritime Library, volume 4.) Middletown, Conn.: Wesleyan University Press for the Marine Historical Association. 1972. Pp. xviii, 496. \$17.50.

James P. Baughman has reminded us that behind the stark trend lines that measure the patterns of economic development in nineteenth-century America there were real men and women doing things, making choices and money, building homes as well as firms, sometimes going bankrupt, and occasionally founding financial dynasties. These people actually had children who were more important to them than one could tell by analyzing demographic data, and the children and their parents sometimes disagreed even when they were all attempting to maximize profits in a common business venture.

In this case the family is the Mallorys and their business history began in the years following the War of 1812, when Charles Mallory, a sailmaker in Mystic, Connecticut, used the money and knowledge he obtained from his craft to become a maritime capitalist. He first took shares in other men's whaling voyages and then became a managing agent for various whalers; meanwhile, he had begun to invest in the carrying trade and then during the forties became a shipbuilder. In the following decade he wisely shifted his money out of whaling, concentrating instead on ship construction, coastal shipping, and finally commercial banking. By 1860 he had turned an initial investment of \$1.25 into a fortune of \$300,000—a success story that reminds us that while Americans

may not have been as upwardly mobile as they liked to think they were, some of them did manage to improve their situation slightly in the antebellum years.

The subsequent generations of Mallorys deployed and increased this fortune in a variety of maritime companies. They ran commission houses, guided the affairs of the New York and Texas Steamship Company, played a role in a twentieth-century nautical combine—the Atlantic, Gulf and West Indies Steamship Lines—and even provided some second-level leadership for the United States Shipping Board in the First World War. During the twenties and thirties, Clifford Mallory carried forward the family tradition by conducting a coastal and international shipping business, first under government auspices (1919–25) and then as an unsubsidized, independent operator. Baughman ends his saga in 1941, when the death of Clifford Mallory ended the family's involvement in this last seaborne business.

In recording the exploits of the Mallorys the author does not always avoid the sand bars that have grounded lesser business histories. At times he adds too many details of ship construction and cost, as well as business data that could have been buried in a footnote. American business historians would not suffer greatly, for instance, if they did not know that "Thornton Paillon (or Paillou)" was one of Charles Mallory's earliest journeymen (p. 18). Trivial details of this sort become especially bothersome as Baughman traces the Mallorys into the twentieth century, when their undertakings became more complex and at the same time less closely attuned to the main lines of American economic development.

These are, however, minor quibbles about an otherwise superb volume, clearly one of the best business histories of the past decade. Unlike many students of firm history, Baughman reports the bad news along with the good. He makes it quite clear that the Mallorys spent most of their career in a market protected by their government from foreign competition; cabotage was, in this case, the keystone of capitalism. In the early years of the nineteenth century, as Baughman shows, the family built a fortune that was in part wrung from labor through an exploitive system for paying wages. The author tells us about the Mallorys' mis-

takes as well as their successes, matching the debits suffered when the Brazil steamship line collapsed against the assets piled up in the trade between Galveston, Texas, and New York.

Most important are the clarity, authority, and honesty that the author displays when he is placing his subjects in their historical context. Baughman never exaggerates the extent to which the Mallorys were innovators. As he pictures them, they were rather cautious men who followed when the path was clear. This was true even in the first generation when Charles Mallory was founding the family fortune. This, I believe, will be the section of the book that will be of greatest interest to economic and business historians and, indeed, to all those concerned with America in the first half of the nineteenth century. Here Baughman does a truly outstanding job of showing the complex inner workings of an economy in transition. He blends history with micro- and macro-economic analysis, while never letting the reader forget that the subject at hand is a particular New England family that made its wealth at sea.

LOUIS GALAMBOS

*Johns Hopkins University*

JAMES D. HORAN. *The McKenney-Hall Portrait Gallery of American Indians*. New York: Crown Publishers. 1972. Pp. 373. \$29.95.

THOMAS L. MCKENNEY. *Sketches of a Tour to the Lakes, of the Character and Customs of the Chippeway Indians, and of Incidents Connected with the Treaty of Fond du Lac*. Reprint; Barre, Mass.: Imprint Society. 1972. Pp. xx, 414. \$40.00.

Thomas Loraine McKenney was a key figure in American Indian-white relations. As superintendent of Indian trade from 1816 to 1822 and then as head of the War Department's Indian bureau from 1824 to 1830, he had a dominant influence on trade relations with the Indians, on treaty negotiations, on education and civilization programs, and on Indian removal. Moreover he began a national museum of Indian artifacts and collected a remarkable gallery of Indian portraits that later formed the basis for one of the first great lithographic publications in the United States. With such a career it is hard to understand why he has remained so

shadowy a figure. Now, it appears, he is emerging from his obscurity.

James D. Horan's volume reproduces in color 125 portraits of Indians from the *History of the Indian Tribes of North America*, which McKenney published with James Hall between 1836 and 1844. Included also is a lengthy biography of McKenney. The book is grandly conceived and to the casual reader will supply a useful introduction to McKenney and his significant work. Yet the volume has many faults and will disappoint scholars of Indian affairs and of American art. The color reproductions, some of which are poorly done, were not all taken from the magnificent lithographs of the first edition of the *History*; many come from a later edition that did not capture the splendor of the original work. Horan has omitted the biographical and ethnological sketches of the Indians that McKenney and Hall supplied for their work and replaced them with brief biographies of his own. He has thus, unfortunately, eliminated much of the historical value of the original.

Horan's biography of McKenney is based on a wide array of primary sources and furnishes valuable insight into the man's character and career. It also tells a good deal about early American Indian policy. But the style is anecdotal—interesting stories get much more emphasis than they warrant—and sustained development is frequently lacking. Factual and typographical errors in the text, the notes, and the bibliography give evidence of carelessness or haste; they require that the book be used with caution.

McKenney's own *Sketches of a Tour of the Lakes*, now reprinted in splendid and expensive form, is the account of his excursion to the head of Lake Superior with Governor Lewis Cass of Michigan Territory in 1826 to conclude a treaty with the Chippewa Indians. It is a chatty and effusive book, which is one of the best accounts available of early treaty negotiations. It also reflects McKenney's concern for Indian affairs and his interest in Indian customs. An admirable introduction to this edition by Herman J. Viola sets the account in its historical perspective and tells the publication history of the book.

FRANCIS PAUL PRUCHA  
*Marquette University*

SYLVIA E. CRANE. *White Silence: Greenough, Powers, and Crawford, American Sculptors in Nineteenth-Century Italy*. Coral Gables, Fla.: University of Miami Press. 1972. Pp. xviii, 499. \$9.95.

Three expatriate American sculptors, torn in their loyalties between the States and Italy "like the ass between 2 bundles of hay," as Horatio Greenough described his condition, are the subject of Sylvia Crane's study. Based almost entirely on manuscript sources, Crane's triple biography adds many details to our knowledge of the day-to-day activities of Greenough, Hiram Powers, and Thomas Crawford. In this sense it supplements the work of Nathalia Wright on Greenough and Robert Gale on Crawford and makes available as yet unpublished correspondence and writings of Powers. But this additional information does not contribute much that is new to our understanding of the work of these men or to the expatriate experience. Because the author in her single-minded concern with manuscripts has ignored recent studies in the history of American esthetic theory and the social history of American art, she fails to place these men within longstanding American esthetic traditions or achieve deep understanding of the cultural complex in which they participated. In particular, she falls into the error of accepting as true the myths, fostered to a great extent by the artists themselves, concerning American indifference to the arts and the nation's "materialism" at this time in its history.

Greenough, Powers, and Crawford went to Italy to study and practice the art of sculpture because Italy afforded excellent and inexpensive marble, cheap skilled workers, and presumably an encouraging atmosphere for art. There they continued to work in the classical mode that dominated late eighteenth- and early nineteenth-century taste. Crane asserts that the three sculptors were responsible for "the transmission back home of the Italianate neoclassical aesthetics of their epoch" (p. xiii)—a questionable claim since the irony of their careers is that esthetic interest in the United States was moving away from neoclassicism even while the sculptors were achieving eminence in its practice. Greenough and Crawford returned home for a sufficient length of time to catch a whiff of the new romanticism before returning to the

sterilities of the older mode, and their later works and writing reflect their efforts to understand creatively some of its concepts. Greenough's functionalism, then, was derived as much from contemporary nature thought and his seventeenth-century Puritan past as these were embodied in Transcendentalism as from the theories of the German neoclassical critic Johann Winckelmann or the Italian architect Francesco Milizia. Crawford's belated efforts to define an American esthetic grew out of his attempt to fulfill governmental commissions that would reflect the American spirit as he understood it. Powers's art, on the other hand, did not develop beyond the imitative because he never returned to the land that had encouraged his best artistic skills. These sculptors did not so much carry the neoclassical ideal to America, where it had already made its converts, as they were encouraged by the fresh winds blowing in America to develop their most interesting and significant artistic insights.

Useful for its primary materials, Crane's book combines some new facts with outmoded interpretations and old errors. Since much of it involves repetition of what has been said well by others, and since there is so much still in American art history that requires exploration, it is regrettable that scholarly energy and intelligence should have been expended on a subject already adequately treated.

LILLIAN B. MILLER  
National Portrait Gallery

JOHN W. BLASSINGAME. *The Slave Community: Plantation Life in the Antebellum South*. New York: Oxford University Press. 1972. Pp. xv, 262. \$7.95.

This book promises new insights concerning the nature of slave life utilizing the trifocal perspective of slave, planter, and traveler. Unfortunately these aims are imperfectly realized, primarily because Professor Blassingame lacks a clear analytical perspective. The absence of a conceptual focus is expressed at all levels of the book from the ordering of chapters, through the muddled, often repetitious, discussion of issues within chapters, to confusion about the use of borrowed social science concepts. Most important, chapters 5 through 7, which dis-

cuss the nature of the plantation system and its consequences for slave activities, relationships, and personalities, should precede a discussion of the topics of chapters 1 through 4, which discuss leisure activities, African survivals in slave culture, resistance to slavery, and slave family life. Without a systematic institutional framework, the significance for slave life of the topics discussed in the first four chapters cannot be assessed. Nevertheless, Blassingame's discussion of three slave personality stereotypes in antebellum Southern literature is fascinating; his concluding historiographical essay provides a useful evaluation of primary and secondary sources on slavery; his methodological aims are important if not systematically pursued; some of his assertions are better considered as hypotheses for further study (for example, the prevalence of Sambo personalities among house slaves and on small plantations). Blassingame's overall contribution to the understanding of slave life, therefore, is frustratingly uneven.

A serious substantive weakness in Blassingame's description of slavery is his underplaying of the coercive role of blacks while he deals exhaustively with their resistance to slavery. In describing the enslavement of blacks in Africa, Blassingame mentions only indirectly the role of Africans in this process, though an appreciation of that role is essential for understanding the slave trade. Similarly, while Blassingame refers to the driver as part of the authority system of the plantation and deals extensively with the coercive role of the planter and the overseer, the author does not cope in any way with the social and psychological complexity of the black driver's role. Blassingame's failure to analyze the coercive role of blacks necessarily distorts his portrayal of slave life.

The most intriguing promise of Blassingame's book is the utilization of a trifocal perspective on plantation slavery. Although this perspective is not systematically developed, the viewpoints of slaves and planters are clearly articulated, while that of the traveler *qua* traveler never receives distinctive treatment. In the preface Blassingame suggests that scholars have rarely used slave autobiographies to understand slavery; while this may be true in general, it would be disingenuous not to recognize E. Franklin Frazier's seminal paper of

1930, "The Negro Slave Family," which is based on slave autobiographies, and such collections of slave autobiographies as Gilbert Osofsky's *Puttin' On Ole Massa* (1969) or George P. Rawick's recently published *From Sundown to Sunup: the Making of the Black Community*. As its title suggests, Rawick's insightful and analytically sophisticated study shares many of the aims of Blassingame's book; moreover, it is based upon the narratives of former slaves, narratives collected under the auspices of the WPA from people who were more representative of most slaves than the exceptional slave autobiographers on whom Blassingame relies.

Blassingame's *The Slave Community* promises more than it achieves, in part because his intellectual integration of social and psychological orientations has yet to be fully achieved, and in part because he attempts to satisfy a variety of audiences from scholarly peers to students in introductory black studies courses.

MARION D. DE B. KILSON  
Newton College

BETTY FLADELAND. *Men and Brothers: Anglo-American Antislavery Cooperation*. Urbana: University of Illinois Press. 1972. Pp. xiv, 478. \$11.50.

PENELOPE CAMPBELL. *Maryland in Africa: The Maryland State Colonization Society, 1831-1857*. Urbana: University of Illinois Press. 1971. Pp. vi, 264. \$7.95.

Frederick Jackson Turner's shade still lingers in the American historical consciousness. Like the great historian himself, most scholars search for domestic, purely American roots to explain national character and institutions. On the whole we tend to forget our commonality with transatlantic peoples, particularly the British, an interconnection that persisted long after formal ties were broken. Betty Fladeland's *Men and Brothers* challenges the parochial view, adding a political dimension to David B. Davis's brilliant studies of an evolving antislavery concept in Western culture. A model of thorough research, *Men and Brothers* reminds us that America, especially in its religious and reform tradition, participated in the vast social and intellectual transformation of the Atlantic littoral from the days of the Stuarts to the reign of Victoria and Albert.

Fladeland early sets the sympathetic tone of the work: "I count the antislavery advocates as worthy of respect" for their bold efforts in behalf of recognition of human worth. Although Fladeland admits that abolitionists could be "petty" and "overbearing" (p. xiii) whether they ate mutton or Boston beans, less reverence and more candor in the analysis would have sharpened the critical edge. Yet the author has fully mastered the inherent difficulties of conceptualization. Especially skillful is her account of the interplay of reform and self-interest, domestic politics and foreign relations, in the almost simultaneous outlawing of the international slave trade in both countries. Nor is she wholly uncritical. American abolitionists, Fladeland observes, occasionally fell into self-satisfied lassitude, most crucially at the time of the passage of a proslavery Constitution.

On the whole, Fladeland concludes, Anglo-American reform stimulated activity and exchanges of methods, but a price was exacted. As sectional feelings hardened, Southerners grimly warned of British imperial designs disguised in pious antislavery rhetoric. West Indian emancipation in 1833 intensified American nativism, proslavery fears, and mob hostilities. At the same time, the British measure radicalized and heightened the frustrations of American reformers, impressed by the success of "immediatist" demands abroad in contrast to unchecked slave expansion here. Throughout Fladeland reveals how British interest in American slavery, especially after 1833, became a significant factor in the rise of sectional antipathies leading to war.

To her credit, the author traces the parallels and continuities of Anglo-American reform, so often artificially divided by nation, chronology, and sectarian strategy. Yet the texture of an emerging transoceanic Victorian culture with its mass communications and uniformities is rather faintly portrayed. Moreover the theme of continuity hides the shifting class basis of transatlantic reform leadership. English antislavery became an increasingly strident social protest of a dissenting middle class against Tory hegemony, one reform among various measures to diminish feudal privilege. Parallel with the English trend, the American "wise and good" of enlightened benevolence relinquished antislavery to a new breed of evangeli-

cal romanticists in the late 1820s. Nevertheless, Fladeland has corrected a longstanding error: the view that American reform was cultivated in native soil without foreign nourishment.

Penelope Campbell's *Maryland in Africa* could well serve as an appendix to Fladeland's impressive study. Maryland's colony was, in a sense, a microcosm of transatlantic reform interests derived, as it was, from Britain's experiment at Sierra Leone. Campbell lacks Fladeland's style, complexity, and insights, but she shows that colonizationists were considerably less inhumane than Garrisonians so loftily proclaimed. One must admire the persistence of John H. B. Latrobe and James Hall as they strove to make the settlement a success. Fear of border-state free Negroes prompted this offshoot of the American Colonization Society, but Maryland philanthropists honestly endeavored to give freedmen an opportunity for selfhood unavailable in racist America. While blessed with fertile land and ordinarily peaceable African neighbors, the Cape Palmas settlers developed neither a political nor an economic resiliency. Finally they were absorbed into Liberia in 1857. Apparently the Negro colonists shared their former masters' aversion to manual labor, taste for alcohol and revivals, and contempt for native Africans, sometime victims of American black oppression. If this outpost was a tiny replica of Fladeland's world of Anglo-American culture, Campbell's interesting study suggests that white and black Americans, as well as Englishmen, evinced similar tendencies for imperial ambition, humanitarian paternalism and religiosity, along with some gentler traits that Turner had so eloquently celebrated.

BERTRAM WYATT-BROWN

*Case Western Reserve University*

DOROTHY M. JOHNSON. *The Bloody Bozeman: The Perilous Trail to Montana's Gold*. (The American Trails Series.) New York: McGraw-Hill Book Company. 1971. Pp. xx, 366. \$9.95.

*The Bloody Bozeman* is a chronicle of American occupation of the Northern Plains and Rockies, centering on Montana Territory, during the period 1862-68. The mining frontier entered this region in 1861 when a party of Californians found gold on a tributary of the

Clearwater in Idaho. By 1862 prospectors were ranging eastward into Montana. In July, John White found gold on Grasshopper Creek, miners swarmed to the Montana diggings, new fields were discovered including the fabulous Alder Gulch, and before the close of 1862 miners had established a cluster of camps across southwestern Montana centering on Virginia City. Several roads connected this remote gold mining region with settlements in Washington Territory, Salt Lake and Denver, and the East. The principal artery for access to the Montana gold camps was the Oregon Trail. Laterals from this transcontinental emigrant highway, including Bridger's Road, ran north to Virginia City and the peripheral camps. These roads were relatively free of Indian attack but, because rough terrain deflected direct routes to the camps, they followed time-consuming oblique courses. In 1863 John Bozeman blazed a more direct passage from the Emigrant Road at Deer Creek Crossing on the North Platte River to the Montana mines, reducing the passage by 400 miles. For the benefit in time and travel saved, Bozeman Trail travelers paid the price of great risk, for it coursed through the domain of the fierce Sioux. In 1866 the United States Army fortified the road with three military stations manned by cavalry and infantry. Red Cloud and his Sioux warriors bitterly contested this passage and regularly confronted army units guarding the road. In military annals these include the Wagon Box Fight and the Hayfield Fight. Relentless Sioux pressure forced abandonment of the military posts and the Bozeman Trail in 1868 as the principal route to the Montana mines.

*The Bloody Bozeman* is a melange of personal experiences, military and civil, along the Bozeman Trail and in the mining communities the road served. Through this medium the author presents engaging glimpses of life in the mining camps, mining camp law, vigilantes, and the Henry Plummer episode. The book's seemingly disparate, episodic, sketch-type content is laced into a provocative, if at times elusive, continuity by the Bozeman Trail theme. It is written in a familiar, homey style, drawn from letters, diaries, and other private primary sources of the miner and military pioneers along the Bozeman Trail. The work closes

with an epilogue section titled "What Happened to Some of Them," the principals of the drama along the Bozeman Trail, including John White, who made the initial strike at Grasshopper Creek, John Owen, Franklin Kirkaldie, the ubiquitous Jim Bridger, and Thomas Francis Meagher, international Irish revolutionary figure and acting governor of Montana Territory.

ARRELL M. GIBSON

*University of Oklahoma*

PHYLLIS DAIN. *The New York Public Library: A History of Its Founding and Early Years*. [New York:] New York Public Library, Astor, Lenox and Tilden Foundations. 1972. Pp. xix, 466. \$15.00.

C. H. CRAMER. *Open Shelves and Open Minds: A History of the Cleveland Public Library*. Cleveland: Press of Case Western Reserve University. 1972. Pp. x, 279. \$9.95.

For many years American library history was neglected by historians and librarians alike. Most of the books published were laudatory biographies or fact-filled institutional histories, frequently written from a provincial point of view and displaying more sentiment than scholarship. In the past decade, however, the situation has improved dramatically. Serious researchers are producing, with increasing regularity, studies that view library development as an integral part of American cultural history. The histories reviewed here take this wider view. Each author has made an effort to examine the political and social forces that shaped the institution. As a result, their volumes are useful syntheses that avoid many of the defects of traditional library history. Phyllis Dain's book is of particular significance.

Dain is a professor in the School of Library Science of Columbia University, where this work was first written as a doctoral dissertation. The first volume in a comprehensive history of the New York Public Library, it is described by the author as "an analysis of the decisive first years . . . viewed against the history of New York City and its library conditions." It also "complements and to some degree supersedes" Harry M. Lydenberg's history of the same institution (*History of the New York Public Library* [1923]), a task skillfully accomplished, for Dain not only updates Lydenberg,

she supplements the earlier volume by emphasizing subjects that he treated in a sketchy manner.

Professor Dain concentrates on the merger of the privately financed Astor and Lenox Libraries with the Tilden Trust, which established the New York Public Library in 1895, and the initial organizational efforts of its formidable first librarian, John Shaw Billings. In 1901 NYPL began absorbing newly created, tax-supported circulating libraries. The resulting growth of the circulation department, the planning and construction of the central building at Fifth Avenue and Forty-second Street, and Andrew Carnegie's gift of branch library buildings are also detailed. Even when discussing personalities or local politics, she stays focused on the processes that ultimately determined the library's unique character; the result is a well-organized, readable narrative. Despite her efforts to link the library and the history of the city in a direct manner, organizational details dominate; consequently, the volume must be viewed primarily as administrative history. But it is an admirable analysis, copiously documented with eighty-four pages of scrupulous notes. This handsome volume will endure as the authoritative account of the early years of a remarkable institution.

While informative, C. H. Cramer's centennial history of the Cleveland Public Library (1869-1969) is less successful. His book is especially disappointing since it is the first full history of the library and because Cramer—a professor at Case Western Reserve University—is an experienced historian. The problem is his limited purpose: "to attract interest in the major events, whether they are triumphs or failures, in the record of the Cleveland Public Library." This "highlights" approach may be suitable for the general reader, but it is too superficial to satisfy most historians. The volume was commissioned by the Friends of the Cleveland Public Library.

Despite his limited approach, Cramer has produced an interesting, crisply written volume. "Open shelves" for public perusal was an innovation of William Howard Brett, the distinguished librarian from 1884 to 1918. Cramer provides fine character sketches of Brett and John Griswold White, a fascinating book collector and library trustee who also exerted

great influence during the institution's early years. The author's frank description of the disastrous impact that narrow-minded city politicians had on the library during the 1930s is intriguing and thought-provoking. Unfortunately his coverage of later years is both inadequate and uneven, lacking the more balanced approach of earlier chapters. Skillful use is made of official documents and local newspaper accounts, and the forty-two pages of notes contain many colorful anecdotes, often based on personal interviews. Cramer's history is a useful and generally well-integrated overview, but it does not go far enough. A thorough analysis of the Cleveland Public Library and its importance to Cleveland and the American public library movement is still needed.

JOHN Y. COLE

*Library of Congress*

MARVIN LAZERSON. *Origins of the Urban School: Public Education in Massachusetts, 1870-1915*. (Publication of the Joint Center for Urban Studies of the Massachusetts Institute of Technology and Harvard University.) Cambridge, Mass.: Harvard University Press. 1971. Pp. xix, 278. \$10.00.

ARTHUR G. WIRTH. *Education in the Technological Society: The Vocational-Liberal Studies Controversy in the Early Twentieth Century*. (The Intext Series in Foundations of Education.) Scranton, Pa.: Intext Educational Publishers. 1972. Pp. xi, 259. Cloth \$8.75, paper \$5.25.

EDWARD A. KRUG. *The Shaping of the American High School*. Volume 2, 1920-1941. Madison: University of Wisconsin Press. 1972. Pp. xv, 375. \$15.00.

In the urban Massachusetts schools of 1900, classes of sixty children were common, sanitary conditions were often primitive, and forty per cent of the students took longer than six years to pass through the first six grades. Yet educators worked in a climate of buoyant faith about the possibilities of the public school system. Schools would wean immigrant children away from the bad influences of street life and their own parents, offering them hope for social mobility. Education was linked to goals of productivity and efficiency which seemed entirely appropriate to an industrial society. In American schools today class sizes are smaller, teachers are better trained, physical plants



usually somewhat more adequate. The faith, however, has largely evaporated. Cynicism and defeatism are far more prevalent, and fundamental questions are raised about the value of the undertaking that would have been unthinkable in earlier discussions.

These three books take us back to the age of confidence in education. Marvin Lazerson gives us a solid look at ten large Massachusetts school systems in the years from 1870 to 1915, as seen primarily through the eyes of those who ran them. His book is more descriptive, less thesis-ridden than Michael Katz's *The Irony of Early School Reform* (1968), which covers the mid-nineteenth century. Lazerson's book is grounded in a hard sense of social realities. Rhetoric is measured against concrete results. Like Katz, Lazerson emerges with a negative verdict: "Challenged to be relevant, educators adopted programs that claimed more than they could accomplish, that were soon found wanting, and that, once institutionalized, affected the schools far more than society's ills" (pp. 249-50). The broad moralism of the late nineteenth century was replaced by vocationalism and patriotic citizenship training. Vocationalism was supposed to bring the schools closer to the practical world of industry, but it failed to make a real connection; instead, its main effect was increasingly to segregate the classrooms of the poor from those of the middle class. Educators with limited budgets were too greatly concerned about making some impact in the slums to face the issue of declining democracy. Lazerson has produced an impressive, well-written monograph.

Arthur G. Wirth reviews the national debate over vocational education in the Progressive era. Steeped in the older Deweyan educationist tradition, he bravely tries to come to terms with newer perspectives, welcoming Paul Goodman as a Dewey for our times. He sees the battle as one between narrow, business-oriented advocates of social efficiency and humanistic utilitarians like Dewey. Despite occasional breadth of vision, the book plods. On page 173 Henry James (not Henry Adams) is credited with positing the contrast between the virgin and the dynamo.

By 1920 one-third of American youth were attending high school, and school leaders were pushing hard to make attendance universal,

armed with the slogan of social efficiency. Edward A. Krug, author of a highly regarded history of the high school before 1920, now continues the account down to Pearl Harbor. His research is wide-ranging and resourceful. In lucid prose he presents abundant material for a devastatingly negative appraisal of American secondary education in this period, and of the lackluster schoolmen who carried on third-rate intellectual debates (mainly quarreling over specific varieties of anti-intellectualism) while simultaneously bolstering their empires. But, except regarding the social conformism of the 1920s, Krug shrinks from quite making the indictment. A level-headed tone of description predominates. He has really written two quite different books, one on each decade. The treatment of the 1920s is far superior. We learn something of what students and teachers were actually like, and of concrete curricular practices as well as abstract philosophies. In discussing the 1930s Krug loses this focus, largely confining himself to an endless rehash of verbal controversies within the ranks of educators. We get a much clearer picture of the various factions than we do in Lawrence Cremin's *The Transformation of the School* but wonder if their rhetoric is worth so much attention. Had Krug stuck to social history, his book would be as consistently engaging as it is authoritative. It is noteworthy that in it Freud's name is not mentioned once.

Regardless of their authors' intentions, all three books will heighten the impression that American schools of the early twentieth century were generally mediocre and that their leadership dwelt in a curious cloudland.

LAURENCE VEYSEY  
*University of California,  
Santa Cruz*

DUNCAN LYLE KINNEAR. *The First 100 Years: A History of Virginia Polytechnic Institute and State University*. Blacksburg: Virginia Polytechnic Institute Educational Foundation. 1972. Pp. xiv, 498. \$6.95.

S. ARTHUR WATSON. *Penn College: A Product and a Producer*. Oskaloosa, Iowa: William Penn College. 1971. Pp. xiv, 417. \$7.50.

WILLIAM LLOYD FOX. *Montgomery College: Maryland's First Community College, 1946-1970*. [Rockville, Md.]: Montgomery College. 1970. Pp. xi, 115. \$1.95.

J. MARTIN KLOTSCH. *The University of Wisconsin—Milwaukee: An Urban University*. [Milwaukee:] University of Wisconsin—Milwaukee. 1972. Pp. xiii, 151. \$5.00.

The histories under consideration are concerned with four types of American colleges and universities that have been conspicuous in the rapidly changing educational scene since World War II. Two are centenarians that have been substantially altered in recent years, and two are emerging or developing institutions.

Each was intended to fulfill a designated "mission." Virginia Polytechnic Institute, founded in 1872, was the Virginia land-grant college, and it was deliberately separated from the older state university to ensure that education for farmers and mechanics would not be subverted by the older, elitist-professional education offered in Charlottesville. William Penn College, opened a year later in Oskaloosa, Iowa, was to provide sectarian-oriented training as well as learning for the children of a Quaker community in a limited geographical area. Montgomery Junior College was launched in 1946 to provide a terminal vocational education as well as the conventional preuniversity junior college curriculum for students of a suburban community of Washington, D. C., and to bring that education practically to their doors. The University of Wisconsin—Milwaukee, organized in 1955, is illustrative of a related movement whereby the state universities would be brought within easy reach of large population centers.

Like so many others of its kind, Wisconsin—Milwaukee began as a tightly controlled and subordinate branch of a jealous parent university. From the very beginning the leaders of the new institution have striven for independence on the ground that only by separation could they discharge the "urban mission" called for by its situation. Yet that mission has proved harder to define and to implement than it has been to announce. In fact, the Milwaukee university is already showing increasing resemblance to Wisconsin—Madison, including a foreign assistance program in which the chancellor-author takes great pride. They are even building dormitories at Milwaukee in order to vary the student mix by attracting nonresident students!

American institutions of higher education

tend to become increasingly alike. VPI exemplifies the once separate agricultural and mechanical colleges that have evolved into state universities not greatly different from the original establishments. Actually the process has been that of both types moving toward each other. It is probable that most of the community colleges can resist the powerful urge to upward mobility and homogenization. Yet the terms "junior" and "community" have been successively dropped from Montgomery's official name, and its historian may be giving us a glimpse of things to come when he makes a point of informing us that the college is now the second largest higher education institution in Maryland and that it appears headed for an expanding future.

If William Penn's example means anything, there is certainly little future in standing still. Unable to obtain any significant amount of public support, even from its own locality, and still dependent upon a regional denominational community that is in decline, Penn exemplifies the dilemma of a great many sectarian colleges unable to break loose from their original mission after it has lost broad appeal. Although it is presently in official good standing with the regional accrediting association, Penn has lost accreditation twice in its history, and it faces the grim realization that a great many institutions like it have gone under.

Only one of these books has much merit as a full-fledged history. Professor Kinnear of VPI knows a lot about his institution, and he loves it; he has done extensive research, writes with considerable vitality, and has provided a bibliography and index. A good editor might have given help on organization, at least to the extent of varying chapter titles, which plod on and on, administration by administration.

The history of Wisconsin—Milwaukee, nicely packaged, with all of the scholarly apparatus of citations, bibliographical note, and index, has some of the self-justifying tone of a corporation report. Written and published by the chancellor who has headed the institution since it was organized seventeen years ago, the book does have some worth as a memoir of the man who knows more firsthand about his subject than anyone else.

Montgomery College wasted the talents of a professional historian. It does not appear that

Professor Fox had full access to all of the material necessary for a full study, although, having been on hand since the college opened, he has managed to produce a readable and informative work within the limitations imposed. The book itself is cheaply done, and there is neither index nor bibliography.

The general level of the history of William Penn is apparent in the subtitle: *Product and Producer*. It is confusingly organized and loaded with lists of students, donors, and what-not. The most useful sections are those on Quaker settlement in southeastern Iowa and on the Quaker academies in the region; only one survives of the eighteen that were founded.

LOUIS G. GEIGER  
Iowa State University

WILLIAM S. POWELL. *The First State University: A Pictorial History of the University of North Carolina*. Chapel Hill: University of North Carolina Press. 1972. Pp. x, 309. \$12.95.

In an impressive volume whose format, dust jacket and all, can only be described as opulent, William S. Powell has brought together an interesting and remarkable collection of pictures in *The First State University: A Pictorial History of the University of North Carolina*. The author refers to the volume as "a picture book." It is certainly that, and an excellent one, rather than a history. That critical material is lacking is to be expected, but there is no index, a real necessity in a volume containing several hundred illustrations. There is a full page of credits indicating the source of much of the pictorial material. Looking over its three hundred pages, the reader feels that here is a book of plates designed to supplement the text of a more traditional history of Chapel Hill. The organization of the book suggests that it is designed for viewing rather than reading.

Starting with a few pages of uninterrupted narrative dealing with the launching of the university, the reader has then to jump back and forth across the two-column pages to read the captions accompanying the illustrations. Between some of these captions there is a degree of continuity; with others, particularly the many useful biographical notes, there is none. Reading is made difficult, and because of the

lack of an index, use for reference is made even more difficult.

No criticism of the form of the book can lessen respect for the author's industry, good taste, and skillful collection of such a vast amount of graphic material, so excellently reproduced. Future historians of the University of North Carolina and countless unnamed writers of promotional material will pilfer shamelessly from Powell's vast store of material illustrating the activities of the university and its people from the beginning of its history to the present day.

ELMER LOUIS KAYSER  
George Washington University

NORMAN CLEAVELAND, with GEORGE FITZPATRICK. *The Morleys—Young Upstarts on the Southwest Frontier*. Albuquerque: Calvin Horn Publisher. 1971. Pp. xi, 269. \$7.50.

A glance at the literature about the Southwest in the 1870s would not seem to indicate that William R. Morley was a figure of historical importance, but he was a man of some significance and even greater promise until his untimely death at the age of thirty-six. Like so many other veterans, Morley went west after the Civil War and worked as an engineer for the Kansas Pacific Railroad. He became manager and later vice-president of the Maxwell Land Grant Company in New Mexico Territory, a position that did not prevent him from supporting the interests of the settlers or from becoming one of the earliest and most vocal critics of the Santa Fe Ring. He was also one of the editors of the *Cimarron News and Press*. Later, as an employee of the Atchisón, Topeka and Santa Fe Railroad, Morley was a key figure in the race against the Denver and Rio Grande for control of Raton Pass and Royal Gorge and also selected the Santa Fe's route through Sonora.

Although centered around William Morley, this book is a family history by William Morley's grandson and is based on family reminiscences supplemented by research in primary and secondary materials. Although it is occasionally rambling and disorganized and should be used in conjunction with other books, such as Robert Athearn's *Rebel of the Rockies*, Howard Lamar's *The Far Southwest*, and Jim Pearson's

*The Maxwell Land Grant*, it does provide some new perspectives on the history of the Southwest and may be of use to regional historians.

RICHARD N. ELLIS  
University of New Mexico

KENNETH E. DAVISON. *The Presidency of Rutherford B. Hayes*. (Contributions in American Studies, number 3.) Westport, Conn.: Greenwood Press. 1972. Pp. xvii, 266. \$12.00.

Perhaps no historian has studied the life of Rutherford B. Hayes with more care and devotion than Kenneth Davison, professor of history at Heidelberg College. In a useful article in *Ohio History* ("The Nomination of Rutherford Hayes for the Presidency," 77 [1968]: 96-110) and in several convention appearances, Davison has impressed scholars with his meticulous knowledge of the nineteenth president and his family, the result largely of a decade of labor in the rich collections at the Hayes Library in Fremont, Ohio.

The major contribution of this volume is not its analysis of the issues confronted by the Hayes administration but its description of those who participated in the administration. Lavish attention is paid to the Hayes family, whom Davison admires without reservation. (At times this approach results in treacle. Of Lucy Hayes, for example: "She sang beautifully and even her call to breakfast—'Familee-ee-ee'—was sweet and full voiced.") There is no better single source of biographical information in print on the members of Hayes's cabinet. The White House staff—right down to domestic servants—is described at length.

Davison has collected a great many details about the operating costs and procedures at the White House that will attract some readers. One can learn, for example, the costs of banquets and carriage horses, the salaries of cooks, or how many laundresses were employed at one time. He surely has corrected the view, popular during the 1880s, that the Hayeses had been extremely reluctant to entertain.

On the larger matters chapters are almost exclusively terse summaries of the scholarship of others. The most positive interpretations prevail. Accounts of the campaign of 1876, the ad-

ministration's approach toward the South, and Carl Schurz's Indian policy are solid and make interesting reading. The author's understanding of the New York customhouse controversy, on the other hand, leaves much to be desired. His thin treatment of the silver issue and the railroad strikes of 1877 will disappoint many historians of the period. The contention that Hayes left the nation united, prosperous, and happy is, to say the least, debatable.

The volume contains an extensive bibliography, and includes a valuable list of dissertations written on relevant topics. A note on sources briefly describes the major collections in the Hayes Library.

THOMAS C. REEVES  
University of Wisconsin—  
Parkside

KEITH L. BRYANT JR. *Arthur E. Stilwell: Promoter with a Hunch*. Nashville: Vanderbilt University Press. 1971. Pp. xi, 256. \$10.00.

Other than his autobiographical forays in the 1920s, the only published knowledge of Arthur Edward Stilwell was presented by David M. Pletcher in his *Rails, Mines, and Progress: Seven American Promoters in Mexico, 1867-1911* (1958). *Arthur E. Stilwell* is, therefore, a welcome addition to the knowledge of this promoter's life as well as a welcome addition to the growing published literature dealing with the less spectacular businessmen of the nineteenth and early twentieth centuries. Knowledge of Stilwell and others like him is increasingly illuminating the nature of American economic growth and power, and the inevitable result will be a marked reconsideration of views of businessmen in this period. Arthur E. Stilwell certainly does not comfortably fit the interpretations of many American historians.

Stilwell entered the business community and successfully pursued careers in insurance, real estate investment, railroading, and writing. Most notably, after 1890 he conceived and guided railroad projects that resulted in two major lines with 2,300 miles of track—this when the American railroad network had been substantially completed and railroads were losing the confidence of the investing public. The Kansas City, Pittsburg, and Gulf Railroad connected Kansas City to Port Arthur, an in-

land port Stilwell created on the Gulf of Mexico. When he lost the KCPG to Edward Harri-man and John Gates about 1900, Stilwell began the Kansas City, Mexico, and Orient Railway, which was to connect Kansas City to Baja California. Stilwell lost control of this enterprise long before it was completed by the Mexican government in the 1960s.

After his unwilling retirement from business Stilwell turned to writing on a wide variety of subjects. His two most successful books were *Cannibals of Finance* (1912), which castigated the machinations of "Wall Street," and *Confidence or National Suicide* (1910), which defended the railroads against increased federal control.

One of the particularly interesting aspects of this well-researched work is chapter 2—"The Promoter and His Methods." Here the author relates Stilwell to some key theories and definitions of economic history, e.g., was Stilwell an entrepreneur or a manager? In comparison to similar biographies the reader therefore finishes the book with a surer grasp of what Stilwell was and what he was not.

BRIT ALLAN STOREY  
State Historical Society  
of Colorado

B. JOYCE ROSS. *J. E. Spingarn and the Rise of the NAACP, 1911-1939*. (Studies in American Negro Life.) New York: Atheneum. 1972. Pp. xii, 305. \$10.00.

This is more than a fine study of Joel Spingarn's role in the NAACP; it is the best account we have of the association during its first three decades. Spingarn joined the fledgling NAACP in 1910 and was elected chairman of the board of directors in 1914 to help heal an internal schism. One of the few white NAACP officers not of abolitionist descent, Spingarn nevertheless coined the phrase "New Abolition" to express the association's philosophy of uncompromising opposition to segregation. Ironically, it was Spingarn who in 1917 almost singlehandedly persuaded the War Department to create separate training camps for black army officers (as the only alternative to no officer training at all for Negroes), an achievement that caused embarrassment in 1934 when he opposed Du Bois's proposals for black self-

segregation as an instrument of social and economic progress. After distinguished combat service in World War I, Colonel Spingarn returned home broken in health and disillusioned by postwar reaction. His participation in the NAACP declined for a few years, but he did serve as treasurer in the 1920s and was elected in 1931 to the dual positions of president and chairman of the board, from which he steered the association through the difficult depression years until a brain tumor curtailed his activities and brought death in 1939.

Professor Ross has made good use of sociological studies of leadership and organizations to inform her interpretation of Spingarn and the NAACP. She is especially skillful in portraying Spingarn as an "interpersonal leader" who mediated factional and personal feuds within the NAACP. Ross also analyzes effectively the NAACP's evolution from an *organization* to an *institution* in the 1920s, a process that centralized control, ossified strategy, and inhibited the association's capacity to respond flexibly to black economic needs during the depression. Less successful is the author's attempt to apply the concept of "noneconomic liberalism" to the NAACP. True, many of the association's white leaders fit Seymour Lipset's definition of noneconomic liberals whose education, status, and psychic security enabled them to focus on issues of civil liberty rather than social welfare. But some of them were also socialists or otherwise active in economic and social reform movements. And to argue that noneconomic liberalism was the primary reason for the NAACP's continuing emphasis on civil and political equality slights the importance of other factors (especially the desire to avoid duplication of the National Urban League's efforts), fails to explain why socialists such as Mary White Ovington insisted that the NAACP concentrate on civil rights, and begs the question whether black leaders who took the same position were indeed noneconomic liberals. In any case, Ross's discussion of the crisis that led to Du Bois's resignation in 1934, of the consequent debate over future strategy, and of the gradual transition from white to black leadership during Spingarn's three decades of service to the NAACP are the best treatments of these matters in print. It is no easy task to write the history of a biracial or-

ganization; Ross has performed the task with grace and clarity.

JAMES M. MCPHERSON  
Princeton University

SIDNEY BELL. *Righteous Conquest: Woodrow Wilson and the Evolution of the New Diplomacy*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1972. Pp. 209. \$10.95.

Professor Bell, a Wisconsin Ph.D., presents still another New Left account of American diplomatic history. He notes that much of the debate about Wilson's role at the Paris Peace Conference has focused upon the manner in which Wilsonian goals were modified by Allied objectives rather than upon the nature of those goals themselves. He fails to refer to the recent careful study by N. Gordon Levin, Jr., *Woodrow Wilson and World Politics* (1968), which while emphasizing America's quest for a "liberal capitalist world order" avoids the simplistic and dogmatic analyses that this work reveals.

According to Bell, Wilson clothed America's special interests in terms of international principle and morality, viewing American society as on the verge of a social and economic crisis. Like many of his contemporaries Wilson saw the solution in terms of looking abroad, and when international complexities interfered with his vision of American needs and interests, he tried to define a "new system of relationships in order to 'make the world safe for democracy'" (p. 6). The new order would avoid internal factionalism and conflict by renewing opportunity provided by the growth of the United States, a nation of "the elect," in the world economy. The problem of understanding Wilson resides in an apparent disparity between his thought and his actions, but, as the author comments, moral principles are not policies.

Bell depicts Wilson's "New Freedom" as a "highly nationalistic coordinated campaign to expand American trade throughout the world . . ." (p. 41). He first became concerned with Latin America as the main area where he could apply his concepts—hence the interventions in Mexico and the Caribbean, and the genesis of Wilson's plan for a hemispheric pan-American pact. War in Europe caused the president to

project his concept to the world scene, envisaging a global community that would permit the United States to flourish everywhere. The author traverses well-known ground in relating the story of neutrality and the decision to enter the war upon the Allied side early in 1917, giving insufficient attention to Germany's motives in the U-boat campaign. His assertion that Wilson "moved to war almost automatically while still talking of peace" (p. 186) distorts a complex transitional period.

Even as Wilson took the nation into war, the image of the peace had begun to emerge in his thought: "American economic expansion required collective security—a primarily Anglo-American alliance against hungry rivals for markets and against revolutionary disruption and war" (p. 192). If these were his objectives, he achieved them at Versailles, the author concludes, and one may view the peace treaty and its failure ultimately as the failure of Wilsonian liberalism.

It is hard to find any real contribution to our knowledge of the period in this dogmatic volume. What it says has been better said elsewhere.

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ORVILLE H. BULLITT, editor. *For the President—Personal and Secret: Correspondence between Franklin D. Roosevelt and William C. Bullitt*. With an introduction by GEORGE F. KENNAN. Boston: Houghton Mifflin Company. 1972. Pp. xlvii, 655. \$12.50.

Researchers at Hyde Park denied access to parts of the Bullitt-Roosevelt correspondence may be interested now to find out what they missed. Some of these messages have appeared in whole or in part in *Foreign Relations* volumes for 1933-44, but much in this book, put together by Bullitt's brother from family papers, has not been published before. As first American ambassador to the Soviet Union, Bullitt entertained the president with lively vignettes. At a welcoming dinner in December 1933 he met Stalin, who offered to see Bullitt at once any time of the day or night. When he left Stalin took Bullitt's head in his two hands and gave him "a large kiss!" Bullitt added a curious compliment. "Like every real statesman

I have known," he told Roosevelt, Stalin has the "quality of being able to treat the most serious things with a joke and a twinkle in his eye. Lenin had that same quality. You have it."

Bullitt came to understand that the Russians lavished friendliness in 1933 only because they feared Japanese aggression. It took longer to realize that one did not easily understand Stalin or Kremlin politics. When S. M. Kirov was murdered the ambassador relayed the "highly confidential" news that it had been a crime of passion. The assassin Nikolaiev had killed Kirov because of his liaison with Nikolaiev's wife. Who could have realized in 1935 that Stalin was probably behind the Kirov assassination? Yet Bullitt's rush to report "the facts" suggests the level at which he early approached Soviet complexities.

Bullitt's correspondence is an odd mixture of intense idiosyncrasy and brilliant foresight. In June 1940, as ambassador to France, he demanded that Washington send submachine guns (he would pay for them himself) to protect Paris from the communist mob likely to appear when the French government fled. More often Bullitt's messages were prophetic. In a series of remarkable letters in 1943 Bullitt warned FDR that he did not understand Stalin's hope to dominate postwar Central and Eastern Europe. America ought to obtain promises from Stalin in exchange for aid to rebuild the Soviet Union. To make it more difficult for Stalin to renege, England and the United States should try to reach the eastern frontiers of Europe via Salonika and Constantinople before the Red Army. And if they did not? Europe would be divided.

There is no reply. Judging from this volume the president preferred conversation. If individually Roosevelt's answers seem lighthearted, in large numbers they are banal. "Pin a rose on Lenin when you attend the May first celebration. He is a great man because dead!" The level of FDR's correspondence rarely becomes more reflective.

By 1943 the president was no longer receptive to Bullitt or his ideas. Angered over Bullitt's role in forcing Sumner Welles from government—an imbroglio documented here—FDR refused his ambassador a position. Should Bullitt have been allowed to serve? George Kennan recalls in his introduction that

Bullitt told him "he had never subordinated his life to the needs of any other human being." And Bullitt's letters indicate that he was unlikely to see issues in other than black or white. Perhaps Bullitt would not have functioned well in the cabinet. Yet this correspondence suggests that few people in the early forties provided Roosevelt with a comparable range of insight and information.

BEATRICE FARNSWORTH  
Wells College

JERRE MANGIONE. *The Dream and the Deal: The Federal Writers' Project, 1935-1943*. Boston: Little, Brown and Company. 1972. Pp. xvi, 416. \$12.50.

In a photograph of the executive staff of the WPA Federal Writers' Project Jerre Mangione is seated at director Henry Alsberg's immediate right. Thus Mangione had, on that interesting venture of the New Deal into the arts, a seat with a view. Three decades later, after searching his own memory and the memories of other project members, reviewing the writings of and about the project, and going into the surviving records, he has come up with an account considerably more readable than most of the books the project itself produced. He has recaptured it all—the despair, the hope, the excitement. Like the repatriated writers of the 1930s recalling their nights and days in the Paris of the 1920s, Mangione and his former colleagues reminisce about the gallons of whiskey drunk, who slept with whom, the Stalinist-Trotskyist fights on the big-city projects, the Dies Committee investigations, and all the confusions and uncertainties that were characteristic of the project.

Unlike the carefully planned Historical Records Survey the Writers' Project sort of grew, mostly out of the mind of Alsberg. It put jobless writers and persons in related fields to work producing guidebooks: national, regional, state, and local. Eventually there was a published guide for each state and for some of the cities.

As the depression went on, the dreamers dreamed of "portraying the nation in such an honest and effective way that it would help create a more noble standard of social behavior." Or as one of the writers later expressed it,

their goal was "to produce the Story of America. Not just history, not merely the politics, the economics, the village folklore, the literature, but the whole thing." All this called for books "that would have revealed the nation's soul more tellingly than the guidebooks." The project began collecting life stories, slave narratives, and folklore, and material on ethnic groups, place names, and other subjects, and prepared a few local histories. The result was the 150-volume "Life in America" series.

Meanwhile the state projects were having enough troubles with the guidebooks without getting into soul-revealing. A few of the cities that were the natural gathering places of artists had writers to spare (including plenty of prima donnas); but out in the provinces they were scarce. The state projects filled with persons whom the relief agencies certified as "writers." Few really were. The Idaho state director, Vardis Fisher, after looking over his staff and not finding anybody who could write, in ten months singlehandedly wrote 374 of the 405 pages of the Idaho guidebook. Only his secretary and his typist were of any real help. Idaho's guidebook was the first to be published. Ray Billington, who in addition to teaching directed the Massachusetts project, had little doubt that "a well-trained team of researchers could have produced the Massachusetts Guide with one fourth the number and less than one fourth the time."

The guidebooks are, deservedly, what the Writers' Project is best remembered for. (And Mangione gives due credit for their quality to a dedicated headquarters editor, Katherine Kellock.) Included in the *Dream* is a bibliography of the project's publications. In view of the thousands of persons employed—6,684 at the peak—and the more than seven years the project lasted, the list is not impressive. Mangione mentions the hoard of material that did not get published—much of which is now stored in the Library of Congress—and suggests that all such material ought to be located and inventoried. This should be done, just as the nearly forgotten unpublished products of the Historical Records Survey should be located and inventoried. A guess is that the HRS material will prove a historical find, and that the final judgment of almost all of the Writers'

Project unpublished material will be that it was not and never will be publishable.

If the Writers' Project, viewed in retrospect, comes close to qualifying as a well-intentioned boondoggle, it was not a scandalous waste of the taxpayers' money. It made nobody rich. It helped a number of people through some bad times; and if when they left most were no nearer to being writers than when they came, the experience did them little harm. Within a few years many of them were fighting or working in a war, the cost of which exceeded in one day the entire expenditure on the project.

Mangione concludes that "the experience of the Federal Writers' Project offers no clearly defined lessons for those who champion the cause of governmental subsidy of the arts." But the book supports, even though the author may not, W. H. Auden's contention that the WPA arts project was perhaps "one of the noblest and most absurd undertakings ever attempted by any state. Noblest because no other state has ever cared whether its artists as a group lived or died. . . . Yet absurd, because a state can only function bureaucratically and impersonally—it has to assume that every member of a class is equivalent or comparable to every other member—but, every artist, good or bad, is a member of a class of one. . . ."

There was in the 1930s some good writing, of which a little will last; but not much of it was by the Writers' Project. Writing such as young James Agee's *Let Us Now Praise Famous Men*, which tells more about the tenant farmers of the depression South than all the life stories gathered by the project and which will be read when the project's books are out of print and forgotten, is not to be found among the project's publications. Auden was right. A federal writers' project is likely to make the wrong assumptions and to function in the wrong way, whether under a Roosevelt or a Nixon or a future president of one's choice. The talented, creative, serious writer has to be free to write as he pleases; and for that the garret, however uncomfortable, is better than the federal office building.

LEONARD RAPPORT  
*National Archives*

GEORGE H. GALLUP. *The Gallup Poll: Public Opinion, 1935-1971*. Volume 1, 1935-1948;



volume 2, 1949-1958; volume 3, 1959-1971. New York: Random House. 1972. Pp. xlv, 777; 779-1584; 1585-2388. \$95.00 the set.

The three large volumes of this collection contain the findings of all of the published Gallup reports from the founding of the Gallup poll in October 1935 through December 1971. There are more than 7,000 reports in all. Each gives the survey and the question numbers, and the dates on which the interviewing was conducted. The questions appear exactly as they were asked, but, to save space, the editorial and interpretive material that accompanied the reports has been eliminated. Most questions were asked of a cross-section of the national population. But there are also reports of Gallup interviews with special groups—among them the county chairmen of the two major political parties, college students, and interviews with persons listed in *Who's Who in America*.

From the very first Gallup question—"Do you think expenditures by the Government for relief and recovery are too little, too great, or just about right?" (60 per cent thought they were "too great" in 1935)—to the last—"What woman that you have heard or read about, living today in any part of the world, do you admire the most?" (the answer in 1971: Mrs. Golda Meir), the pages chronicle both the changing and the not so changing attitudes of the American people on a wide range of economic, social, and political issues. All of the great moments are here: the correct Gallup forecast that President Roosevelt would be re-elected in 1936; the public's reactions to the marriage of the duke of Windsor and Mrs. Simpson; the changing American attitudes in 1939-41 toward possible involvement in World War II; the great embarrassment of 1948, when the Gallup poll elected President Dewey (the actual deviation of the final poll from the election results was *less* in 1948 than in the Democratic landslide of 1936); the emergence and sometimes the fading of public perceptions of such figures as Dwight D. Eisenhower, Estes Kefauver, John Foster Dulles, Sherman Adams, Adlai Stevenson, John Kennedy, Lyndon Johnson, George Romney, and Richard Nixon; and a series of final pre-election polls that were for the most part breathtakingly close to the actual vote in the years since 1952.

The impact of technological change on

American life is also reflected in the figures, from the mere 4 per cent of the public who reported watching any part of the national conventions on television in 1948, to the emergence of near universal public reactions to events, personalities, and issues as a result of seeing them on television just a few years later. And there are the changing social concerns, as new issues that were hardly mentioned in earlier years begin to emerge in response to the famous Gallup question: "What do you think is the most important problem facing this country today?"

Several aspects of the collection will be of special interest to those who want to use the data for scholarly work. The trend data, which capture changes in public attitudes in response to the same or similar questions asked repeatedly over time, are particularly valuable. To take one example, Dr. Gallup apparently first asked a sample of the American public whether they approved of the "18 year old vote" in 1939. The answer: 17 per cent yes, and 83 per cent no.

Then came World War II, when the argument was pressed that "if they are old enough to fight they are old enough to vote." The trend during the war and thereafter was as follows: (The three numbers after each date represent, in order of appearance, yes, no, and undecided or no opinion.) October-November 1942: 41 per cent, 53 per cent, 6 per cent; March 1943: 42, 52, 6; August 1943: 52, 42, 6; February 1947: 35, 60, 5; August 1951: 47, 49, 4; May-June 1953: 63, 31, 6; January-February 1954: 58, 34, 8; July 1965: 57, 39, 4; March 1967: 64, 38, 4. By 1970, when the nation was on the verge of moving to the eighteen-year-old vote, the percentages stood at 58 per cent yes, and 38 per cent no.

The data also provide revealing insights into the attitudinal context within which American political leaders have had to operate. On some issues public attitudes were severely limiting. On others, however, they were permissive. Before President Roosevelt publicly committed himself to an attempt to lead the United States into a new League of Nations, Dr. Gallup asked the following question in December 1942: "Should the Government take steps now, before the end of the war, to set up with our allies a world organization to maintain the fu-

ture peace of the world?" The response was 64 per cent yes, and 24 per cent no. (As one reads through the findings, one wonders what the effect of some of the Gallup questions was on the political leaders of the time.)

Sometimes the data also illuminate the interplay—and the differences—between attitudes in the mass public and the views of more elite groups in the population. In the spring of 1953 the Gallup poll found that 57 per cent of the general public did not have an opinion about Senator Joseph McCarthy of Wisconsin. And in the minority with an opinion, sentiment was about evenly divided between those who were favorable toward the Wisconsin senator and those who were unfavorable. (The figures: 57 per cent no opinion, 2 per cent neutral, 19 per cent favorable, and 22 per cent unfavorable.) In 1953 Dr. Gallup also interviewed a cross-section of persons listed in *Who's Who In America* about Senator McCarthy. Only 9 per cent of this group had no opinion, and the rest divided 28 per cent favorable, and 63 per cent unfavorable.

Some additions might be made when what we hope will be a new supplementary volume of this Gallup collection appears in a few years time. A series of tables summarizing the answers given over the years to certain major questions that have been asked repeatedly would be helpful. As it now stands, one must work through all three volumes to come up with a set of data such as the trend of public thinking since 1939 on the eighteen-year-old vote. To make comparisons easier, it would also be useful if the actual election percentages could appear beside the detailed listings of the final pre-election polls. And one could use a fuller index with more cross-references, to facilitate the recapture of specific questions.

But these are minor quibbles. The collection is a major landmark in the history and measurement of American public opinion. It provides fascinating, and at times surprising reading. It will encourage many to delve more deeply into the basic reports themselves (complete data card decks for all of the Gallup reports released since 1935 are on file at the Roper Public Opinion Research Center in Williamstown, Massachusetts). And scholars who work with pre-1935 materials will envy their colleagues of the post-Gallup era who have

these reports as one more data source in their efforts to recapture and analyze the American past.

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*Foreign Relations of the United States, 1947. Volume 5, The Near East and Africa.* (Department of State Publication 8592.) Washington: Government Printing Office. 1971. Pp. ix, 1377. \$6.50.

The latest volume of *Foreign Relations of the United States* to deal with the Middle East, which covers the very critical year 1947, is a fitting illustration, not merely of the enduring relationship of the United States with that troubled area, but of the wide-ranging character of American policy and interest. The volume centers, appropriately enough, on developments along the northern tier of Greece, Turkey, and Iran and on the events in Palestine that were to lead, in the end, to its partition.

One notes at the very outset the regional character of American policy in the Truman Doctrine of March 12, 1947, for assistance to Greece and Turkey; the "Pentagon Talks of 1947" between the United States and the United Kingdom concerning the Middle East and the eastern Mediterranean; American participation in the development of Middle Eastern petroleum resources; the American interest in Communist and Nationalist activities in North Africa and the confusion in American policy relative thereto; the American interest in the proposals of King Abdullah of Transjordan for a greater Syria; and the American interest in resolving the dispute between Afghanistan and Iran regarding distribution of waters of the Helmand River.

The volume covers such a wide area that it is possible to provide only a sketch of the problems covered. Here one will find, for example, ample documentation (pp. 761-814) on the American interest in the Anglo-Egyptian negotiations for revision of the treaty of August 26, 1936, which still governed Anglo-Egyptian relations, and the question of possible Egyptian restriction of the right of transit through the Suez Canal. The latter problem, of course, persisted in one form or another throughout the post-World War II period.

The problem of aid to Greece and Turkey

covers some 484 pages. The research student is now able to get at the essential elements in the development of American policy as symbolized in the Truman Doctrine. A good section (pp. 816-89) of the documentation deals with the development of the Greek question in the United Nations during 1947 and the investigation of incidents along the northern frontiers of Greece and the activities of Greek guerrillas. The documentation supplements the more detailed records of the United Nations, though more evidence on this subject would have been useful. The section on Iran (pp. 890-998) is largely devoted to the Iranian-Soviet impasse over the agreement of April 1946 concerning the exploitation of oil resources in the northern part of Iran.

Especially noteworthy in this volume are documents regarding the Palestine problem, which take up some 345 pages (pp. 999-1328). These well-selected documents tell something of the story of the involvement of the United States in the Arab-Zionist conflict over the future status of Palestine, a conflict that culminated in the partition resolution of November 29, 1947. Among the interesting documents in the published materials is that of Loy W. Henderson, then director of the Office of Near Eastern and African Affairs, Department of State, of September 22, 1947 (pp. 1153-59), in which he indicated that he could not recommend the partition of Palestine as being in the national interest of the United States. As Mr. Henderson put the problem: "An advocacy on our part of any plan providing for the partitioning of Palestine or the establishment in Palestine of a Jewish state would be certain to undermine our relations with the Arab, and to a lesser extent with the Moslem, world at a time when the Western World needs the friendship and cooperation of the Arabs and other Moslems."

The Historical Office of the Department of State is to be congratulated on its selection and editing of documents in this volume dealing with developments in the Middle East since World War II. Complaint may be made, as indeed it is from time to time, as to the lag between events and the publication of the documents—some 25 to 26 years. While there are omissions, granted the tons of materials that come into the department, there would appear

little doubt that these volumes constitute a valid official record of American policy. All students of American policy and interest in the Middle East must now study the record of 1947.

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#### CANADA

SAMUEL ELIOT MORISON. *Samuel de Champlain: Father of New France*. (Atlantic Monthly Press Book.) Boston: Little, Brown and Company. 1972. Pp. xix, 299. \$10.00.

Samuel Eliot Morison has written some excellent books. This is not one of them. In the preface he states, "here is my best effort to honor one of the greatest pioneers, explorers and colonists of all time." In short, the work belongs more in the realm of hagiography than critical biography.

Champlain was certainly an outstanding figure of his time. He was a highly proficient navigator, and Morison deals with that aspect of his career in exemplary fashion. His chief claim to fame, however, was that for a quarter of a century he governed the commercial enterprise at Quebec that eventually became a thriving colony, but at his death in 1635 the struggling settlers numbered only some 150. Thus to claim, in the concluding sentence, that "no other European colony in America is so much the lengthened shadow of one man as Canada is of the valiant, wise and virtuous Samuel de Champlain, Xaintongeois," is to exaggerate.

No real attempt is made to put Champlain in a proper perspective. He always appears much larger than life. The involved background of forces, both in America and Europe, that controlled Champlain's actions is treated superficially, and some aspects are misrepresented. The abundance of factual errors, not all of them trivial, make it plain that the author lacks a proper familiarity with the history of the period, and of this particular colonial endeavor. We are told (p. 160) that the Iroquois-Huron war "dragged on for as long as the colonial period lasted." It ended in 1650. The old, bankrupt notion is resurrected that New France would have thrived had Huguenots been encouraged to settle there. The value of the French *livre* is equated throughout at five to

the dollar "in gold," whatever that may mean, which results in highly misleading comparisons. The context alone should have indicated that the *charrue*, so eagerly awaited by the first settlers, was a plow and not a "two-wheeled cart." The misspelling of other simple French words makes the author's unfamiliarity with the language all too obvious.

The literary style is, to say the least, racy. The reader is swept along by the swift flowing, narrative account of events, but is too frequently jarred by colloquialisms. Things are "all bunged up"; Indians "hit the war path" and are "rubbed out"; portraits are "phonies"; Henry IV "beefed up his company"; a murderer "continued to be a bad actor."

To sum up—there is nothing new in this book, except some of the mistakes.

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*Mike: The Memoirs of the Right Honourable Lester B. Pearson.* Volume 1, 1897–1948. [New York:] Quadrangle Books. 1972. Pp. x, 301. \$12.50.

This book is the first of a projected three volumes, but Pearson died December 27, 1972, while this review was being written. He would be the first to deny the proposition, *de mortuis nil nisi bonum*.

Mike Pearson was born and bred a Canadian and came from an Ontario Methodist parsonage. His world view in later years always retained that belief in the perfectibility of the human condition that is the eighteenth-century inheritance of the Methodists. After a familiar Toronto-Oxford education he became a history professor at the University of Toronto. But he was not an intellectual, and when he was given the opportunity to join the Canadian Department of External Affairs he did not refuse. He served honorably and well, and from 1942 to 1946 was at the Canadian embassy in Washington, becoming ambassador in 1944. He was prime minister of Canada from 1963 to 1968.

There is a good deal of Pearson in this book: his utter lack of pomposity, his sly sense of humor, his charming quality of being not infrequently a lovable loser, his understandable and unobtrusive pride in his own achievements. It is pre-eminently a book for Ca-

nadians, who will enjoy it, and it will puzzle Americans, who may wonder why Canadians published it at all. It is old-fashioned, prosy, and good-natured, like a leisurely tea in a spacious and sunlit garden in a quiet Ontario parsonage, taking the small with the large almost as if they were equal, very like Pearson, who took people as they came. He will often tell a story against himself—how, for example, he lost the university football cup for his college by dropping the football when he was behind his own goal line. One cannot but help enjoying his honesty. There frequently comes to the surface a fine sense of irony. Once in Washington during the war the Pearsons had a royal duchess to dinner at the Canadian embassy, and James the butler was serving a white wine, a Liebfraumilch. The royal dame asked the name of the wine. "Very conscious of the presence of Royalty and the impropriety of using German words in wartime, especially in such august company, James did a quick and literal translation: 'Milk of the Virgin, your Royal Highness.'" Or on Mackenzie King, the Canadian prime minister: "You would think of him on these occasions as merely a kind, simple old man and you could hardly be more mistaken."

When all is said and done, it is very much a Canadian book. Reviews of it in Canada have been generally favorable, and it is on best seller lists. But for Americans, this book is, in repeating Pearson's remark when he succeeded in making some small corrections to Churchill's Fulton, Missouri, speech, "a tiny footnote to contemporary history."

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#### LATIN AMERICA

M. S. AL'PEROVICH and L. IU. SLEZKIN. *Novaia istoriia stran Latinskoï Ameriki* [A New History of the Countries of Latin America]. Moscow: Izdatel'stvo "Vysshiaia Shkola." 1970. Pp. 382.

M. S. AL'PEROVICH. *Ispanskaia Amerika v bor'be za nezavisimost'* [Spanish America in the Struggle for Independence]. Moscow: Izdatel'stvo "Nauka." 1971. Pp. 221.

Soviet and most of U.S. historiography on Latin America is founded on radically different axioms, the one on dialectical materialism, the

other on a variety of economic, social, and cultural assumptions. During the Stalinist years Soviet historiography was shackled by dogma that lumped all Latin Americans, past and present, into the "counter-revolutionary camp." Since the mid-fifties there has been a return to Leninist analysis, which is far more subtle, drawing many more distinctions within the "bourgeois" world, and great strides have been made in both the quantity and quality of scholarship. These two works are a continuation of this improvement.

Al'perovich is the dean of Soviet historians specializing in Latin America, and for fifteen years he has written or edited numerous works on the wars of independence, the national period, and the Mexican revolution. Slezkin is a specialist on the Cuban revolution who more recently has concentrated on Brazil. Their coauthorship of *Novaia istoriia* is designed to meet the need in the USSR for a college textbook on Latin American history from the sixteenth century to the end of World War I, to help fill the gap in "Marxist scientific and popular literature" on the subject. It is not a complete textbook because of space limitations, as the authors state at the outset. The colonial period is reviewed briefly, primarily as an introduction to the wars of independence to which a third of the book is devoted. The rest concentrates on the subsequent histories of Argentina, Brazil, Chile, Cuba, and Mexico, while the other Latin American countries are discussed only in their relation to important historical events.

Latin American history is presented in terms of class struggle, and those areas of the eighteenth and nineteenth centuries that smack of twentieth-century issues are highlighted. The Haitian revolution of the 1790s and early 1800s receives more than ordinary attention for a textbook and is credited with having influenced slaves in the Hispanic colonies and the United States to rebel. The Petion constitution of 1816 is cited as having prohibited foreign ownership of property in Haiti and thereby protecting the country's new-found independence. In actuality the constitution prohibited any whites from ownership; after twelve months' residence foreign blacks had all the rights of a Haitian citizen including land ownership. Nevertheless, the point is interesting

and deserves further investigation. The War of the Pacific also receives special attention, and the role of British capital in it is emphasized. The British had investments in both Peru and Chile, but in Peru they were of low value because of their exclusion from nitrate mining and the continued financial crisis of the government. In Chile, on the other hand, there was a financially solvent government that gave the British free rein in the mining of nitrates. When a showdown came over the mineral rich areas of Tacna, Arica, and Tarapaca the British strongly supported the eventually victorious Chileans and thus became primary beneficiaries of the war.

*Ispanshaia Amerika* is another addition to the growing Russian bibliography on the wars of independence, a favorite subject of Soviet historians. The independence movements in Latin American colonies are seen as having many aspects of a bourgeois revolution, in that in most areas they ended a number of forms of feudalism such as slavery, compulsory work by Indians, royal monopolies, and prohibitions and regulations that inhibited economic growth. More favorable conditions were created for establishing Latin American capitalism and drawing the region into the world economy. The struggle with Spain temporarily united all classes: Negroes, Indian peasants, petite urban bourgeoisie, and landowners. The main burden of the struggle was carried by the masses who did the majority of the fighting, but because of the weakness of the petite bourgeoisie and the incapacity of the peasants to lead, the movement for the most part was led by Creole landowners. Thus the masses were unable to put forth their own class requirements. Hidalgo's revolt in Mexico and Francia's regime in Paraguay were in part exceptions in that fundamental questions about landownership were raised. The Creole landowners feared the possibility that the independence movement would become a social revolution and restrained any activity in that direction.

Both books are recommended for the insights they provide into Leninist analysis of Latin American history, but a serious drawback is that neither has reference footnotes. While this is understandable for a textbook, it is less so for *Ispanskaia Amerika*. It is important that the Soviet historians footnote their

works on every possible occasion, because their different interpretations of particular events or documents inspire Western scholars to consult the original sources to see if indeed their views are more valid than conventional Western interpretations.

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[FIDEL CASTRO.] *Fidel in Chile: A Symbolic Meeting between Two Historical Processes. Selected Speeches of Major Fidel Castro during His Visit to Chile, November 1971.* New York: International Publishers. 1972. Pp. 234. Cloth \$7.50, paper \$2.65.

JAIME SUCHLICKI, edited and with an introduction by. *Cuba, Castro, and Revolution.* Coral Gables, Fla.: University of Miami Press. 1972. Pp. xii, 250. \$7.95.

LOWRY NELSON. *Cuba: The Measure of a Revolution.* Minneapolis: University of Minnesota Press. 1972. Pp. xiii, 242. \$10.00.

MAURICE HALPERIN. *The Rise and Decline of Fidel Castro: An Essay in Contemporary History.* Berkeley and Los Angeles: University of California Press. 1972. Pp. x, 380. \$12.95.

The flood of books on Cuba is unceasing. By now, probably fortunately, the early torrent of facile, lightly rooted, and often misinformed journalistic accounts has tapered off, and many of the new studies are scholarly products of permanent value. The four volumes under consideration in this review are all valuable, and in three cases will deserve a lasting place on the much-more-than-five-foot-shelf of sober studies devoted to Cuba.

The one that can be treated most briefly is *Fidel in Chile*, ambitiously subtitled with a quote from Castro: "A symbolic meeting between two historical processes." This slender volume contains a selection of the many speeches, sometimes abbreviated, made by Castro during his whirlwind three week visit in Chile in late 1971. No editor is identified, and the editorial treatment in some places is more lyrical and less tight than might be desired. The freewheeling and exuberant Castro was at his oratorical best in many of his speeches in Chile and in exchanges with members of his audiences. Many of the questions asked of Castro sound "planted."

The collection of speeches is pure Castro,

filtered through rosy-tinted editorial glasses, but as source material the book is extremely useful.

The volume edited by Jaime Suchlicki, who also contributes an excellent synthesizing introduction, is a collection of essays by authorities on various aspects of the Cuban scene. *Cuba, Castro, and Revolution*, as a title, covers a broad spectrum but the contents are as varied as the title would suggest. The essays were written specifically for this collection and in most cases report on the findings of the respective authors' ongoing research projects.

W. Raymond Duncan discusses Cuban nationalism, Lowry Nelson reports on changes in the island's social structure, Sergio Roca and Roberto Hernández deal with economic problems, Gemma Del Duca treats the Cuban cultural scene, former ambassador Foy Kohler writes of Cuba and the Latin American-Soviet problem, Leon Goure and Julian Weinkle continue the Russian aspect with an essay on "the growing integration" between Cuba and the USSR, and Michael Kline concludes the volume by writing about Castro's challenge to Latin American communism.

It is not to denigrate the other essays, all of which were competently done, to record that this reviewer felt that the meatiest ones were those by Duncan, Nelson, Roca and Hernández, and Goure and Weinkle. Almost all of the seven essays reveal a high level of scholarship, and all but one are carefully documented.

Lowry Nelson's *Cuba: The Measure of a Revolution* is a sober and altogether excellent assessment of the political, social, and economic consequences of the convulsion that has been Cuba during the almost decade and a half just past. Nelson, author of the landmark *Rural Cuba* (1950), could not repeat his earlier field work in Cuba inasmuch as the Cuban government would not grant him a visa, but his consultation of sources in the United States, both documentary and personal, was extremely thorough, and the result is an entirely scholarly and well-documented study.

The study will not win its author any medals from the current Cuban government. Its net effect is to upset many of the common assumptions about the revolutionary regime. All revolutions, Nelson properly points out, are matters

of controversy and cannot be put in perspective for many years or even generations. The Cuban revolution is the more controversial both because of its extremes and its intricate involvement in the power plays and feints of the world's two great states.

The main divisions of the study are essentially historical, economic, and sociological. The author on balance gives full credit for moral reforms, educational achievements, and other accomplishments, but he also points up the fantastic downhill slide of the Cuban economy, the quixotic dangers inherent in keeping the revolution in Castro's own pocket, i.e., failing to institutionalize it, and the anesthetizing of all critical reaction to political judgments from the Cuban masses.

Halperin's *Rise and Decline of Fidel Castro* is based on more firsthand Cuban experience than the books of any of the other authors (*Fidel in Chile* of course excepted). He spent nearly six years, 1962-68, at the University of Havana. The result is more of a feel for the nuances and subtle byplays in Cuban politics than the other studies here reviewed show. The author's ample documentation is drawn largely from Cuban sources, *Hoy*, *Revolución*, *Granma*, and numerous books. This is both an advantage and a liability. Many *norteamericano* writers have not had access to such long runs of the periodicals, and yet it would be the ultimate in fallacies to believe those sources are objective or unbiased.

Halperin's treatment is far ranging, especially with regard to foreign policy involvements. He is no Castrophile, but his basic sympathies with the Cuban revolution do come through loud and clear. Yet his biases do not prevent his seeing the decline as well as the rise of the charismatic Castro. Though the chief emphasis is on the period through 1964 (a second volume is promised) there are flashbacks and glances ahead in such fashion that the organization is at times chronologically confusing.

The study is a valuable one, especially by virtue of its full-dress treatment of the complex, delicate, and often contradictory maneuvering of the Castro regime in Latin American and world politics.

RUSSELL H. FITZGIBBON  
University of California,  
Santa Barbara

WILLIAM B. TAYLOR. *Landlord and Peasant in Colonial Oaxaca*. Stanford: Stanford University Press. 1972. Pp. x, 287. \$10.00.

A work in Latin American colonial history dealing with a province remote from any capital, covering the whole colonial period, and synthesizing an aspect of social-economic life from a broad, scattered documentation, can hardly fail to make an important contribution to the field. Taylor's book, thoroughly and judiciously done, belongs to a second or post-Chevalier wave of agrarian studies; abandoning Chevalier's eclecticism, it concentrates on the single region of the valley of Oaxaca, with relentless attention to land tenure, somewhat like Borde and Góngora's *Valle del Puangué*. The study also, Taylor being a student of Charles Gibson's, in some respects comes close to being a corporate history of the Indians of the region. The social or general history that has dominated scholarly writing in the field for some five years is not without its influence either, since Taylor's book goes far beyond the narrow restriction to land that made the Borde and Góngora study so relatively arid, to become in many ways a general regional history. Whereas most books have a derivative, often outmoded *mise en scène*, Taylor's "The Setting" is a meaty chapter of largely new social and economic information.

Countless maps and tables give the reader an overall picture. A more basic value, however, attaches to the four central chapters, which (together with the appendixes) provide not merely aggregate information, but an exhaustive series of reconstructions of individual entities and organisms through time—chieftaincies (*cacicazgos*), haciendas, villages, and religious institutions, at least in landholding aspects. Like Gibson's *Aztecs* or a good novel, this book contains the germ of interpretations and patterns not fully articulated by the author and which will not be immediately grasped by the reader, but which will make the work productive of new insights for many years.

The Indians retained adequate valley land, indeed most of it, through the whole period; chieftains kept their identities and estates; Spanish haciendas were neither all-encompassing nor generally stable in ownership; ecclesiastical owners sold land as others did. Such is

the main and incontestable intended burden of the book.

Questions remain. Is "land tenure" really a good approach to history? "Land was the main economic foundation of the social order," we are told. It is hard to imagine that this is the same province that Brian Hamnett recently discussed predominantly in terms of its principal economic resource, the cochineal trade. The word "cochineal" appears in Taylor's study about ten times, and he himself says that cochineal was produced mainly on otherwise worthless land.

In the ongoing discussion concerning "debt peonage" Taylor does follow the trend in playing down the significance of that alleged institution, but he accepts the category, making the now hardly tenable assumption that peonage was based on debt if debt was present at all. He reduces the distinctions between temporary and permanent labor to chronological dimensions, not allowing for or describing temporary labor after the demise of the formal *repartimiento*.

When characterizing the Oaxacan situation, Taylor tends to speak of "southern" as opposed to north or central Mexico. This type of categorization will not do if we are ever to understand the evolution of Spanish American estates; we cannot go on investigating northern and southern variants in some twenty countries. We must speak in terms of general principles or tendencies and structural characteristics. Taylor knows enough to do this, but in his maddening caution refuses to make a statement about the clear causal links. Near a large capital city or a major economic asset like silver, there will be large numbers of Spaniards and consequently a market, estates will quickly consolidate, and the Indians (if any) will lose land, population, and aspects of organization. Where, as in Oaxaca, those things are lacking, estates will be slow to consolidate and Indians will retain their position longer.

JAMES LOCKHART  
University of California,  
Los Angeles

JOHN PATRICK BELL. *Crisis in Costa Rica: The 1948 Revolution*. (Latin American Monographs, number 24. Institute of Latin American Studies, University of Texas at Austin.)

Austin: University of Texas Press, for the Institute. 1971. Pp. xiii, 192. \$7.00.

This monograph is a solid contribution to Central American historiography and to the story of Latin America's place in the cold war. It analyzes the Costa Rican revolution of 1948 that brought José Figueres to hemispheric prominence as an anticommunist advocate of political change.

The author interprets this revolution as the culmination of social tensions that had been mounting in Costa Rica since 1940. The success of late nineteenth-century agriculture in Costa Rica had produced an elite class that by 1940 had become an oligarchy unable to face needed reforms. When President Rafael Calderón Guardia (1940-44) introduced such reforms to head off social violence, the elite viewed him as a radical and he became very controversial. His reputation was further tarnished when he imposed Teodoro Picado as his successor (1944-48) through electoral fraud. The result was a coalescence of diverse dissenting groups into the Opposition. José Figueres used this coalition as an instrument of revolution in 1948. He also used the nebulous Caribbean Legion and the growing hostility to communism in the United States.

Figueres, the son of a Catalan immigrant, is pictured as a loner-critic who became a revolutionary *caudillo civil* through a Machiavellian sense of purpose and timing. If there was any quixotic element in Figueres's personality and motivation, it does not appear here.

Bell's work demonstrates both the advantages and difficulties of the topical approach to history. Any of the major issues of Costa Rican politics in the 1940s (the social question, communism, corruption, conspiracy, or the electoral question) can be pursued quickly and in detail. But if one seeks a straightforward account of Costa Rican history in that decade, culminating in the revolution of 1948, he will be frustrated. For example, while the author refers early and often to the political importance of the fraudulent election of 1944, the reader remains uncertain and thus unpersuaded about it until pages 100 and 112-13. This antinarrative effect and the lack of a map or bibliographical essay or of citations to any of the author's two hundred interviews with principals, combine to create an unnecessary



air of abstraction. Citation of a wide range of sources and a good index save the day. This useful book is for specialists.

WAYNE M. CLEGERN  
Colorado State University

ALAN H. ADAMSON. *Sugar without Slaves: The Political Economy of British Guiana, 1838-1904*. (Caribbean Series, 13.) New Haven: Yale University Press. 1972. Pp. ix, 315. \$12.50.

Professor Alan H. Adamson's main task in this book is to explain how the hegemony of sugar came about and to describe how it affected Guyanese society in the nineteenth century. With the emancipation of the slaves in 1838, the survival of sugar was threatened both from within and without. Planters had to contend with a rapidly forming society of peasant villagers who could not be relied upon to supply combined and constant wage labor to the plantations. Loss of labor was compounded by a series of external threats, including Britain's move to a free-trade policy, competition from the slave-grown sugar of Cuba and Brazil, and the European beet sugar industry.

Adamson's account of the survival of sugar, the plantation economy, and the plantocracy is most convincing. The planters' economic power was buttressed by their domination of the Guyanese government and support from the West India Committee in London. Political power was used for a variety of purposes. Village settlements of ex-slaves were restricted by means of high prices placed on crown lands and heavy tax levies. After growing for a time, the villages stagnated and their inhabitants found it necessary to seek wage employment on the plantations.

Between 1835 and 1918 a total of 341,491 immigrants arrived in British Guiana, 236,205 of whom were Indian. A system of state-controlled and state-conducted immigration led to a continuous influx of laborers who were indentured for a period of approximately five years. Stringent controls governed work, wages, and freedom of movement. Immigration was financed, in part, from public revenues that came chiefly from indirect taxes. "These high indirect taxes," writes Adamson, "were in effect a subsidy to the working capital of the planters by

the nonplanting part of the community" (p. 244).

Pressure to scale down costs in relation to prices led to the transformation of the Guyanese sugar industry and water-control system. Highly mechanized plantations were linked to one another and to trading firms in vertically integrated structures having joint-stock organization and a strong metropolitan orientation. By means of wage reduction, economies of scale, and subsidies a handful of giant concerns managed to weather the crisis of the late nineteenth century. But sugar monoculture exacted high social costs, since it drove out nonsugar industries, made the colonial economy vulnerable to world market conditions, narrowed the range of trading partners, worsened the terms of trade, contributed to capital starvation, and resulted in commercial isolation from regional neighbors.

The strength of Adamson's book lies in its balanced use of literary evidence and statistical data taken from Colonial Office and Parliamentary Papers, the care with which the study is organized, and, above all, the high standards of economic analysis and historical interpretation which are consistently maintained. It is, however, somewhat disappointing to find so few references to, and no index items for, the British sugar colonies of Jamaica and Trinidad, which had many experiences in common with Guyana. Comparative study, for example, might have revealed why the peasantry of Jamaica increased by natural means, while that of Guyana failed to reproduce their numbers. These mild criticisms notwithstanding, Professor Adamson has given us an outstanding study of the transformation of a tropical export economy in a century of momentous change.

RICHARD B. SHERIDAN  
University of Kansas

J. VALERIE FIFER. *Bolivia: Land, Location, and Politics since 1825*. (Cambridge Latin American Studies 13.) New York: Cambridge University Press. 1972. Pp. xiii, 301. \$23.50.

The recognized frontiers of the Republic of Bolivia today include only about half of the area that the country claimed at the time it obtained its independence in 1825. This book is a thorough account of just how Bolivia's diminution took place.

As the author, principal lecturer in geography at Goldsmiths' College of the University of London, points out, Bolivia was based on the colonial *audiencia* or presidency of Charcas, also known as Upper Peru. This unit had been shifted from the viceroyalty of Peru to that of Buenos Aires, when the latter was established in 1776. It appeared to many, including Bolívar, at the time of independence that Charcas had little chance of maintaining a separate existence.

The Creole leaders who insisted on Bolivian independence did not listen to these fears, which in any case proved exaggerated. Although Professor Fifer notes all this, she might have indicated a bit more about why her neighbors did not partition Bolivia, a possibility that has even been seriously discussed in recent years.

Bolivia had problems on all of her frontiers. The only neighbor from which it was to gain any territory was Argentina, who offered no opposition to the decision of leaders of Tarija to shift from Argentina to Bolivia soon after independence. Yet even on this southern border, Bolivia was not able to make effective all of its other territorial claims against Argentina.

Probably most serious was the problem in the west. Bolivia emerged from the colonial period with a small wedge of territory bordering on the Pacific. This area, however, was largely desert and included no significant population center. Professor Fifer traces the efforts of the Bolivians to establish a port in the area, its attempts to convince Peru to transfer Arica (the natural outlet to the sea of Upper Peru in the colonial period), and the conflict with Chile that culminated in the War of the Pacific (1879-83) during which that country seized all Bolivian coastal territory. Bolivia's final loss of its seacoast was provoked by the discovery of rich nitrate fields there.

Rubber served the same function along the

Bolivian-Brazilian frontier as nitrates did on the Bolivian-Chilean border. After extended attempts to make its sovereignty effective over the Acre region in the Amazon area, this controversy was also resolved by armed conflict. Brazilian settlers revolted, first setting up the Republic of Acre and then successfully asking for annexation by Brazil. United States readers will find reminiscences of Texas here. In the same general area, Bolivia also had conflicts with Peru. These were, however, solved by peaceful negotiation—Bolivia losing claimed territory at the conference table rather than by force of arms.

Militarily the most serious conflict was that with Paraguay, in the east and southeast. From the time of its establishment, Bolivia tried unsuccessfully to establish an effective presence along considerable reaches of the Paraguay River and its tributaries, thus giving it access to the Rio de la Plata exit to the sea. A century after obtaining its independence, Bolivia's efforts caused a military conflict with Paraguay, in the disastrous Chaco War of 1932-35. Bolivia was ignominiously and decisively defeated. An interesting question, which Professor Fifer does not raise, is why the Chaco conflict left no spirit of "revanchism" in Bolivia. This contrasts with the aftermath of the War of the Pacific, which, as the author points out, left a still-persisting aspiration to regain a Pacific port.

Professor Fifer expertly traces all of Bolivia's frontier problems. Her prose is not scintillating, although it is adequate. The book is generously provided with maps and pictures. This volume leaves the reader well informed about Bolivia's long-standing frontier problems.

ROBERT J. ALEXANDER  
Rutgers University,  
New Brunswick

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

*The following exchange of letters has been received in connection with the publication of Hans W. Gatzke's review article, "Hitler and Psychohistory," AHR, 78 (1973): 394-401.*

### TO THE EDITOR:

Since I contributed a foreword to my brother Walter's book, *The Mind of Adolf Hitler*, I hope you will permit me to make a few observations on the lengthy analysis and criticism by Professor Hans Gatzke. In the first place it astonishes me that a professional historical journal should devote so many valuable pages to a book that is not history at all. My brother is not a historian and had no intention of writing history. His book is what it professes to be: a psychological study of a then living

person, based on admittedly fragmentary evidence and, from the very nature of the project, bound to rely in part on conjecture and clinical experience. I myself, though a historian, had almost nothing to do with the project. Neither had any of the members of the Research and Analysis Branch of the OSS, of which I was the chief. Apart from the pressures under which the entire agency was laboring during the last years of the war, it was hardly likely that a report, being secretly prepared for General Donovan in another branch of the OSS, should have been opened for general discussion. Nor is it clear to me what contribution even the ablest of the historians in the Research and Analysis Branch might have been expected to make to a psychological evaluation.

Once he became convinced that his report was not being effectively used, my brother lost interest in it. For twenty-five years he made no effort to have it declassified. When finally persuaded to have it published, he steadfastly refused to do so if revisions or updatings were to be required. I think I can say that he had only a mild interest in its publication and never anticipated the widespread interest it evoked.

One of the strangest features of Professor Gatzke's critique is his insistence that the book is not what it purports to be: an original wartime report or "historical document." So far as I can discover, he is saying that only a first draft, however hurried and crude, has authenticity, and that changes, even editorial changes made in the interest of clarity, destroy its character as a historical document. This is rather novel doctrine, considering that historical documents are continually being pub-

lished in various forms: in selection, in translation, in modernized language and style, etc. So much is certain: that the first draft of the report, which eventually found its way into the National Archives, was superseded by the revised official, printed OSS report of 1943, from which the present book is taken. Why this official document did not reach the National Archives is anyone's guess, but will hardly amaze those who have had long experience in government work. It is to be hoped that Professor Gatzke is not so enamored of first drafts that in the case of his own writings he sends them directly to the printer. I have read most if not all of his published works and would say that they reveal careful, considered editorial revisions.

By referring to many "major and minor changes" made by my brother or by the publisher in the course of preparing the report and the book for publication, Professor Gatzke leaves the reader with the impression that he (the reader) has been made the victim of a hoax: that he is not getting the true version of what my brother wrote in 1943, but a doctored account. Actually most of the changes made at any time are of a piddling nature. Nowhere has Professor Gatzke or anyone else demonstrated that important changes were made either in substance or in interpretation.

It is hard, in perusing Professor Gatzke's critique, to escape the impression that it is designed to discredit the book in one way or another. Those of us who, with some reluctance, decided to publish a document already over twenty-five years old did so in all good faith. We still regard it as an interesting and important contribution; we are gratified by the interest it has aroused; and we resent the suggestion of intentional misrepresentation and particularly the imputation that there is anything even remotely fraudulent about *The Mind of Adolf Hitler*.

WILLIAM L. LANGER  
Cambridge, Massachusetts

#### TO THE EDITOR:

I was honored to have your journal publish Hans Gatzke's lengthy review article, which was primarily an attempt to evaluate my re-

cent publication, *The Mind of Adolf Hitler*. Inasmuch as Mr. Gatzke has raised questions concerning the book's validity and integrity I would appreciate this opportunity of replying to his allegations and insinuations.

Mr. Gatzke has failed to realize that my book is not a psychohistorical study and never pretended to be. It should, therefore, not be evaluated on this basis. Any similarities that may exist are superficial and coincidental. *The Mind of Adolf Hitler* was the product of a crash psychological program designed to fill an urgent need at a critical stage of the war. A psychohistorical study to fill this need would undoubtedly have required years of research. The war could not wait. It was probably for this reason that General Donovan did not assign the task to the able historians on his staff. Instead he turned to psychoanalysts in the hope that with their training and experience they might devise techniques for producing a pragmatic profile of our adversary in a relatively short period of time. This was a novel approach. In order that the reader might not be misled into thinking that he was getting anything more than a tentative and conjectural reconstruction of Hitler's character, I devoted considerable space in the introduction to a description of the new techniques that were improvised to screen the mass of unreliable and contradictory information and the modes of evaluating its significance in terms of the total personality. It should be obvious, therefore, that our study was limited to a consideration of probabilities to which traditional historical research procedures are wholly inappropriate. Nevertheless, Mr. Gatzke as well as some other historians have failed to grasp this important distinction and attempt to evaluate the one in terms of the other. If historians are going to concern themselves with psychological studies of this type it would, in my opinion, be far more fruitful for them to confine their evaluative efforts to items such as: how well did this novel approach fill the need for which it was designed? how accurate were the basic conclusions in the light of subsequent historical investigations? can these new methods be improved and incorporated more advantageously in a psychohistorical approach to an understanding of the past?

Mr. Gatzke raises the question of the rela-

tionship between *The Mind of Adolf Hitler* and a report written by Dr. Henry A. Murray. Had he read my introduction with the same diligence he has shown in comparing a quotation taken from an American translation of *Mein Kampf* with that found in a German edition I do not believe that he would need much further clarification. I stated very clearly that one of the collaborators, after reading all of the raw material (the "Source-Book"), found that he would not be able to take part in our joint evaluative meetings to be held in New York City. Since Dr. Murray was at Harvard at that time and the other two collaborators lived in New York it would seem rather obvious who the defector was. I also pointed out "that he promised, however, to write down his views and conclusions and submit them to us for our consideration." Gatzke then completely ignores the next sentence which reads: "Unfortunately, not a word was ever received from him." Under these circumstances it should be perfectly clear that Dr. Murray did write down his views and conclusions, but instead of submitting them to us, as promised, he chose to divert them into an unrelated channel. In view of the fact that we are both psychoanalysts, working with the same directives from identical source material at exactly the same time, it should not be too surprising that the language used and the conclusions reached are not unlike each other, as Gatzke observes. Some objective investigators might even conclude from these similarities that there must be more to this psychoanalytic approach than meets the eye.

Even more disturbing are Mr. Gatzke's assertions that the book is neither "the secret psychological report written in 1943" that it purports to be, nor does it qualify as a "historical document." The pertinent facts are quite simple although subsequent events have provided room for confusion. As set forth in the introduction, pressure of time forced me to submit the first draft of my report to the OSS for consideration. It was favorably received and deemed worthy of wider distribution under OSS auspices. Early in the fall of 1943 my manuscript was turned over to the printer who promptly set up the galleys. I was asked to do the proofreading and make whatever corrections were necessary. Incredible as it

may now seem the galleys afforded me the first opportunity to read what I had written. I was not greatly impressed. There were countless omissions, transpositions, and misspellings, as well as unclear passages, irrelevant statements, repetitions, and the like. To this day I do not know whether most of these were the result of my hasty writing or the hasty typesetting of the printer. At the time it made no difference. We were in the process of preparing an official document for distribution, and since I was the author it was my duty to make every effort to state the substance of my report as clearly and concisely as possible in the allotted time. Consequently I had no hesitation in rearranging a sentence or a paragraph here and there to improve its clarity or in adding a sentence or two if some amplification seemed desirable. Likewise I had no hesitation in eliminating any material that was repetitious or irrelevant. There was nothing wrong with this procedure. Every author has the privilege of making changes in the galleys before publication. I would have made far more extensive changes had I not been cautioned to hold all changes to a minimum in order not to disrupt the tight schedule of the printer. In any event it was these corrected galleys that became the official OSS document. A very limited edition was printed, bound, classified "Secret," and distributed to a restricted clientele. It is this document, with some minor corrections that escaped us in 1943, that has been published under the title *The Mind of Adolf Hitler*.

There would be no question of the authenticity of this report as a historical document except for the fact that years later, due to ignorance or carelessness, the Strategic Services Unit, which was assigned the task of evaluating the OSS papers, made the mistake of filing my original first draft manuscript in the National Archives instead of the official OSS document that evolved from it. This is most unfortunate. The National Archives, before the publication of *The Mind of Adolf Hitler*, had sold microfilm or Xerox copies of this manuscript to interested scholars. This has led to some confusion, since the corrections made on the galleys were not made on the manuscript. When one is compared to the other, as Gatzke has done, it is obvious that he will

find that the official document differs in some respects from the manuscript. Since the book is based on the printed document and not on the first draft manuscript the same differences are to be found. Mr. Gatzke, in his review, has listed a number of these divergences, and it is on the basis of these that he would disqualify *The Mind of Adolf Hitler* not only as a "historical document" but also as a faithful reproduction of "the secret psychological report written in 1943." This, it seems to me, is a very strange view for a professional historian to hold in the face of reliable evidence to the contrary. According to this view anything filed in the National Archives, even though it is only a first draft of a subsequent document, automatically acquires a sacrosanct quality as a "historical document" irrespective of what use was made of it later on; any divergence from its text must, *ergo*, be a fraud and consequently unfit for historical consideration. In the case under discussion neither the changes made before publication in 1943 nor those made before publication in 1972 have altered the substance or modified the conclusions of the original report in any way. The report, as published, is, in my opinion, a valid reproduction of a historical document.

WALTER C. LANGER  
Sarasota, Florida

#### TO THE EDITOR:

Permit me the privilege of responding to Hans Gatzke's criticism of my work in his review article.

Let us first come to grips with the Führer's singed scrotum and its missing testicle. Mr. Gatzke says that I have misused the Soviet autopsy report in a "desire to prove [my] case." I was not trying to "prove" anything with the scrotum except monorchism. I also suggested, very tentatively, that the missing part might "conceivably" be the result of self-inflicted castration. We know that Hitler's body was burned in petrol for an indeterminate number of hours. We do not know with any assurance the exact amount of damage done to the scrotum. The Soviet pathologists who report the missing testis note that the scrotal sac was "singed." No photograph of the area was given; the Russian word for singeing or

burning is not used in the published report. If it had been *obozhena* [partially burned] or if singeing had been extensive, it occurred to me that there might possibly have been enough damage to cover up the scar of self-inflicted mutilation. Not likely, perhaps, but possible. Given Hitler's sadomasochism and his advanced psychopathology, I said it was "conceivable" that self-castration might be one way to account for the manifestly missing testicle. As I noted in my footnote, my consulting psychiatrist thought such mutilation unlikely. But with Hitler strange things were possible; I let the conjecture, as conjecture, stand. Nevertheless Gatzke is probably right: I may have overstated the amount of burning of the scrotum.

Mr. Gatzke suggests that the way a historian cites his evidence may be influenced "unconsciously" by the point he is trying to make. He may be right. Certainly he himself provides us with a persuasive illustration of his own thesis. He strains very hard to discredit Langer's interpretation of an episode in *Mein Kampf* in which an anonymous little boy experienced a primal scene trauma. Langer believes the incident to be autobiographical. Mr. Gatzke quotes from the German edition of Hitler's memoirs to argue that the passage is not autobiographical and does not depict a sexual scene. In quoting from the original, however, Professor Gatzke deletes two rather interesting phrases. In one, Hitler says that such scenes from early childhood are particularly vivid in the memory of gifted people ["bei Begabten"] —a phrase that suggests he had himself in mind. Hitler then describes crowded living conditions, not unlike those of his own family, and writes that this anonymous little boy remembers how members of the family "do not so much live with one another, rather they *press down upon each other* [*sondern drücken aufeinander*]" (*Mein Kampf*, unabridged edition [Munich, 1941], 32 [italics mine]). Not at all decisive words, but they evoke suggestive images. Dr. Langer did not quote these passages even though they would have strengthened his argument. Professor Gatzke is usually a stickler for full and precise quotation. Did he delete these particular phrases (quite unconsciously) because they did not help his case?

Professor Gatzke has said I am guilty of misusing another source when I concluded that Hitler found it important to associate himself directly and personally with the Nuremberg Racial Laws of 1935, and that he had his own grandmother's experience in mind when he personally set forth as one of the three laws that Aryan women were forbidden to work as housemaids in Jewish homes. Notice the wording in the memoir I cited written by the man who was the legal expert on "racial law" in Hitler's ministry of interior: "*Hitler selbst wollte das Gesetz (das sogenannte Blut-schutzgesetz) unterzeichnen. . . . An den strikten Befehlen Hitlers über die drei erwähnten Punkte war nichts zu ändern*" (Bernhard Löser, "Das Reichsministerium des Innern und die Jüden-gesetzgebung," *Vierteljahrshefte für Zeitgeschichte*, 9 [1961]: 273). These words conveyed to me an extraordinary degree of personal concern with these peculiar strictures. I thought it not excessive to say that Hitler took special pains to dictate the precise language of the racial laws.

With regard to my assertion that Hitler saved his blood and looked at it apprehensively, Mr. Gatzke is correct in saying that the way he looked at his own blood is not given in the source. But Gatzke's concentration on an adverb obscures a main point: according to one of Hitler's secretaries, a generally reliable witness, Hitler had the habit of having his blood drawn out; he saved it and offered to feed it to his secretaries in the form of blood sausage. This kind of testimony with its psychological implications does not, apparently, interest Mr. Gatzke. It does me. But that is another and longer story. Given Hitler's anxiety about the lineage of his father and his preoccupation with the purity of blood, it seems to me not unlikely that he looked at his own specimen "apprehensively."

Mr. Gatzke says he can supply other examples of mistakes and misinterpretations in my early essays. I have written him urging him to tell me about my errors—as I hope all readers will—so I can make corrections before my biography goes to press.

Mr. Gatzke makes a very serious charge with regard to Walter Langer's book, *The Mind of Adolf Hitler: The Secret Wartime Report*. He suggests that both Professor Wil-

liam Langer and I have endorsed a book that is not in fact the OSS report of 1943; he lists discrepancies between the book and a typescript version of the report now available in the National Archives; he writes that the subtitle of the book is "unfounded"; he takes me to task for calling the book a historical document (p. 395).

Before I undertook to write the afterword, I was assured by the publishers that this was indeed the original OSS report, with only the usual minor editorial changes such as the correct spelling of proper names and the ironing out of grammar. After publication, when I pointed out to the publishers discrepancies such as those Mr. Gatzke has noted in his article (p. 395 n. 8), I was informed that the original OSS report, "A Psychological Analysis of Adolph [sic] Hitler," appeared in 1943 in two different versions: there was a first draft in typescript to which Mr. Gatzke refers; but there was also a slightly revised, limited, and classified edition of the report printed especially for the OSS. It is this official printed version that Basic Books has published. (I have subsequently learned that the actual copy used by the publishers was Professor Langer's own personal copy of the printed report which he had received in 1943 by virtue of the fact that he was then chief of the Research and Analysis Branch of the OSS.) When one compares the present book with the printed OSS report of 1943, the discrepancies so carefully listed by Mr. Gatzke do not exist. The publishers had no knowledge of a typescript copy of the report; they believed they were publishing the only extant version. Consequently they could not inform the readers which of the two reports of 1943 they were publishing. Hence Mr. Gatzke's misconception, though deplorable, is perhaps understandable. But before writing his review could he not have telephoned his mentor of thirty years and seminar professor, William Langer, to ascertain the facts of this matter?

As noted in my afterword, Walter Langer made mistakes in his report, but as a pioneering study in the difficult but necessary business of applying psychopathology to so pathological a personality, I believe his book to be of great value. It illustrates A. J. P. Taylor's observa-

tion: "Error can often be fruitful; perfection is sterile."

ROBERT G. L. WAITE  
Williams College

TO THE EDITOR:

As an aspiring psychohistorian I should like to respond to Hans Gatzke's review article. Dr. Gatzke maintains at the outset that "with few exceptions, the preoccupation with the dead Führer has thus far not produced anything that cannot already be found in the still two best books about him"—Heiden's *Der Fuehrer* and Bullock's *Hitler: A Study in Tyranny*. Although conceding that Langer's *The Mind of Adolf Hitler* is one of the exceptions, his criticism of the book's overall approach assures one that, in Gatzke's opinion, the Langer study is anything but exceptional. Yet to be content with Bullock's political biography, which timidly characterizes Hitler the man is simply a "megalomaniac," seems to me to employ an egregious suspension of the very disbelief with which Gatzke observes the recent attempts (so to speak) on Hitler's life.

Gatzke is correct in stressing the importance of clinical experience that historians must bring to the study of any historical figure or era. Such a caveat, however, does not recognize the wealth of theoretical material and published case histories which stand at the ready in any psychohistorian's initial arsenal. Certainly Langer, as an analyst, could and did draw upon a great wealth of material even within the short time he professes to have had under OSS timetables. I fail to see, as Gatzke contends, that Langer's and the research committee's findings were "preconceived" in the same manner as Nazi historians' encomiums to their Führer. The difference, regardless of any inadequacies in the amount of historical data on such a contemporaneous figure, of course lies in the theoretical basis on which the respective treatments were anchored. It hardly needs to be pointed out that Freud's writings offer a significantly firmer ground for objective inquiry than the wishful fantasies of National Socialist propagandists.

As for Langer's interpretation itself, I think Gatzke shows an appreciable lack of under-

standing as to how a psychohistorian properly approaches his material. Although he is correct in questioning the formulistic and reductionistic invocation of the primal scene, Gatzke manifests an inability to discern the layered vicissitudes of sexual imagery, which do not have to be pictorially sexual in expression. But beyond this Gatzke insists upon an appreciation of Hitler that operates only on a conscious level: "There is nothing to indicate that the Führer, always most secretive about his early life, *intended* the passage to be autobiographical, nor does the grim picture painted agree with *what we now know about Hitler's far from dismal childhood*" (my italics). This critique would seem to be historically sound, but in fact it leaves the most important question unanswered: why did *Hitler* see things the way he did? Not Bradley Smith. Not Franz Jetzinger. Not August Kubizek. *Mein Kampf* is a personal document, reflective of the man who set it to paper, not a history and analysis of Austro-German family dynamics in the late nineteenth century. This is not to say that Langer's study is definitive; it perforce was overly reductionistic given the paucity of biographical data that is just now surfacing to help refine the image of the man throughout his entire life. But the intensity and scope of Hitler's projections in *Mein Kampf* alone are revealed when he reflects upon the examples of family life quoted by Langer and, in turn, by Gatzke: "I have seen this in hundreds of instances." Rather, he has relived his own perceived experience a fateful multitude of times.

One final observation: Gatzke claims that a perusal of the "Source-Book" included in the original report at the National Archives provides "evidence to support almost any image of the man—from repulsive, dirty, lazy, and sexually perverted psychopath to attractive, neat, hardworking, and sexually normal statesman." One would be hard pressed to describe any more succinctly the neurotic Hitler and the obsessive-compulsive character armor so symptomatic of Central European authoritarian family structure.

GEOFFREY COCKS  
University of California,  
Los Angeles



## PROFESSOR GATZKE REPLIES:

I shall try to keep my comments brief. But since most of the writers are in one way or another connected with *The Mind of Adolf Hitler*, their objections to my review deserve careful consideration.

To begin with, Professor William Langer is astonished "that a professional historical journal should devote so many valuable pages to a book that is not history at all." He has apparently forgotten that in his foreword to his brother's book he himself refers to it as "a milestone marking new and fruitful directions in historical study," and that the dust jacket proclaims it "a masterpiece of 'psycho-historical reconstruction.'" One might argue whether the quality of the book justified devoting so much space to it. But here both the editor of the *AHR* and the reviewer felt that given the publicity the book has received and the debate it has caused, it merited the attention it was given.

Professor Langer then touches on his own relations to his brother's report for the OSS, from which the book derives. I already stated (and expressed regret) in my review that neither William Langer nor his fellow historians in the OSS had anything to do with the report; it would doubtless have been improved if they had. Professor Langer's objection that historians could hardly have contributed to an evaluation of Hitler's psyche misses my point. All I said was that historians could have checked the validity of the sources on which Walter Langer's report was based. Professor Langer states further that the members of his group were too preoccupied with other matters to pay much attention to his brother's work. This may have been so. It should be noted, however, that Dr. Langer did collaborate with his brother and apparently some other historians in the OSS on at least one earlier occasion (see "Memorandum to Dr. William L. Langer from Walter C. Langer. Subject: Analysis of Hitler's Speech of April 26, 1942," Un-classified Historical OSS Records, National Archives, Washington, D.C.).

The bulk of Professor Langer's letter deals with what he calls "one of the strangest features" of my review—my claim "that the book is not what it purports to be." Here, briefly, are the facts:

When Walter Langer's book was published, I already had a copy of the original manuscript from the National Archives. Comparing the two, I noted a number of discrepancies. I wrote to the publisher asking for an explanation. It took almost five months to get the whole story straight.

It seems that the OSS, before circulating copies of Langer's report in 1943, had prepared a printed version. (This apparently was not known to the National Archives or even to some of the people connected with Langer's book.) The publishers at first claimed that the OSS, in doing so, had only made some minor copyediting changes. That claim proved false. After continued probing I discovered that not only the OSS but also Dr. Langer himself had made considerable changes, the latter adding long passages and whole paragraphs. This was the version that Basic Books decided to publish. Before doing so, however, they did their own copyediting, and, in addition, Dr. Langer made a second round of changes. These, I suppose, are the ones Professor Langer refers to as "piddling." Maybe so. Though I wonder if the change in terminology made just before publication—from "hysteric" to "neurotic psychopath"—in the basic diagnosis of Hitler's mental condition was really so unimportant.

So much for how the Langer book came about. In his introduction Walter Langer tells a quite different story. After recounting the last-minute rush to get his manuscript done in 1943, he concludes that because of that rush "the first draft automatically became the one and only draft. In this way the study was completed at the appointed hour." Dr. Langer then goes on to regret not to have been able to revise his draft since such revision and discussion with his collaborators "would have resulted in a much better product."

Here, then, are two different versions of the book's origins. In commenting on this issue in my review, I said: "Recent correspondence with the publishers has revealed that the original manuscript was changed and edited several times by Dr. Langer and others, both in 1943 and again before publication. The claim made on the dust jacket, therefore, that 'here is the secret psychological report written in 1943,' and the statement by Walter Langer in his introduction and by Robert G. L. Waite in his

afterword that the book presents a 'historical document,' are hardly justified." Nevertheless, Professor Langer accuses me of imputing intentional misrepresentation and fraud to those connected with the publication of his brother's book. I do not think the evidence warrants such an accusation.

The letter by Walter Langer does not add much to his brother's charges against my "allegations and insinuations" concerning the origins of his book. Dr. Langer's statement that he merely added "a sentence or two" before his report was printed in 1943 understandably minimizes the changes he made at that time; and the alterations made immediately before publication in 1972 are merely mentioned in passing but not explained. I agree with Dr. Langer that every author has the privilege of making such changes, but only if he did not expressly tell his readers that he had *not* done so, as Dr. Langer did in his introduction (see above).

The main interest of Walter Langer's letter, however, is in what he says about his relations with Dr. Henry A. Murray, whose analysis of Hitler was prepared at the same time as his own and in some parts bears considerable resemblance to *The Mind of Adolf Hitler*. First of all, he takes me to task for not realizing that the unnamed collaborator who, according to Langer's introduction, defected from his project was none other than Dr. Murray. How is one to know thirty years later that Dr. Murray (whose name appears nowhere in the book) "was at Harvard at that time and the other two collaborators [also unnamed] lived in New York," and that these domestic arrangements made it "rather obvious who the defector was." I see nothing obvious about all this. Having failed to guess who the anonymous culprit was, I am then berated for not realizing that the statement "unfortunately, not a word was ever received from him" refers to Dr. Murray. Instead of giving his manuscript to Langer, we are told, Murray diverted it "into an unrelated channel."

The Murray manuscript, as explained in my review, is in the Franklin D. Roosevelt Library at Hyde Park, New York. With it is a statement by Henry Field, dated October 29, 1943, explaining that "the cost of typing, stencilling

and mimeographing [of this manuscript] was paid by O.S.S." This, of course, does not necessarily mean that the OSS showed Murray's report to Dr. Langer. Still the similarity between Langer's and Murray's reports is striking. For example, Langer (*The Mind of Adolf Hitler*, p. 209) writes: "Hitler may get killed in battle. . . . This would be most undesirable from our point of view because his death would serve as an example to his followers to fight on with fanatical, death-defying determination to the bitter end . . . and insure his immortality." And Murray ("Analysis of the Personality of Adolph [*sic*] Hitler," pp. 30-31) writes: "Hitler may get himself killed leading his élite troops in battle . . . which would be very undesirable from our point of view, first because his death would serve as an example to all his followers to fight with fanatical death-defying energy to the bitter end, and second, because it would insure Hitler's immortality." Objective investigators may indeed agree with Dr. Langer "that there must be more to this psychoanalytic approach than meets the eye."

The issue of whether the published version of Walter Langer's book was the original one is also dealt with in the letter by Professor Waite. Since I have already explained my position, I need not repeat it. As for my criticism of Professor Waite's own work, I can be brief. On the specific points he mentions, which are not all the ones I raised in my review, nothing he says makes me want to change my earlier position. Just two observations: (1) The statement in the Russian autopsy report mentioned by Mr. Waite does not merely say that Hitler's genital area was "singled," but, as I already pointed out in my review, that it was "singled but preserved." Mr. Waite's disquisition, therefore, on whether "singled" meant partial or extensive burning seems somewhat labored. (2) The source cited by Professor Waite in one of his articles for the statement that Hitler's doctor drew blood from him "and preserved it in test tubes, so that Hitler could gaze at it apprehensively," actually states only that the doctor performed the bleeding operation—the test tubes and the apprehensive gazing are Waite's invention. I think, therefore, that it is Mr. Waite's "concentration on an adverb" that obscures my main point, namely,

that Mr. Waite, in citing evidence, sometimes gives rather free rein to his imagination. I might add, as Mr. Waite says in his letter, that I am preparing a list of additional mistakes or misinterpretations that I have found in his work. As he suggests, I shall be glad to hear directly from any reader interested in it.

Besides taking issue with my criticism of his own work Professor Waite charges that in my discussion of the primal scene incident I deleted some phrases because they did not help my case. Here, I think, he himself is straining a bit hard. I was aware, of course, of the passages he cites, as, I am sure, Dr. Langer was. But since the latter did not consider them worth mentioning I saw no reason to attribute to them the significance they seem to hold for Mr. Waite. All I did was take the passages quoted by Dr. Langer and retranslate them. There was no reason why I should add further passages from *Mein Kampf* which, in any event, would not have changed my interpretation of the evidence for the alleged primal scene.

The letter by Geoffrey Cocks comes close to what I had in mind when wishing, in my review, that Langer's book would lead to fruitful debate among historians and psychoanalysts. We obviously disagree on some points—I think Bullock's is a better book than he does, and he thinks I do not really understand what psychohistorians are all about. I am not sure about the latter. I realize that my evaluation of the primal scene evidence is rather literal or unimaginative. But the reason is not so much my "inability to discern the vicissitudes of sexual imagery" as my belief that the evidence for so crucial an event in an analysis of Hitler should for that very reason be scrutinized with special care. In addition to what I have said in my review, I am not certain how personal a document *Mein Kampf* really is, and I would more readily accept evidence from a source like Hitler's "Table Talk" (which, of course, was not as yet available to Langer). Should corroborating evidence for the primal scene turn up, I would be the first to welcome it as a major aid to psychohistorians in their efforts to understand Hitler.

HANS W. GATZKE  
Yale University

*The following exchange of letters has been received in connection with the publication of Stephen H. Haliczer's article, "The Castilian Urban Patriciate and the Jewish Expulsions of 1480-92," AHR, 78 (1973): 35-58.*

TO THE EDITOR:

It is good to have more sense made of the expulsion of the Jews from Spain. Stephen H. Haliczer's article, however, raised more questions for me than it answered. He established that *conversos* were influential in the decree of expulsion, yet also discussed *conversos* as managing to escape the Inquisition. If we accept, as Haliczer apparently does, that *conversos* called in by the Inquisition were Judaizers, must we then conclude that secret Jews desired the expulsion of their religious fellows? Or was it only the true New Christians who wanted to separate Jews from Spanish society? He has indicated that both sorts of *conversos* sat on town councils, but not whether both shared the *converso* attitude toward the Jews with which he is concerned. In other words, to say that *conversos* were so influential that they escaped the Inquisition, served on town councils, and influenced the decree expelling the Jews is not enough. We must know which sort, true or feigned, of *conversos* did what. Otherwise, many of his supporting arguments make little sense, for vital links are left wide open. The idea that *conversos* were active in anti-Jewish movements is not new. (For example, see Henry Kamen, *The Spanish Inquisition* [1965], 38-40.) Haliczer's connection of *conversos* to town oligarchies accords with Ruth Pike's work on Seville, most recently, and is interesting, but only as a step toward explanation. We still need more information on the frames of mind, as well as on the activities, of *conversos* in order to understand them and their place in the history of Spain and Spanish America.

PEGGY K. LISS  
Akron, Ohio

TO THE EDITOR:

A central thesis of Stephen Haliczer's article on the expulsion of the Jews from Spain is that

the prime movers behind that expulsion were the *conversos*, who applied pressure on the crown through their influence on the town councils.

The crucial steps in establishing this thesis are the following: (1) *conversos* were sufficiently influential in the town councils to frustrate certain policies of the Inquisition (pp. 43-47); (2) it was "the *converso* position" that Jews should be prevented from contact with Christians and be made to "embrace Christianity, using force if necessary" (pp. 47-49); (3) town councils are known to have segregated Jews shortly before the expulsion. Since this was *converso* policy, and since *conversos* had influence in the town councils, it follows that this policy of the councils and hence the expulsion itself resulted from *converso* pressure.

This argument is far from conclusive even if the premises are granted, but its greatest weakness lies in the assertion that expelling the Jews was, in fact, "the *converso* position." Since an expulsion of the Jews would clearly add to the number of Judaizing *conversos* and thus to increased inquisitorial pressure, the wisdom of such a *converso* policy, at least for the short term, would be dubious at best. Moreover, the author's evidence that this stand was that of the "*converso* intellectuals" consists of precisely one quotation from a *converso* priest in a book written in 1465 (pp. 48-49). Two lines after this citation, Dr. Haliczzer is already referring to this view as "the *converso* position" (p. 49). A few earlier quotations showing that *conversos* insisted on the unity of Old and New Christians do not, in themselves, constitute even the slightest evidence that New Christians wanted the Jews expelled, and even Dr. Haliczzer notes that the one quotation from Alonso de Oropesa is "crucial" to his case.

There is, furthermore, another serious methodological flaw in the article. If, in fact, the moving force behind the expulsion was *converso* pressure, contemporary Jews would obviously have known this and resented it deeply. The article, nevertheless, contains no indication that Dr. Haliczzer consulted the considerable Jewish literature of the period to determine whether or not Jews ever accused New Christians of responsibility for the expulsion. This would not be a very difficult

task, because there exists an English summary of most of the pejorative Jewish comments about the *conversos* (B. Netanyahu, *The Marranos of Spain from the Late XIVth to the Early XVIth Century According to Contemporary Hebrew Sources*, 1966). Netanyahu's thesis that most *conversos* were good Christians is itself controversial (see G. D. Cohen's review in *Jewish Social Studies*, 29 [1967]: 178-84; and Y. H. Yerushalmi, *From Spanish Court to Italian Ghetto* [1971], 21-42), but it is revealing that his exhaustive study (especially pp. 135-208) reflects no explicit charge that *conversos* brought about the expulsion of the Jews. Perhaps Dr. Haliczzer can find some explanation for this, or perhaps he will argue (implausibly, in my opinion) that such a charge is implicit in some of the stronger anti-*converso* statements that can be found (see Netanyahu, pp. 175, 180-81), but failure to consider contemporary Jewish sources at all constitutes an extremely significant oversight.

DAVID BERGER

Brooklyn College,  
City University of New York

#### TO THE EDITOR:

Stephen H. Haliczzer's excellent article traces the edict of expulsion to the alliance between the crown and the urban oligarchies of the Castilian cities. According to Professor Haliczzer, the Castilian *conversos* enjoyed a prominent social, economic, and political status in Castile. Although they achieved this status after the forced mass conversions following the anti-Jewish riots of 1391, the *conversos* remained vulnerable to the growing enmity of the Old Christian element within the urban oligarchies. To protect their newly gained status, the *conversos* adopted an anti-Semitic program. This program called for segregation of the Jews, their exclusion from economic life, and eventually their expulsion from the land.

I found Professor Haliczzer's article extremely interesting, but I believe he has neglected the existing anti-Semitic program of the urban oligarchies before 1391 and the influx of *conversos* into city government. An examination of the *Libro de las Cortes* (edited by the Real Academia de la Historia [Madrid, 1861,

1863]) will show the long-standing anti-Semitic attitude of the Castilian urban oligarchies.

Beginning as early as 1252 at the Cortes of Seville, the urban representatives protested against the power and influence of the Jews. They asked for, and the Cortes repeatedly granted throughout the next century and a half, stringent anti-Jewish legislation. The edict of expulsion of 1492 was the end product of a long historical process. The anti-Jewish program began earlier than 1391 when *conversos* were not yet incorporated into the urban oligarchies. The ordinances of the Cortes show that segregation of Jews and attempts to exclude them from the economic and political life of Castile were well under way before the mass conversions of 1391. Let us review some of these measures.

At the Cortes of Valladolid (1258), Jews were forbidden to wear white clothes or gold ornaments. They were ordered to dress in black or dark-colored clothes unless personally exempted by the king. At the *Ayuntamiento* of Jerez (1268) these controls became more strict and included an extensive list of banned feminine garments plus penalties for those disobeying the law. In 1313 the Cortes of Palencia ordered the Jews to wear a yellow circle in the front and back of their upper garments as a sign of their religion. The Cortes of Toro (1371) confirmed this regulation. Ordinances controlling the type and style of the clothes worn by Jews were not limited to the Cortes already mentioned.

As early as the Cortes of 1268 and in most that followed, measures were enacted forbidding Jews to bear Christian names and to marry or cohabit with Christians. Jews could not be used as nannies for Christian babies. Ordinances restricting Jews to specific parts of the cities were passed at the Cortes of Valladolid (1322) as well as later Cortes.

The urban procurators did not limit their petitions to the social segregation of the Jews. Their petitions, born of the economic crisis of the mid-thirteenth century, reflected the desire of the urban oligarchies to destroy the economic and political position of the Jews. At the Cortes of Palencia (1286) and most Cortes for the following fifty-three years, the *caballeros villanos* and good men claimed the

exclusive right to collect taxes while protesting against tax farming (done mainly by Jews).

Legislation limiting the interest rate of Jewish usury can be found in the ordinances of the Cortes of Valladolid (1258) and in almost every Cortes thereafter. The urban procurators also asked the king for partial or total cancellation of their debts to Jewish moneylenders. They complained against the Jews' acquisition of land and real estate under municipal jurisdiction and opposed Jewish ownership of land, as well as their exemption from certain taxes.

Politically, the *concejos* sought to exclude Jews from financial and administrative positions in royal government. Above all, the *concejos* fought to end Jewish autonomy within the cities and to bring them under their jurisdiction. The urban oligarchies demanded that Jewish affairs within the cities be dealt with by municipal officials and not by special *alcaldes* assigned to the Jewish *aljamas*.

Regardless of the anti-Semitic program of the urban oligarchies from 1252 on, the Jews counted on royal support throughout the period. Their favored position reached its peak under Pedro I (1350-69) and Enrique II (1369-79). Their growing influence continued to draw bitter protests from the urban oligarchies. At the Cortes of Toro (1371), the second proviso of the ordinances summarized in angry words one hundred and twenty years of anti-Semitic programs. The language clearly indicated that the municipalities could do without the Jews altogether. If the Jews remained in the kingdom, it was because the king wished it. But if the Jews were to remain in the realm, the urban oligarchies asked that they be socially and politically segregated and economically excluded in the manner I have indicated above.

Thus anti-Semitic sentiment appeared among the urban oligarchies long before 1492 or 1391. A program of segregation, economic exclusion, and even expulsion did not originate with *converso* intellectuals, nor did their program represent a departure from the traditional attitude of the urban oligarchies in Castile.

TEOFILO F. RUIZ  
Brooklyn College,  
City University of New York

## PROFESSOR HALICZER REPLIES:

I should like to begin my reply by thanking Professors Liss, Berger, and Ruiz for their interesting and provocative letters. The Jewish expulsion was a very complicated affair, and I certainly do not think that I have fully covered all of its many aspects in my article.

Although I agree with Professor Liss's suggestion that it would be useful to have more information on the *conversos*' "frame of mind" I feel that the real or feigned conversion of a *converso* city councilor would have played only a small part in his decision to support the anti-Semitic ordinances that led to expulsion. After all, both real and feigned *conversos* were threatened by the Inquisition and both sat on city councils that collectively viewed the existence of separate Jewish communities as an intolerable intrusion into their control of local politics. In short, the political motivations for supporting expulsion united the diverse elements on the councils and cut across the real convert, feigned convert boundary.

The two major objections raised by Professor Berger are that expulsion would have resulted in more Judaizers and increased inquisitorial pressure and that contemporary Jews would "obviously" have known it if *conversos* were the moving force behind the expulsion. To the first point I would reply that the removal of the Jews would have undoubtedly fostered the *conversos*' more rapid integration into the wider Christian community (the major goal of *converso* leaders since mid-century), and that anything that helped to accomplish this would in fact lessen Old Christian hostility and inquisitorial pressure.

With regard to the second argument I really doubt that Jews would have had any way of becoming aware of the nature of the *conversos*' anti-Semitic activities on city councils and in Hermandad juntas since the former were held in secret session and the Jews had no representatives in the latter. Under these conditions it would have been nearly impossible for Jews to know which elements on these bodies worked against them with the greatest persistency. I believe that Professor Netanyahu's book supports my general argument by demonstrating the bitter hostility of Jews and converts in the years before the expulsion.

I should like especially to thank Professor Ruiz for pointing out the many early instances of anti-Semitic feeling expressed at the level of the Cortes by the representatives of the Castilian urban oligarchy. In doing so, he supports my attempt to reorient the debate about the expulsion and look at it as a royal response to overwhelming political pressure from the periphery rather than a genuine royal policy aimed at achieving political homogeneity.

I tend to think, however, that Professor Ruiz has missed some essential points. As Américo Castro and others have pointed out, the anti-Semitic ordinances passed at the Castilian Cortes before 1391 were seldom enforced. What is really unique about the period 1480-92 is the rapid enforcement on a municipal level of anti-Semitic laws passed at the Cortes of Toledo and the passage of local ordinances that implemented earlier anti-Semitic legislation. I suggest that this change in enforcement was brought about in part because of the influx of *conversos* into the ranks of the Castilian urban elite during the fifteenth century.

STEPHEN HALICZER

*Northern Illinois University*

## TO THE EDITOR:

Being in complete agreement with Gabriel Jackson's interpretation of the late Américo Castro's recent and earlier books on the Spaniards, I am reluctant to point out a merely technical error in his review (*AHR*, 78 [1973]: 114-16). But it is incorrect to refer to Cardinal (Pedro González de) Mendoza as a *converso*. The "Grand Cardinal" of Spain was a son of the marquis of Santillana, descendant of an ancient Basque-Castilian lineage without any known Jewish antecedents. Though the famous *Tizón de la Nobleza*—a "document" of dubious authenticity, perhaps mostly fabrication, that attributes the "taint" of Jewish or Moorish ancestry to the entire Spanish higher nobility (reprinted in Julio Caro Baroja, *Los judíos en la España moderna y contemporánea* [Madrid, 1961], 3: 287-99)—includes many Mendozas, these belong to the third-following generation. Even if the cardinal's great-grand-

children and grandnephews possibly had some non-Christian admixture from eight or ten generations back, none of the aristocrats, whether "tainted" or not, was called *converso*. Strictly speaking, this term should apply only to a former Jew like the Rabbi Solomon Ha-Levi who, after his baptism, took the name of Pablo de Santa María; but eventually it came to comprise the descendants (born and raised as Christians) of such converted Jews as well, the "caste" consisting of clerics, merchants, and professionals—in short, a large part of the middle classes. Of this class was Hernando del Pulgar, a secretary of the Catholic Kings, who wrote the letter to Cardinal Mendoza cited by Professor Jackson. Incidentally, Castro did not have to demonstrate Pulgar's status as *converso* from that letter. Pulgar had already been listed as one of the neophytes by, among others, Juan de Mariana and José Amador de los Ríos.

ERIKA SPIVAKOVSKY  
Westport, Connecticut

#### TO THE EDITOR:

I find it necessary to present certain crucial aspects of my book, *Business and Politics in America from the Age of Jackson to the Civil War: A Career Biography of W. W. Corcoran*, with which Robert Sharkey had difficulty in his review (*AHR*, 78 [1973]: 163-64). Not that I am unappreciative of his efforts; the book offers an extraordinary amount of new and important material very compactly, much of it in fields in which historians have not felt at ease. Nevertheless, I believe Sharkey's review illustrates a widespread problem of operating assumptions and approaches, and especially an inability to grasp the general conclusions toward which the massive factual presentation points implicitly and explicitly.

The book's basic theme (inductively derived) is that American political and economic life in the period discussed was pervaded by the dual problems of a vast increase in the scale of power, and the multiplication of centers of power, with resulting gross political and economic instability, separately and in interaction. These conclusions emerged from a concrete and heavily documented examination of the structure and development of banking and

railroads (filling in great historiographical gaps in the process). I demonstrated some of the consequences not only on the national but also on the state level in various parts of the country.

I confess I am astonished that Sharkey barely mentioned the existence, and did not convey the substance, of this thesis, which is clearly set forth in and dominates the preface, the concluding "Summation" to part 1, and the concluding chapter of the book, as well as being suggested in transitional passages and clearly implicit throughout.

The review gave the personal "corruption" an emphasis that became excessive in proportion to the neglect of the major theme. I would not belittle individual responsibility—far from it, as I will show. The Corcorans must bear their share; but it is too simple to portray them as monsters of corruption, nor would their failings bear comparison with, say, the experience of Mexico. (Is it a symptom of enduring parochialism that there was no mention of the extraordinary chapter on the Mexican-British-American triangle?) Rather, the emphasis of the book is on the civilization of which Corcoran was a representative part, and to the exigencies of which he responded, as well as on his individual role. In shifting the primary emphasis to personal corruption, the review verges upon making him a scapegoat for the institutional structures and the drives of the society as a whole.

The personalism comes through in other ways. There seems to be, for example, a kind of escapism in the craving for information, beyond what is available and historically significant, about Corcoran "as a man"; or is it only a yearning for simplicity? But let us look at a more consequential example. Sharkey could not comprehend the chapter on railroads and politics: "One is at a loss to understand what it has to do with Corcoran." The land speculation associated Corcoran with twenty-six representatives of major factions, chief of them Stephen A. Douglas, some of whom in direct consequence became advocates of specific projects, which in turn directly contributed to the turmoil of the Kansas-Nebraska conflict in nation, state, and territory. At what point is the historian supposed to stop pursuing the direct chain of conse-

quences, in the origins of which his subject participates?

Nor is this only a philosophical problem; it bears heavily upon the writing—and the living—of history. I say with dismay that there are few historians who would have made the discoveries I made about the structure of American banking, precisely because they would not have followed the direct chain of consequences to its ultimate ramifications. Similarly the problem of the railroad origins of Kansas-Nebraska, first posed by F. H. Hodder six decades ago, would still remain mysterious if some other biographer, forgetting his role as historian, had ceased his quest at the point at which Corcoran became “only” one of many, rather than personally a major causal figure. (It may be apropos to note that the basic sources for these discoveries have been available to historians at least since the 1920s.)

The perceptive reader will note that I explicitly studied the consequences of Corcoran’s acts, not only when he was one of the “big” participants, but also when he was a representative participant. To neglect his significance as a representative participant would have meant writing history according to a version of the “great man” formula; it would have meant sacrificing history to biography, instead of achieving some synthesis of the two modes of inquiry.

HENRY COHEN  
*Loyola University,  
Chicago*

#### TO THE EDITOR:

In his review of *The Foundations of the Modern World* (*AHR*, 77 [1973]: 1406–07) by Louis Gottschalk and others, Franklin L. Ford quotes Professor Roland Mousnier’s printed criticism of that volume as if it were only “an encyclopedic work of random reference.” From among the comments of Professor Mousnier and his colleagues only those were printed that seemed to the Secretariat of the International Commission as relevant to substantial weaknesses of the manuscript. Hence the following final paragraph of Professor Mousnier’s remarks was omitted: “But, after having made our observations and expressed our reservations in all sincerity, as the International

Commission had asked us to do, it is with the same sincerity, taking into account the difficulty of the task confronted by M. Louis Gottschalk and his collaborators and fully aware of the diversity of the possible solutions, each with its advantages and disadvantages, that I thank and congratulate, in my name and in the name of my collaborators, the authors of this book for their objectivity, their idealism, their love of humanity, and for their rich harvest of facts which are going to enlighten all men in their striving toward the happiness and the perfection of the human species.”

My aim in citing Professor Mousnier’s comment is that his more complete critique may be known.

LOUIS GOTTSCHALK  
*University of Chicago*

#### TO THE EDITOR:

Permit me to add a few remarks to the discussion of Gerhard Masur’s *Imperial Berlin* (*AHR*, 78 [1973]: 191–92). Critic and author have spoken. Here is what a historian who himself has grown up in “Imperial Berlin,” prior to 1914, jotted down when he first read the book: this book has an all-pervading “theme”—the coexistence of the Prussian tradition that had been enlarged to become “Imperial,” and of the “World-City,” industrial, exuberant, and critical. It is the great achievement of the book that the “theme” is never presented as a thesis, but rather in the form of concrete description. Only by reading this book have I become fully aware of the double character of the city of my youth. Perhaps this reaction may be of interest to your readers.

DIETRICH GERHARD  
*Washington University*

#### TO THE EDITOR:

I regret the printer’s error that represents J. H. Hexter’s characterization of the historian’s mode of discourse as “possessive” in my review of his *The History Primer* and *Doing History* in the April 1973 *AHR* (pp. 402–03). The word I quoted in my manuscript was “processive.”

W. WARREN WAGAR  
*State University of New York,  
Binghamton*



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## Recent Deaths

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HARRY J. BENDA, professor of history and associate chairman of Southeast Asia Studies at Yale died on October 26, 1971; two days later he would have been fifty-two.

A heart attack terminated the life of the nation's leading expert on Indonesian and Southeast Asian history and ended a career that had only begun in the 1950s. Benda was born a few months after the Versailles peace treaty in newly created Czechoslovakia (Liberec) as a son of Jewish parents. In 1938, under the threat of Nazi occupation, he left his country and found asylum in the Dutch East Indies where he entered a promising business career. In 1943 the Japanese occupation army (following a directive of their Axis partner in Berlin?) imprisoned Jewish emigrants in areas under their control, and it was in the internment camp that Benda decided to pursue an academic career. Conversations with fellow prisoners, in particular with the eminent Dutch scholar W. F. Wertheim, led him to reflect on what he was witnessing—the impact of the Japanese in a colonial setting.

When, after a time of intense studies in New Zealand (1946–52), Benda was accepted in the graduate school of Cornell University, he wrote his dissertation on the emergence of Indonesian Islam in the war period as a political potential. He thoroughly analyzed Dutch policy vis-à-vis Islam in the colonial period and compared it with the different approach of the Japanese, who needed a reliable ally in their fight against the West. By their enforcement of a united front of the various Islamic organizations, which the Dutch had always tried to prevent, the Japanese unleashed new dynamics that were deeply to influence postwar developments in Indonesia.

This work, published under the title *The Crescent and the Rising Sun* (1958), earned him, after a first appointment at the University of Rochester (1955–59), an associate professorship in the department of history at Yale (1959); his book is still a classic in the field of Indonesian studies.

Benda's way to Yale, where he became professor of history in 1966, thus contrasts sharply with that of his "predecessor" Clive Day (1871–1951), perhaps the first American scholar ever to publish a scholarly work on Indonesia (*The Policy and Administration of the Dutch in Java*, 1904). Day was as wholly a Yale man as one well could be: being a descendant of a former president of Yale, he himself received his B.A. (1892) and his Ph.D. (1899) from Yale and was on its faculty in the department of history from 1898 to 1936, when he retired. Although Day did not continue his studies on Indonesia (his interest lay with the history of commerce), he left to his "successor" an invaluable collection of material on colonial Indonesia, which was gratefully used by the latter and the steadily growing number of his students.

Benda's next publication, together with Ruth McVey, was a translation and annotation of Dutch documents, dealing with *The Communist Uprisings of 1926–1927 in Indonesia* (1960). Here it becomes quite apparent, what was already discernible in his dissertation, that his scholarly concern was with the colonial period rather than with contemporary events. He was fascinated with the phenomenon of "indigenous response" to "Western input" and he soon went beyond Indonesia and examined Southeast Asian history with this question in mind. For some time he was struggling

with the role of elites in transitional societies, and he wrote stimulating articles on their function in the Western and the non-Western world that are now republished in a nice memorial volume (*Continuity and Change in Southeast Asia. Collected Journal Articles of H. J. Benda*, 1972). He soon discovered, however, that elitism would not provide the answer for his questions, in particular not the modern Westernized intelligentsia. He repeatedly remarked "that Western impact had not necessarily affected the fundamental character of Southeast Asia itself." So he paid increasing attention to prevailing pre-Western values and orientations, to the world of the peasants and their beliefs, to the "Indianized," "Sinicized," and "Islamized" traditions in their local context. These themes were colorfully depicted in his selected readings *The World of Southeast Asia* (coauthored with J. A. Larkin, 1967) and perceptively analyzed in *A History of Modern Southeast Asia* (coauthored with J. Bastin, 1968), his last monograph, and in various articles now easily accessible in the above mentioned volume.

No wonder that the man who was pleading for a better understanding of the peoples of Southeast Asia in scholarly approaches was a severe critic of the American involvement in that part of the world and that he was bitter when he saw how little his advice (he lectured regularly at the Foreign Service Institute in Washington) was heard. In his personal relations Benda was the most amiable man. He was a loving husband to Eva, who had survived the terror of Auschwitz, and an affectionate father to Peter and Susan. The number of his personal friends was seemingly unlimited—at national and international conferences he was always a center of attraction. A highlight in his short but intense and fruitful career was the invitation by the government in Singapore to found and be the first director of the local Institute of Southeast Asian Studies (1968–69), which aptly reflects the international recognition of Benda and his work.

BERNHARD DAHM  
University of Kiel  
Yale University

JOHN DANIEL BRIGHT, professor emeritus of history at Washburn University, died February

9, 1973. He was seventy-five years old. Dr. Bright received his A.B. from Manchester College and his M.A. and Ph.D. from the University of Wisconsin. From 1926 to 1939 he was chairman of the department of history at McPherson College. Joining the faculty of Washburn University in 1939, he served as a professor of history and chairman of the department of American citizenship until 1962. Dr. Bright was a life member of the AHA.

RUSSELL C. EWING, professor of history at the University of Arizona and head of the department from 1958 to 1969, died suddenly at his home in Tucson on October 14, 1972, at the age of sixty-six.

Born in Manhattan, Kansas, he moved with his family to San Francisco at the age of ten. He received his B.A. degree in political science in 1929 from the University of California, Berkeley, where, as a track star and All-American halfback, he excelled in athletics as well as scholarship. Entering graduate school in the fall, he soon came under the stimulating guidance of Herbert Eugene Bolton with whom he took his M.A. in 1931 and his Ph.D. in 1934. Academic positions were scarce indeed in the midst of the depression, so like many others he entered government service, accepting a position as regional historian for the National Park Service. Two years later an acceptable opportunity arose to return to his vocation, and he joined the faculty of the University of Arizona. There he found a congenial academic home as a teacher and for the pursuit of his major research interests, the history of Mexico and the Spanish borderlands.

In the years following, Ewing published a number of articles and coauthored or edited several volumes: *Greater America* (1945), *The United States and Latin America* (1960), *Arizona, Its People and Resources* (1960), and *Six Faces of Mexico* (1966). At the time of his death he was writing a history of Mexico and had completed research on a guide to the history of the Spanish borderlands.

He assumed the leadership of the department at a difficult moment in its growth and development. Early in his tenure he guided the separation of political science into a department of government and undertook the inauguration of a doctoral program in history. Throughout

his decade in office, rapidly expanding enrollments necessitated almost constant faculty recruitment and curriculum revision. These challenges he met with efficiency and equanimity.

During his long tenure at Arizona, Ewing's attention to teaching and research was diverted and sometimes refreshed by several memorable episodes. In the summer of 1940 he joined Professor Bolton and several other scholars in retracing much of the 2,500 miles covered by Francisco Vázquez Coronado on his famous expedition in 1540, an accomplishment in which he took a good deal of pride and pleasure. The outbreak of World War II brought experience of a different sort. He volunteered for the United States Navy and was assigned as an instructor in the Naval Officers School at the university. He left the service in 1946 as a lieutenant commander. In 1956-57 Ewing taught as a Mundt-Smith grantee at the University of the Andes, Bogota, Colombia, where he had the unique opportunity to witness the revolution that ended the Rojas Pinilla dictatorship in that country.

Professor Ewing's services to his students, his profession, the university, and the community of Tucson, numerous and time consuming, reflected the high ideals of personal dedication he brought to every task as well as the trust placed in his integrity and judgment by everyone who knew him. All of his endeavors were marked by a compassionate concern for the well-being of others and by a constant effort to judge fairly his fellow men past and present. These fine qualities were well concealed behind a modest mien that endeared him to both his colleagues and his students.

ROBERT PAUL BROWDER  
University of Arizona

HERBERT HEATON was a Yorkshireman. Born on June 6, 1890, at Silsden, Yorkshire, of Fred and Eva Heaton, he was educated at Batley Grammar School and then at Morley Secondary School. A scholarship took him to Leeds University where he won first class history honors in 1911; in the same year he was Rutson Research Scholar at that institution. He also studied at the London School of Economics.

From 1912 to 1914 Herbert Heaton was an assistant lecturer at the University of Birmingham. Then the threat of weak lungs took him

to the Antipodes by way of Suez, India, and Singapore. For the next three years he was lecturer in history and economics at the University of Tasmania. He then moved to the mainland and was lecturer in economics and director of tutorial classes at the University of Adelaide until 1925.

As part of the eastern movement in the British Empire Herbert Heaton journeyed from Australia to Queen's University at Kingston, Canada, where he was Sir John A. Macdonald Professor of Economics and Political Science. In 1927 he joined the history department at the University of Minnesota. There his wit and his wide-ranging knowledge of man's attempts to make a living from ancient days to the present became proverbial. He continued at the University of Minnesota until his retirement, after four years as chairman, in 1958.

Many honors came his way. He received a Master of Arts degree from Leeds University, a Master of Commerce from Birmingham University, and a Doctor of Literature from Leeds. In 1924 he was special lecturer on Australian problems in London, Cambridge, Toronto, and Edmonton. He was a Guggenheim fellow in 1931-32, and in later years he was a visiting professor at Princeton, Johns Hopkins, and the Universities of Utah, British Columbia, Texas, California, Michigan State, and Pennsylvania State. He was the secretary of the Social Science Research Council's committee on research in economic history. From 1948 to 1950 he served as president of the Economic History Association.

Heaton's first publication was *Welfare Work* in 1919. But better known and an earnest of the future was his *Yorkshire Woollen and Worsted Industries* that saw print in 1920 and was reprinted in 1965. His *Modern Economic History* was published in 1925 at Adelaide. Three years later his classic *History of Trade and Commerce* appeared. In 1934 during the Great Depression he wrote the *British Way to Recovery*, in the war years he contributed to *The Interpretation of History*, he published *Yorkshire Cloth Traders in the United States* in 1944, and in the postwar years he wrote *Socialism in Western Europe*. In 1952 he published *A Scholar in Action*, a tribute to Edwin F. Gay. His greatest work was his *Economic History of Europe*, first published in 1936 and

reprinted in 1948. To this original work he brought new insights, new vistas, new horizons, and a new point of view—all enriched by a questing mind and distinguished by a most gracious literary style. It places him among the great scholars of our generation and will be a lasting memorial.

Herbert Heaton was a good companion. Time spent with him went all too swiftly. One went away stimulated, full of new ideas, new viewpoints, and new challenges. He was exciting, vibrant, serious, and then humorous as the fun of it all struck him. It was good to be with a mind whose boundaries were only those imposed by the limitations of this blue planet and the appearance on it of mankind.

RODNEY C. LOEHR  
*University of Minnesota*

H. DONALDSON JORDAN, professor emeritus of English history at Clark University, died in Worcester, Massachusetts, on December 20, 1972, after a short illness. Besides his membership in the faculty he had served as secretary of the graduate board, chairman of the department of history, government, and international relations, and chairman of the college board, actually dean of the college, a title that characteristically he refused to accept until his last year in office.

Born in Chicago in 1897, he attended schools in that city, Freiburg, Germany, and Geneva, Switzerland. He earned three degrees at Harvard University where he became a teaching fellow and received the Tappan prize for his doctoral dissertation. He became an assistant professor of history at Dartmouth College in 1925, and, after a year abroad as a Guggenheim fellow, accepted an appointment at Clark University in 1931, retiring in 1967.

Dedicated to high standards of scholarship, service, and personal integrity, Professor Jordan practiced long hours, hard work, and self-discipline that, together with a reputation for wise counseling and prudent reflection, earned him an undue amount of committee work and administration. Nevertheless, he never neglected his role as teacher, but rather pursued beyond the call of duty the personal relations with students that a small university made possible. He inspired them with reverence for precision in detail and a shrewd and careful caution in large judgments. Their appreciation, enhanced

rather than diminished by his judicious exercise of the office of dean, won him the title of "Man of the Year" in 1959. The esteem of his colleagues was expressed in his election to Phi Beta Kappa in 1963. Upon his retirement the trustees conferred upon him the degree of L.H.D., a tribute to his years of devotion to the university in a spirit of selfless service rather than that of power hunger or officiousness.

In his busy life of teacher-administrator he coauthored with E. J. Pratt *Europe and the American Civil War* (1931) and contributed to scholarly periodicals, the *Encyclopedia of the Social Sciences*, the *Dictionary of American Biography*, and the *Century Cyclopedia of Names*. At the time of his death he had nearly completed a book on Richard Cobden. A life member of the American Historical Association, he was especially active in the Conference on British Studies.

In addition to his academic life Professor Jordan served in the two world wars. In the first he was an ambulance driver in France, for which he was decorated with the Croix de Guerre, and later was a second lieutenant in the United States infantry. In the second he was an analyst and administrator in the Office of Strategic Services and for a short time served in the Department of State.

His colleagues will remember him not only for his academic leadership and for his broadly based humanism, but also for his warm companionship in both work and play. Despite crippling arthritis he took his part in faculty sports without ever complaining of the pain he often suffered. A teacher-scholar in the best tradition, he maintained a lively interest in the history department after his retirement, continuing to give the benefit of his wisdom to the younger faculty members who often sought him out. Colleagues, former students, and friends are expressing their love and admiration for him by sending a memorial contribution to the H. Donaldson Jordan Memorial Fund, Department of History, Clark University, Worcester, Massachusetts 01610.

DWIGHT E. LEE  
*Clark University*

SHELBY T. McCLOY, University of Kentucky emeritus professor of French history, died at the age of seventy-five in Monticello, Arkansas, on February 15, 1973. A graduate of Davidson Col-

lege, where he received the B.A. in 1918 and the M.A. in 1919, he went on to Oxford University as a Rhodes scholar, taking a B.Litt. and a second B.A. In 1925 he was on the teaching staff at Robert College in Istanbul, an experience to which he often looked back with great fondness. His realization that he was "insufficiently tempered" led him to Columbia University as a Jacob Schiff fellow and to a Ph.D. in 1933. There followed a decade on the faculty of Duke University, and after 1944, until his retirement in 1966, he became a mainstay in the history department at Kentucky.

Professor McCloy was a dignified and quiet man, known for his courteous, almost courtly, manner—a manner that was for him a way of life. His even temper gave way to high dudgeon when he was faced with the slipshod, the unethical, and the unjust. His intellectual cutting edge remained finely honed, even in the last years of his active career when he was forced to battle against a progressive and eventually fatal disease, which he chose to ignore in preference to "more interesting things." He resented most his sudden incapacity to work abroad in what had become his second home, Paris. While he was unable to identify easily the best Parisian restaurants, no one knew the archives more intimately.

He gave himself unstintingly to the University of Kentucky, where he helped to establish the honors program, directed the Rhodes Screening Committee, advised countless numbers of students, directed Ph.D. dissertations, lent a willing ear to the problems of his colleagues, participated in community activities, and with all this, wrote and published extensively. His research interests, centering in seventeenth- and eighteenth-century France, produced fifteen scholarly articles and six books, including *French Inventions of the Eighteenth Century* (1952) and *The Humanitarian Movement in Eighteenth Century France* (1957). In the 1950s, well ahead of present interest in the history of minority groups, he began research on Negro history and in 1961 produced *The Negro in France*. This study then led him to what would be his last book, *The Negro in the French West Indies*, published in 1966.

Professor McCloy's increasing illness forced him into what he and his department viewed as a premature, unwanted, but unavoidable retirement. He took with him the esteem of

the history staff, which had previously recognized his teaching, research, and outstanding service to the university by voting him Hallam Professor of History in 1959–60, and the respect of hundreds of faculty members who in 1960–61 elected him Distinguished Professor of the College of Arts and Sciences, the highest honor they could bestow. The death of this scholar and great gentleman comes hard.

GERARD E. SILBERSTEIN  
*University of Kentucky*

EARLE DUDLEY ROSS, professor emeritus of history at Iowa State University, died in Ames, Iowa, on March 22, 1973. Dr. Ross was born December 20, 1885, in Ross Hill, Tioga County, New York. A graduate of Syracuse University (1909), he received the doctorate in history at Cornell University in 1915. He taught at Missouri Wesleyan College (1915–17), Simpson College (1917–18), Illinois Wesleyan College (1918–19), and North Dakota Agricultural College (1919–23). Professor Ross joined the faculty of Iowa State College as an associate professor of economic history in 1923, and he retired from active teaching in 1956.

Professor Ross's list of publications is impressive especially because of the pioneering nature of several of his works—studies that are still in high repute among professional historians. The two most significant are *The Liberal Republican Movement*, published in 1919, and *Democracy's College: The Land-Grant Movement in the Formative Stage*, which appeared in 1942. The official historian of Iowa State University, he published *A History of Iowa State College* in 1942, and on the occasion of its centennial in 1958 he contributed *The Land-Grant Idea at Iowa State College*. In addition to these and several other books, he produced forty-four scholarly articles and many book reviews.

What this chronicle of bare facts cannot convey is the great esteem and warm regard that Professor Ross earned from faculty and students as a colleague, as a teacher, and as a man. He was a scholar-teacher in the best sense of the term. His colleagues revered him for his good sense and good temper, his sound judgment, and his personal integrity. His students responded to the example he set and to his kindly yet firm stimulation and encouragement. In the words of the citation read on the oc-

casion of his being awarded the honorary degree of Doctor of Humane Letters at Grinnell College in 1952, Earle D. Ross was a "distinguished member of the faculty of a sister institution, discriminating observer of the American scene; a recorder in brilliant fashion of the economic history of the middle west; and inspiring teacher who has added dignity and luster to the teaching profession."

LOUIS G. GEIGER

*Iowa State University*

Other members of the association who have died recently include: S. S. Biro of Leucadia, California; Frank J. Cappelluti of Belleville, New Jersey; Robert E. Cleveland of Loma Linda University in Loma Linda, California; C. Doris Hellman of Queens College in New York, New York; Douglass H. McNeally of Portland, Maine; R. S. Penfield of Hartford, Connecticut; J. B. Warden of Pinehurst, North Carolina; and Frank P. Weberg of Joliet, Illinois.

## Other Books Received

Books listed were received by the *AHR* between February 1, and May 1, 1973. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

- ADAMS, HENRY H. *Years of Expectation: Guadalcanal to Normandy*. New York: David McKay. 1973. Pp. xiii, 430. \$9.95.
- ADLER, CHAIM, et al. *World Politics and the Jewish Condition: Essays Prepared for a Task Force on the World of the 1970s of the American Jewish Committee*. Ed., with an introd., by LOUIS HENKIN. [New York:] Quadrangle Books, in collaboration with the Institute of Human Relations Press. 1972. Pp. x, 342. \$9.95.
- ALBERIGO, GIUSEPPE, et al. *Legge e Vangelo: Discussione su una legge fondamentale per la Chiesa. Testi e ricerche di Scienze religiose*, 8. Brescia: Paideia Editrice. 1972. Pp. 712. L. 7,000.
- ALBERIGO, JOSEPHO, et al. (eds.). *Conciliorum Oecumenicorum Decreta*. 3d ed.; Bologna: Istituto per le scienze religiose. 1973. Pp. xxiv, 1135, 169.
- ARAI, TSUGUO (ed.). *Alaska and Japan: Perspectives of Past and Present*. [Anchorage:] Alaska Methodist University Press. 1972. Pp. 172.
- BÄNTON, ROLAND H., and GRITSCH, ERIC W. *Bibliography of the Continental Reformation: Materials Available in English*. 2d rev. ed.; [Hamden, Conn.]: Archon Books. 1972. Pp. xix, 220. \$10.00.
- BERGER, PETER L., et al. *The Homeless Mind: Modernization and Consciousness*. New York: Random House. 1973. Pp. xii, 258. \$6.95.
- Bibliographie internationale de l'Humanisme et de la Renaissance*. Vol. 6, *Travaux parus en 1970*. Fédération internationale des Sociétés et Instituts pour l'étude de la Renaissance. Ouvrage publié sur la recommandation du Conseil International de la Philosophie et des Sciences Humaines, avec le concours du C.N.R.S. et de l'U.N.E.S.C.O. Geneva: Librairie Droz. 1973. Pp. xii, 678.
- BLACKBURN, ROBIN (ed.). *Ideology in Social Science: Readings in Critical Social Theory*. New York: Pantheon Books. 1972. Pp. 382. Cloth \$8.95, paper \$2.95.
- BLOCH, MARG. *The Royal Touch: Sacred Monarchy and Scrofula in England and France*. Tr. by J. E. ANDERSON. Montreal: McGill-Queen's University Press. 1973. Pp. xi, 441. \$18.95. See rev. of French ed. (1924), *AHR*, 30 (1924-25): 584.
- BLOOMFIELD, MASSE, and WOLF, HARVEY J. *Man in Transition: A Concept of History*. Reseda, Calif.: Mojave Books. 1973. Pp. vii, 87. \$8.00.
- BRODIE, BERNARD. *War and Politics*. New York: Macmillan. 1973. Pp. xii, 514. \$8.95.
- BRODIE, BERNARD and FAWN M. *From Crossbow to H-Bomb*. Rev. ed.; Bloomington: Indiana University Press. 1973. Pp. 320. Cloth \$7.95, paper \$2.95.
- BROWN, W. NORMAN. *The United States and India, Pakistan, Bangladesh*. The American Foreign Policy Library. 3d ed.; Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 462. \$16.00. See rev. of 1st ed. (1953), *AHR*, 59 (1953-54): 175.
- BUTTS, R. FREEMAN. *The Education of the West: A Formative Chapter in the History of Civilization*. New York: McGraw-Hill. 1973. Pp. xvi, 631. \$10.95.
- CARTER, GWENDOLEN M., and HERZ, JOHN H. *Government and Politics in the Twentieth Century*. 3d ed.; New York: Praeger. 1973. Pp. xiii, 278. \$9.00.
- CONNELLY, OWEN. *The Epoch of Napoleon*. Berkshire Studies in History. New York: Holt, Rinehart and Winston. 1972. Pp. viii, 198.
- CONOVER, PAUL H. (ed.). *Asia and Africa: Introductory Studies of Non-Western Societies*. Columbus, Ohio: Charles E. Merrill. 1973. Pp. vi, 249. \$4.95.
- COOPER, JOHN MILTON, JR. (ed. with an introd.). *Causes and Consequences of World War I*. New York: Quadrangle Books. 1972. Pp. 360.
- CROSBY, EVERETT UBERTO, and WEBB, CHARLES R., JR. (eds.). *The Past as Prologue: Sources and Studies in European Civilization*. In 2 vols. New York: Appleton-Century-Crofts. 1973. Pp. x, 498; x, 604.
- DUDLEY, GUILFORD A., et al. *A History of World Civilizations*. Ed. by EDWARD R. TANNENBAUM. New York: John Wiley and Sons. 1973. Pp. xv, 925. \$13.95.
- EYCK, FRANK (ed.). *The Revolutions of 1848-49*. New York: Barnes and Noble. 1972. Pp. viii, 202. Cloth \$10.50, paper \$5.50.

- FLOUDD, RODERICK. *An Introduction to Quantitative Methods for Historians*. Princeton: Princeton University Press. 1973. Pp. ix, 220. \$7.50.
- GERBI, ANTONELLO. *The Dispute of the New World: The History of a Polemic, 1750-1900*. Tr. by JEREMY MOYLE. Rev. ed.; [Pittsburgh:] University of Pittsburgh Press. 1973. Pp. xviii, 700. \$19.95. See rev. of Italian ed. (1955), *AHR*, 62 (1956-57): 602.
- GILLISPIE, CHARLES COULSTON (ed. in chief). *Dictionary of Scientific Biography*. Vol. 6, *Jean Hachette-Joseph Hyrtl*. Pub. under the auspices of the American Council of Learned Societies. New York: Charles Scribner's Sons. 1972. Pp. xiii, 619. \$35.00. See rev. of vols. 1 and 2 (1970), *AHR*, 78 (1973): 64.
- GIURESCU, CONSTANTIN C. *On Romanian-American Cultural Relations*. New York: Romanian Library. [1972.] Pp. 20.
- HACHEY, THOMAS E. (ed.). *Anglo-Vatican Relations, 1914-1939: Confidential Annual Reports of the British Ministers to the Holy See. Her Majesty's Stationery Office, London, England*. Boston: G. K. Hall. 1972. Pp. xli, 403. \$18.00.
- HEATER, D. B. *Political Ideas in the Modern World*. 4th ed. rev.; New York: Barnes and Noble. 1972. Pp. 230. \$2.75.
- HERMANN, CHARLES F. (ed.). *International Crises: Insights from Behavioral Research*. New York: Free Press. 1972. Pp. x, 334. \$12.95.
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#### A GUIDE TO THE ABBREVIATION OF JOURNAL TITLES

GENERAL RULES: Proper names are spelled out with the exception of adjective forms and names of countries used to identify place of publication. Whenever possible, articles, conjunctions, and prepositions are deleted. Exceptions are relatively short titles such as *Past & Present* or *The Americas*.

<i>Abt</i>	Abteilung	<i>archaeol</i>	archaeolog, archaeology
<i>acad</i>	academia, academy	<i>archäol</i>	Archäologie, archäologische, archäologischer
<i>accad</i>	accademia	<i>archeol</i>	archeologia, archeologie, archeology
<i>adm</i>	administration, administrative	<i>Ariz</i>	Arizona
<i>aff</i>	affairs	<i>Ark</i>	Arkansas
<i>afric</i>	african, African	<i>ark</i>	arkiv
<i>afrik</i>	afrikaanse	<i>arq</i>	arquivos
<i>agric</i>	agricultural, agriculture	<i>arqueol</i>	arqueológico
<i>ÄHR</i>	<i>American Historical Review</i>	<i>art</i>	article
<i>aikakausk</i>	aikakauskirja	<i>assoc</i>	association
<i>Akad</i>	Akademie	<i>assyriol</i>	assyriological, assyriologie, assyriology
<i>Ala</i>	Alabama	<i>at</i>	atti
<i>Alas</i>	Alaska	<i>Atl</i>	Atlantic
<i>alemann</i>	alemannisches	<i>AUMLA</i>	<i>Journal of the Australasian Universities Language and Literature Association</i>
<i>allg</i>	allgemein	<i>autobiog</i>	autobiography
<i>Alttertumsk</i>	Alttertumskunde		
<i>alttest</i>	alttestamentliche		
<i>Am</i>	American, Americana, Amerikas	<i>b</i>	buch (compounds only)
<i>an</i>	anales, Annalen, annales, annali, annals, annua, annuaires, annual, annuarium	<i>balt</i>	baltisch
<i>anc</i>	ancien, ancient	<i>bayer</i>	bayerisch
<i>annot</i>	annotation	<i>Beitr</i>	Beitrag, Beiträge
<i>anthol</i>	anthologica, anthology	<i>Ber</i>	Bericht
<i>anthropol</i>	anthropologie, anthropology	<i>bibl</i>	bibliotek, bibliotheca, bibliothèque
<i>antiq</i>	antiquarian, antiquarisch, antiquarischen, antiquité, antiquities, antiquity	<i>bibliogr</i>	bibliografice, bibliographical, bibliography
<i>antol</i>	antología	<i>bijd</i>	bijdragen
<i>antropol</i>	antropologiczny	<i>biog</i>	biography
<i>anz</i>	anzeiger	<i>Bl</i>	Blatt, Blätter
<i>appenzell</i>	appenzellische	<i>BMGN</i>	<i>Bijdragen en Mededelingen betreffende de Geschiedenis der Nederlanden</i>
<i>arch</i>	archiven, archives, archivio, archivio, archiv, Archivum	<i>bol</i>	boletim, boletín
		<i>boll</i>	bollettino

<i>brandenburg</i>	brandenburgisch	<i>Eur</i>	Europas, Europe, European, européennes
<i>bras</i>	brasileira	<i>ev</i>	evangelisch
<i>braunschw</i>	braunschweigisch	<i>explor</i>	explorations
<i>Braz</i>	Brazilian		
<i>brem</i>	bremisches	<i>fac</i>	faculté, faculty
<i>Brit</i>	British	<i>facs</i>	facsimile
<i>bull</i>	bulletin	<i>Fak</i>	Fakulté
<i>bus</i>	business	<i>fil</i>	filosofia, filozofski, filozofskog
<i>byz</i>	byzantine	<i>filol</i>	filología
		<i>Fla</i>	Florida
<i>cah</i>	cahiers	<i>for</i>	foreign
<i>Calif</i>	California	<i>Forsch</i>	Forschung, Forschungen
<i>Can</i>	Canadian	<i>fr</i>	français, French
<i>Carib</i>	Caribbean	<i>francisc</i>	franciscanum
<i>cath</i>	catholic	<i>fränk</i>	fränkische
<i>cent</i>	century	<i>frankf</i>	frankfurter
<i>cercet</i>	cercetări		
<i>českoslov</i>	československy	<i>g</i>	giornale
<i>chron</i>	chronicles, chronique	<i>Ga</i>	Georgia
<i>circ</i>	circle, circular	<i>gaz</i>	gazette
<i>civil</i>	civilization	<i>gen</i>	general, général
<i>class</i>	classica, classical, classique	<i>geneal</i>	genealogy
<i>co</i>	county	<i>geog</i>	geográfico, geographic, geographical, géographique, geographischen, geography
<i>coll</i>	college		
<i>collect</i>	collection, collections	<i>Ger</i>	German
<i>Colo</i>	Colorado	<i>germ</i>	germanistisch
<i>com</i>	comité, committee	<i>Ges</i>	Gesellschaft
<i>comm</i>	commerce	<i>gesch</i>	Geschichte, geschichtliche
<i>comp</i>	compare, comparative	<i>gos</i>	gospodarczych
<i>compil</i>	compilation, compiled, compiler	<i>gout</i>	government
<i>concl</i>	conclusion	<i>grad</i>	graduate
<i>conf</i>	conference	<i>Grafsch</i>	Grafschaft
<i>cong</i>	congress		
<i>Conn</i>	Connecticut	<i>h</i>	hefte (compounds only)
<i>contemp</i>	contemporaine, contemporánea, contemporary	<i>hamburg</i>	hamburgisch
<i>corp</i>	corporation	<i>hann</i>	hannoversche
<i>corr</i>	correspondence	<i>hell</i>	hellenic, hellénique, hellenistic
<i>c. r.</i>	comptes rendus	<i>helvet</i>	helvetian
<i>crit</i>	critica, criticism	<i>hess</i>	hessisch
<i>cuad</i>	cuaderno	<i>Hi</i>	Hawaii
<i>cult</i>	cultura, cultural, culture	<i>hisp</i>	hispanic, hispánicos
		<i>hist</i>	histoire, historiae, historialinen, historical, historická, historický, histórico, historicum, historique, historisch, history, historyczne
<i>D.C.</i>	District of Columbia	<i>hohenzoll</i>	hohenzollerische
<i>Del</i>	Delaware	<i>holstein</i>	holsteinisch
<i>demog</i>	demografie, demographische, demography		
<i>dept</i>	department	<i>iaz</i>	iazyka
<i>deux</i>	deuxième	<i>Ida</i>	Idaho
<i>dev</i>	developing, development	<i>Ill</i>	Illinois
<i>dig</i>	digest	<i>illus</i>	illustrated
<i>dipl</i>	diplomatic, diplomatique	<i>ind</i>	industrial, industry
<i>doc</i>	documentation, documents	<i>Inda</i>	Indiana
<i>dok</i>	dokuments	<i>individ</i>	individual
<i>drev</i>	drevnei	<i>inst</i>	Institut, institute, institution, instituto
<i>dtsh</i>	deutsche, deutschen, deutsches	<i>int</i>	internacional, international, internationale
		<i>interdisc</i>	interdisciplinary
<i>e</i>	east, eastern	<i>intern</i>	internal
<i>ec</i>	economics, économique, economy	<i>introd</i>	introduced, introduction
<i>eccles</i>	ecclesiastical	<i>ist</i>	istorii, istorijski, istoriski
<i>ecles</i>	eclesiástico	<i>istruz</i>	istruzione
<i>ed</i>	edited, edition, editor	<i>ital</i>	Italian, italiana, italienisch
<i>educ</i>	education		
<i>EEH</i>	<i>Explorations in Economic History</i>	<i>j</i>	journal
<i>EHR</i>	<i>English Historical Review</i>	<i>Jb</i>	Jahrbuch, Jahrbücher
<i>ELH</i>	<i>English Literary History</i>	<i>JEGP</i>	<i>Journal of English and Germanic Philology</i>
<i>Eng</i>	English		
<i>epig</i>	epigraphik, epigraphy	<i>jugoslav</i>	jugoslovenski
<i>Epis</i>	Episcopal	<i>jur</i>	juridical, juridiceski, juridique
<i>estud</i>	estudios		
<i>et</i>	études		
<i>ethnog</i>	ethnographisch		
<i>ethnol</i>	ethnological, ethnology		
<i>etnol</i>	etnología		

<i>kan</i>	kanonistisch	<i>oesterr</i>	oesterreichisch
<i>Kans</i>	Kansas	<i>ok</i>	ökonomie
<i>kath</i>	katholik	<i>Okla</i>	Oklahoma
<i>Kd</i>	Kunde	<i>Ore</i>	Oregon
<i>Kl</i>	Klasse	<i>organ</i>	organization
<i>Ky</i>	Kentucky	<i>orient</i>	oriental, orientale, orientalia, orientalistyczny
<i>La</i>	Louisiana	<i>österr</i>	österreichisch
<i>Landesk</i>	Landeskunde	<i>Osth</i>	Osthefte
<i>lang</i>	language	<i>Pa</i>	Pennsylvania
<i>lett</i>	letter, letterário, letterature, lettre	<i>Pac</i>	Pacific
<i>lib</i>	library	<i>pädagog</i>	pädagogik, pädagogisch
<i>Lib Cong</i>	U.S. Library of Congress	<i>paedagog</i>	paedagogica
<i>libr</i>	librarian	<i>pap</i>	papers
<i>ling</i>	linguistics, linguistique	<i>papyrol</i>	papyrologie
<i>lit</i>	literary, literatur, literature, literatury	<i>parl</i>	parlementaire, parlement
<i>lübeck</i>	lübeckische	<i>pfälz</i>	pfälzische
<i>lüneburg</i>	lüneburger	<i>phil</i>	philosophical, philosophique, philosophy
<i>mag</i>	magasin, magazine	<i>philol</i>	philology
<i>marit</i>	maritime	<i>photo</i>	photograph
<i>Mass</i>	Massachusetts	<i>pol</i>	political, politico, politics, Politik, politique, politische
<i>Md</i>	Maryland	<i>port</i>	portuguesa, portuguese
<i>Me</i>	Maine	<i>pres</i>	president, presidential
<i>med</i>	medieval, médiévale, medievales	<i>Presb</i>	Presbyterian
<i>meded</i>	mededelingen	<i>preuss</i>	preussisch
<i>Mediterr</i>	Mediterranean	<i>probl</i>	problems
<i>mél</i>	mêlages	<i>proc</i>	proceedings
<i>mém</i>	mémoires, memorial, memorie	<i>prot</i>	protestant, Protestantismus
<i>mennonit</i>	mennonitische	<i>prov</i>	providence, provinces
<i>Mex</i>	Mexican	<i>PSQ</i>	<i>Political Science Quarterly</i>
<i>Mich</i>	Michigan	<i>psych</i>	psychology
<i>mid</i>	middle	<i>pt</i>	part
<i>midcont</i>	midcontinental	<i>pts</i>	parts
<i>mil</i>	militaire, militarisch, military	<i>publ</i>	publication, publishing
<i>Minn</i>	Minnesota	<i>q</i>	quaderni, quarterly
<i>misc</i>	miscelánea, miscellany	<i>quel</i>	quellen
<i>Miss</i>	Mississippi	<i>r</i>	review, revista, revue, rivista
<i>Mitt</i>	Mitteilung, Mitteilungen	<i>rass</i>	rassegna
<i>Mo</i>	Missouri	<i>Rdsch</i>	Rundschau
<i>mod</i>	modern, moderna, moderne	<i>rec</i>	record
<i>mond</i>	mondiale	<i>rech</i>	recherches
<i>Mont</i>	Montana	<i>regist</i>	register
<i>monum</i>	monumenta	<i>relig</i>	religion, religiöse, religious
<i>movim</i>	movimento	<i>rend</i>	rendiconti
<i>mt</i>	mountain	<i>rep</i>	report, reporter
<i>mus</i>	musei, museum	<i>res</i>	research
<i>n</i>	north, northern	<i>rev</i>	revolution
<i>nac</i>	nacional	<i>rhein</i>	rheinisch
<i>nass</i>	nassauische	<i>R.I.</i>	Rhode Island
<i>nat</i>	national	<i>ric</i>	ricerche
<i>nationalok</i>	nationalökonomie, nationalökonomisk	<i>rocz</i>	roczniki
<i>naz</i>	nazionale	<i>röm</i>	römische
<i>N.C.</i>	North Carolina	<i>roman</i>	romanische
<i>N.D.</i>	North Dakota	<i>roy</i>	royal
<i>ne</i>	northeast	<i>s</i>	south, southern
<i>Nebr</i>	Nebraska	<i>S.C.</i>	South Carolina
<i>neutest</i>	neutestamentliche	<i>Scand</i>	Scandinavia, Scandinavian
<i>Nev</i>	Nevada	<i>schles</i>	schlesisch
<i>newslett</i>	newsletter	<i>Schr</i>	Schrift
<i>N.H.</i>	New Hampshire	<i>schweiz</i>	schweizerisch
<i>niedersächs</i>	niedersächsisch	<i>sci</i>	science, scientiarum, scientific, scientist, scienze
<i>N.J.</i>	New Jersey	<i>S.D.</i>	South Dakota
<i>N.M.</i>	New Mexico	<i>se</i>	southeast
<i>no</i>	number	<i>sec</i>	sectio, section
<i>nos</i>	numbers	<i>ser</i>	série, series
<i>Nor</i>	Norway	<i>slaw</i>	slawistik
<i>nord</i>	nordisk	<i>soc</i>	social, societatis, society
<i>norm</i>	normale		
<i>numis</i>	numismatic, numismatique		
<i>nw</i>	northwest		
<i>N.Y.</i>	New York		

<i>sociog</i>	sociographiques	<i>Va</i>	Virginia
<i>sociol</i>	sociologia, sociological, sociology	<i>vaterl</i>	vaterlandisch
<i>solothurn</i>	solothurnische	<i>ver</i>	Verein, vereinigung, Vereins
<i>sozial</i>	sozialistischen	<i>Verh</i>	Verhandlungen
<i>Soziol</i>	Soziologie	<i>Veröff</i>	Veröffentlichungen
<i>Span</i>	Spanish	<i>vesn</i>	vesnik
<i>spøl</i>	spølecznuch	<i>vest</i>	vestnik
<i>stat</i>	statistical, statistics, Statistik	<i>volksk</i>	volkskunde
<i>stift</i>	stiftung	<i>vopr</i>	voprosy
<i>stor</i>	storia, storici, storico	<i>vrem</i>	vremennuk
<i>stud</i>	studi, studia, Studien, studies, studium	<i>Vt</i>	Vermont
<i>sup</i>	superiore		
<i>suppl</i>	supplement		
<i>sw</i>	southwest	<i>w</i>	west, western
<i>Sw</i>	Sweden	<i>Wash</i>	Washington
<i>Swed</i>	Swedish	<i>westf</i>	westfälisch
<i>symp</i>	symposium	<i>wirtsch</i>	Wirtschaft, wirtschaftlich
		<i>Wis</i>	Wisconsin
<i>tech</i>	technisch	<i>wiss</i>	Wissenschaft, wissenschaftlich
<i>technol</i>	technology	<i>WMQ</i>	William and Mary Quarterly
<i>Tenn</i>	Tennessee	<i>württemb</i>	württembergisch
<i>test</i>	testament, testamentum	<i>W. Va.</i>	West Virginia
<i>Tex</i>	Texas	<i>Wyo</i>	Wyoming
<i>theol</i>	theological, theology		
<i>tids</i>	tidskrift, tidsskrift		
<i>tijd</i>	tijdschrift	<i>yrbk</i>	yearbook
<i>tr</i>	translated, translation, translator		
<i>trans</i>	transactions		
<i>trav</i>	travail, travaux		
		<i>Z</i>	Zeitschrift, Zeitschriften
<i>u</i>	und	<i>Zeitgesch</i>	Zeitgeschichte
<i>U</i>	Universitätti, University	<i>zgodov</i>	zgodovinski
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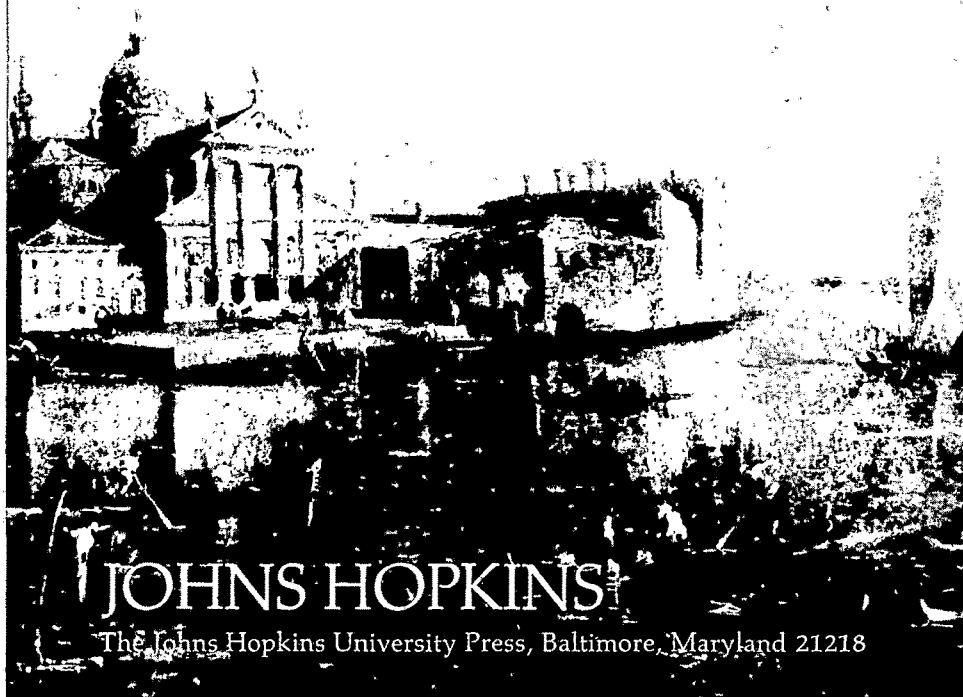
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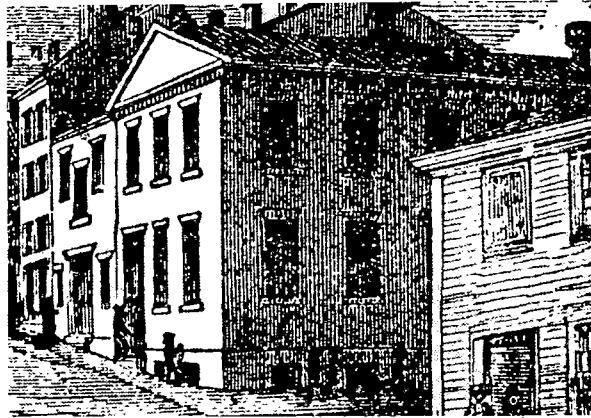
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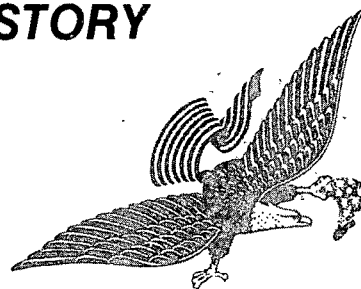
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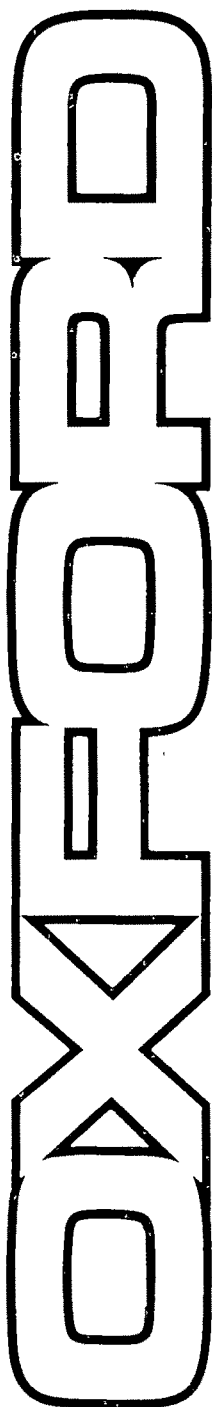
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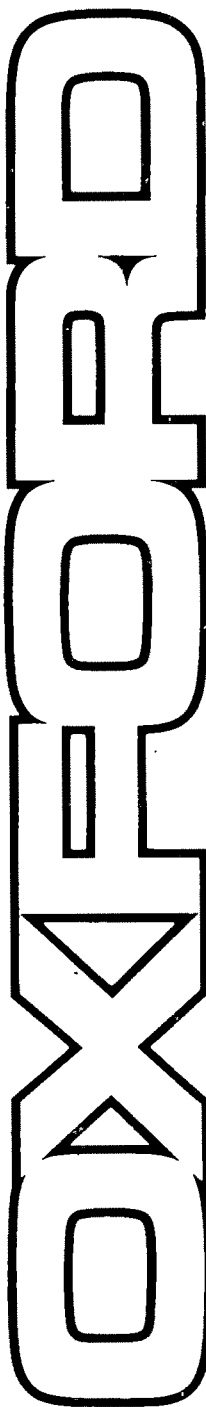
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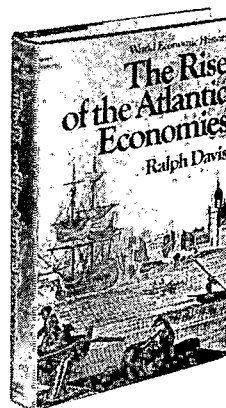
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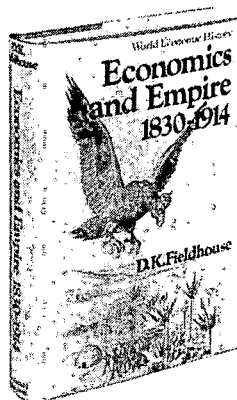


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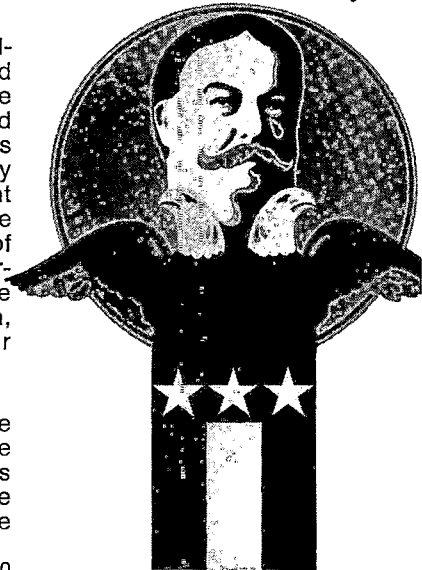
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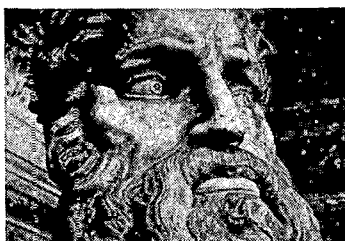
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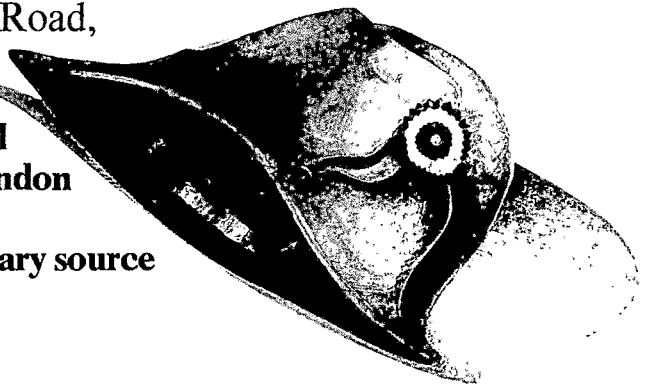
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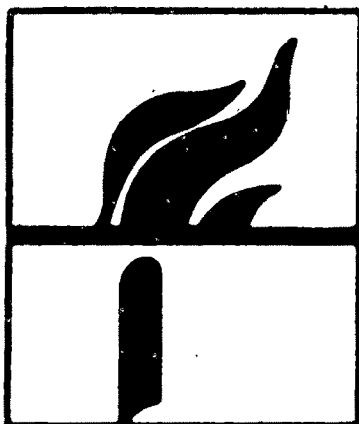
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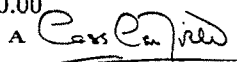
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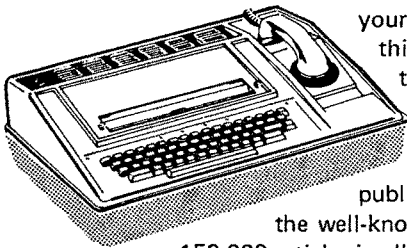
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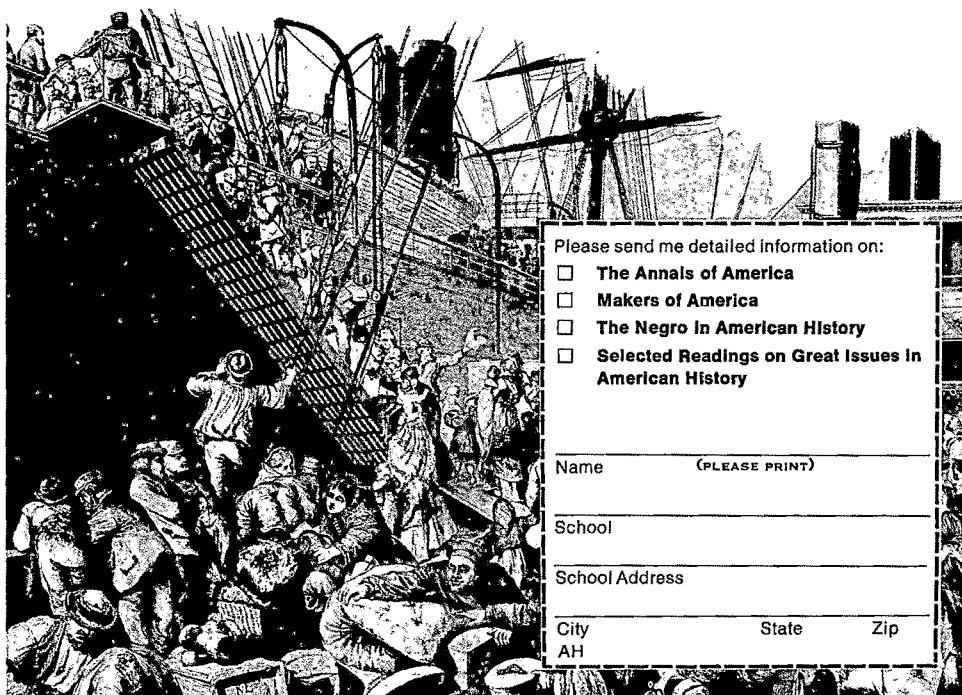
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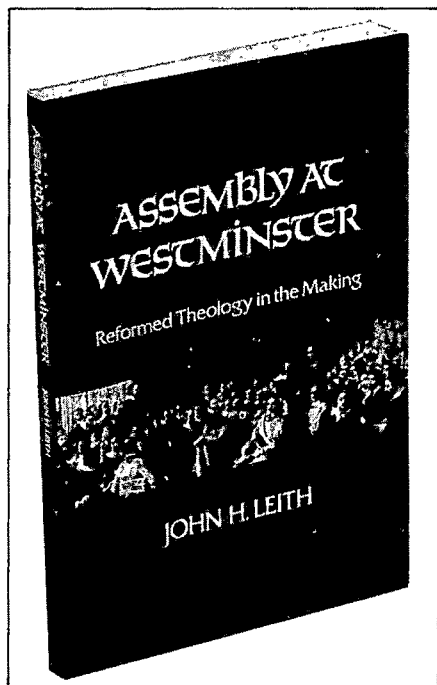
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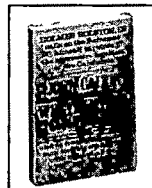
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# Death and Dying in Puritan New England

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DAVID E. STANNARD

IT IS NOW almost a cliché among historians to acknowledge that "tensions" arising from the seemingly paradoxical structure of much of the Puritan faith were an unavoidable and essential element of that faith. Expanding on a theme outlined by Max Weber over half a century ago, recent historians have found these tensions rooted in such matters as the Puritans' contempt for the merits of social virtue—"merit-mongering," Cotton Mather called it—while they simultaneously insisted on proper social conduct, and in their rejection of the ultimate importance of earthly material accomplishments at the same time that they sanctified work.<sup>1</sup> A substantial list of such contrasts might easily be compiled, the tensions apparent, to greater and lesser degrees, in many areas of their life and thought.<sup>2</sup> Principally, however, Weber observed, and historians have concentrated on, tensions between religious belief and worldly activity—between religion and economics, religion and education, religion and art, religion and the community. Less often noted are the tensions that existed within the religion itself between religious ideal and religious experience, tensions that resided solely within the religious sphere of the Puritans' lives but which may well have affected the very structure and durability of their culture.

The vision of death and the act of dying were to the Puritans profoundly religious matters. Much of the average Puritan's life was centered about and predicated on the vision of death, the afterlife, and the expected manner in which the passage from this world to the next should be made. So it was, and is, with most intensely religious communities. But in the Puritan scheme something was wrong, as though an improper "fit" was made between

For their critical readings of an earlier draft of this paper, I am grateful to the following: William A. Clebsch of Stanford University, Bruce Kuklick of the University of Pennsylvania, Michael McGiffert of the University of Denver, and David Brion Davis and Edmund S. Morgan of Yale University.

<sup>1</sup> For the most direct example of this aspect of Weber's thought, see his "Religious Rejections of the World and Their Directions," in H. H. Gerth and C. Wright Mills, eds., *From Max Weber: Essays in Sociology* (New York, 1946); Cotton Mather, *Parentator* (Boston, 1724), 185.

<sup>2</sup> A collection of readings has recently been organized about this theme. See Richard Reinitz, *Tensions in American Puritanism* (New York, 1970).



the vision of death and the manner of encountering it. The resulting tension, I will argue, haunted the devout Puritan throughout his life and grew particularly intense as death approached—and it is only the devout Puritan, not the entire populace of seventeenth-century New England, that is of concern here. But before considering this tension further and speculating on its larger cultural ramifications, it is worth considering the context out of which the Puritan experience emerged.

MEN HAVE ALWAYS feared death, as Bacon wrote, “as children fear to go in the dark.” The handling of this fear, the attempt somehow to mitigate it, is one of the ways cultures have of distinguishing themselves from one another. Thus among tribes of hunters who, as Joseph Campbell puts it, “live in a world of animals that kill and are killed and hardly know the organic experience of a natural death,” death is regarded as externally caused and is strenuously resisted by magic; while the Greeks saw death optimistically, indeed, as the beginning of life, “at least,” as Herbert Marcuse has observed, “for the philosopher.” In ancient Taoist China death was given its place in the “general attitude toward the universal laws of nature, which is one not merely of resignation nor even of acquiescence, but a lyrical, almost ecstatic acceptance”; while the Australian Aranda attempt to keep the spirit at bay by burning the village whenever a death occurs, by committing violent assaults on the grave itself, and by refusing ever again to utter the name of the deceased. Among the Javanese death is treated with a stoic calm, the body of the deceased being disposed of as quickly as possible, often within an hour or two; while in much of modern America cosmetically “restored” corpses lie in so-called slumber rooms for days—a few are even frozen—as acquaintances of the deceased make ritualized public showings of grief.<sup>3</sup>

In each of these cases the behavior in the face of dying is the result of the attitude toward, and the vision of, death. To use the language of the anthropologist, the *ethos*, “the tone, character, and quality of . . . life, its moral and aesthetic style and mood,” grows out of the *world view*, that is, the “picture of the way things, in sheer actuality are, [the] concept of nature, of self, of society.” The influence, of course, is mutual. Suggesting a certain synonymy between these terms and religious “belief” (world view) and “ritual” (ethos), Clifford Geertz writes: “Religious belief and ritual confront and mutually confirm one another; the ethos is made intellectually reasonable by being shown to represent a way of life implied by

<sup>3</sup> Francis Bacon, *The Essays, or Councils, Civil and Moral* (London, 1718), 3; Joseph Campbell, *The Masks of God: Primitive Mythology* (New York, 1959), 125–26; Herbert Marcuse, “The Ideology of Death,” in Herman Feifel, ed., *The Meaning of Death* (New York, 1959), 67; Arthur Waley, *The Way and its Power* (New York, 1949), 54–55; Baldwin Spencer and F. J. Gillen, *The Native Tribes of Central Australia* (London, 1899), 497–511; Clifford Geertz, *The Religion of Java* (New York, 1960), 68–76.

the actual state of affairs which the world-view describes, and the world-view is made emotionally acceptable by being presented as an image of an actual state of affairs of which such a way of life is an authentic expression."<sup>4</sup> The two phenomena, ethos (in the present case, the prescribed way of dying) and world view (the vision or concept of death) reinforce one another and thus fused give meaning, order, and stability to their cultural source.

As all cultures must, the early Christians formulated methods of dealing with the fact of death. Prior to the Christian era the Homeric Greeks had devised the concept of the soul, an idea that grew out of an earlier belief that the dead continued to live under the earth—the later vision of the disembodied spirit thus opening the way to the practice of cremation. It was not until the worship of Dionysius, however, and in the writing of Heraclitus, that the belief in the soul's immortality reached its full development. This answer to the fear of the void of death became a central tenet of Christian doctrine in the idea of the Resurrection. "I am the resurrection and the life," Christ had said, "he that believeth in Me, although he be dead shall live: and everyone that liveth and believeth in Me shall not die forever." Thus in the fourth century Augustine could say of the death of his mother, "We thought it not fitting to solemnise that funeral with tearful lament, and groanings; for thereby do they for the most part express grief for the departed, as though unhappy, or altogether dead; whereas she was neither unhappy in her death, nor altogether dead." And Saint Ambrose, in his oration at the funeral of Valentinian, says:

But if the gentiles, who have no hope of resurrection, are consoled by this alone, in that they say that after death the departed have no life and consequently no sense of pain remains, how much the more should we receive consolation because death is not to be feared, since it is the end of sin, and that life is not to be despaired of which is restored by the resurrection?<sup>5</sup>

In place of fear of the void, however, there evolved the fear of divine wrath, of punishment for sin; for along with the concept of immortality, Christianity devised a place of bliss and a place of misery as the potential residences of the soul. For the saved there was heaven, for the unrepentant hell, and eventually, for the great masses who had not yet been thoroughly cleansed of minor sins, there was purgatory, where, as Hamlet's ghostly father described it, the soul was

confined to fast in fires,  
Till the foul crimes done in my days of nature  
Are burnt and purged away.

<sup>4</sup> Clifford Geertz, "Ethos, World-View and the Analysis of Sacred Symbols," *Antioch Review*, 17 (1957-58): 421, 422.

<sup>5</sup> E. Rohde, *Psyche, The Cult of Souls and Belief in Immortality Among the Greeks* (New York, 1925), 19-24, 253-56; Bruno Snell, *The Discovery of the Mind*, tr. T. G. Rosenmeyer (New York, 1960), 17-18; John 11:25-26; Augustine, *Confessions*, tr. Edward B. Pusey (New York, 1949), 190; Ambrose, *De Consolatione Valentiniani*, 45, quoted in Alfred C. Rush, *Death and Burial in Christian Antiquity* (Washington, 1941), 11-12.

While hell was clearly a place to be avoided, and although the temptations of Satan were many and great, the sinful were afforded a variety of ways through which they might avoid that fate. Baptism cleansed the soul of original sin, confession and the administration of the Eucharist throughout life prepared the soul for heaven, the sacrament of extreme unction and the viaticum at death further cleared the way, and even while the imperfect but uncondemned soul lingered in purgatory, indulgences, requiem masses, and the prayers of the living helped improve the likelihood of imminent removal to heaven. Thus the fate of the individual was largely in his own hands and those of his family and friends; hell was a fearsome end for the soul, but an end that could, with a little work, be avoided. It is not surprising, then, to find the early Christian concept of death reflecting the optimistic *Migratio ad Dominum*, an image perhaps given its fullest previous expression among the ancient Egyptians.<sup>6</sup>

In effect the Christian world view (belief) and ethos (ritual) had successfully fused: supramundane existence was composed of God, of heaven, hell, and purgatory, and of the souls of men assigned their place principally on the basis of their earthly behavior; at the same time the institutions, from baptism to the requiem mass, had been created to make such a world view emotionally and intellectually acceptable. In theory at least, a believing and practicing Christian was armed against fear when death approached. A culturally functional concept of death had been constructed and made viable; now it had only to be maintained.

Fear, of course, still plagued men; even Christians. In Europe during the late Middle Ages it appears to have reached a peak of intensity, probably due in large measure to the devastating plagues that wracked that part of the world during the fourteenth and fifteenth centuries, though other possible explanations have been offered.<sup>7</sup> It was during this period that the *danse macabre* and *Ars Moriendi* traditions flourished, that tomb sculpture turned toward literal representations of the deceased in advanced stages of decomposition, that indeed, as Theodore Spencer has put it, "a great poet, like Villon, had nothing but death to write about." "It is hardly an exaggeration," Spencer writes, "to say that in Northern Europe the whole fifteenth century was frenzied about death."<sup>8</sup>

<sup>6</sup> See Rush, *Death and Burial*, 44-71.

<sup>7</sup> Theodore Spencer, for example, attributes much of the concentration and consternation concerning death in this period to the development of artistic realism and to an attendant rise in emotional identification with the death of Christ, that favorite artistic motif of that and virtually every other era. See Spencer, *Death and Elizabethan Tragedy* (Cambridge, Mass., 1936), 19-21.

<sup>8</sup> On the *danse macabre*, see Leonard P. Kurtz, *The Dance of Death and the Macabre Spirit in European Literature* (New York, 1934); on the *Ars Moriendi* see Sr. Mary C. O'Connor, *The Art of Dying Well: The Development of the Ars Moriendi* (New York, 1942); Erwin Panofsky, *Tomb Sculpture: Its Changing Aspects from Ancient Egypt to Bernini* (London, 1964), 63-66; cf. T. S. R. Boase, *Death in the Middle Ages* (London, 1972), 96-103; Spencer, *Death and Elizabethan Tragedy*, 32.

Frenzied, perhaps; but a controlled frenzy, a frenzy that operated within the confines of the Christian scheme. The primary medieval concern was with the physical horrors of death. The *danse macabre*, as Johan Huizinga has noted, was "a dance of the dead and not of Death." Its principal purpose was to "remind the spectators of the frailty and the vanity of earthly things . . . while at the same time [it] preached social equality as the Middle Ages understood it, Death levelling the various ranks and professions."<sup>9</sup> Similarly the poetry of Villon and others, while it concentrated on death, was most typically rendered in passages like the following:

Death makes him shudder and turn pale,  
The nose to curve, the veins to swell  
The neck to inflate, the flesh to soften  
Joints and tendons to grow and swell.<sup>10</sup>

And the *Ars Moriendi*, "in spite of its purpose," Mary C. O'Connor notes, "is not a doleful book—no clarion call to repentance. There is little stress upon hell, only hope of heaven. Always is *Moriens* encouraged and consoled."<sup>11</sup> Death, in the late Middle Ages, was a ghastly visitation upon the *body* of man, but fear of the soul's fate remained blunted by the Christian tradition. This is clearly seen in the popular belief of the period that, following his resurrection, Lazarus lived in constant torment that he might ever again have to endure the physical act of dying. As Huizinga observed: "The dominant thought, as expressed in the literature, both ecclesiastical and lay, of that period, hardly knew anything with regard to death but these two extremes: lamentation about the briefness of all earthly glory, and jubilation over the salvation of the soul."<sup>12</sup>

Christian optimism, the principal weapon against the fear of death, was not shaken by the morbidity of the Middle Ages, by, as T. S. R. Boase has recently put it, this "strange preoccupation with putrefaction."<sup>13</sup> Indeed by the middle of the sixteenth century it is not at all surprising to find John Haryngton, for example, writing:

Death is a porte whereby we pass to joye;  
Lyfe is a lake that drowneth all in payne;  
Death is so dear, it killeth all annoye;  
Lyfe is so lewd, that all it yields is vayne.<sup>14</sup>

And by the time the seventeenth century was under way, among the more typical responses to death were those of William Drummond, who

<sup>9</sup> Johan Huizinga, *The Waning of the Middle Ages* (London, 1952), 131.

<sup>10</sup> Villon, quoted in Huizinga, *Waning of the Middle Ages*, 133.

<sup>11</sup> O'Connor, *Art of Dying Well*, 5.

<sup>12</sup> Huizinga, *Waning of the Middle Ages*, 132.

<sup>13</sup> Boase, *Death in the Middle Ages*, 106.

<sup>14</sup> Quoted in Spencer, *Death and Elizabethan Tragedy*, 53.

saw it as "but a short, nay, sweet sigh; and . . . not worthy the remembrance"; of Jeremy Taylor, who wrote that "it is so harmless a thing, that no good man was ever thought the more miserable for dying, but much the happier"; or Sir Thomas Browne who, because of the expected negative reactions of others—even "the Birds and Beasts of the field"—to his physical remains, was "not so much afraid of death, as ashamed thereof."<sup>15</sup>

The seventeenth century, however, did not belong entirely to the Drummonds, the Taylors, and the Brownes; it was also the century of the Mathers, the Hookers, and the Willards—the century of New England Puritanism. The Puritans, though heirs to the Christian tradition, sought to purify that tradition as it had evolved in their time, to cut through the extraneous trappings of the formal Church and revive the spirit of the earliest Christians. With this in mind they had closely examined the teachings of the Church of which they were a part, the Anglican, and had challenged many of those teachings, and in the end some of them sought a richer environment for their work in the rocky soil of New England.

"SAVING GRACE," as the Puritans called it, was crucial to their quest for salvation and was imparted by a power beyond coercion. Without it, regardless of how good one may have been in mortal life, there was simply no possibility of moving to one of the most critical levels in the Puritan morphology of conversion, the level of assurance. "The Lord to shew the sovereign freedom of his pleasure," Thomas Hooker wrote, "that he may do with his own what he wil, and yet do wrong to none, he denyes pardon and acceptance to those who seek it with some importunity and earnestness . . . and yet bestowes mercy and makes known himself unto some *who never sought him*."<sup>16</sup> Assurance, this important step up the steep ladder of salvation, was thus a step fraught with much difficulty and confusion. "It is a great exercise to some Saints," noted Solomon Stoddard,

whether they be sincere Saints, they labour in it for many years; and one Minister gives signs, and they try themselves by them, and another gives signs, and they try themselves by them; and sometimes they think they see the signs of Saints, and sometimes the signs of hypocrites: and they dont know what to make of themselves.<sup>17</sup>

Once assurance was attained, however, once God had provided some evident sign of saving grace, the believer was accepted by the community and himself as a Visible Saint, that is, as having been saved. Assurance nonetheless did not guarantee salvation. Men could both deceive and be deceived,

<sup>15</sup> William Drummond, *A Cypress Grove* [1623] (London, 1909), 69; Jeremy Taylor, *The Rule and Exercises of Holy Dying* [1651] (New York, 1869), 95; Sir Thomas Browne, *Religio Medici* [1643] in Browne, *Works*, ed. Charles Sayle (Edinburgh 1912), 1: 58.

<sup>16</sup> Thomas Hooker, *The Application of Redemption* (London, 1657), 299.

<sup>17</sup> Stoddard, *The Tryal of Assurance* (Boston, 1699), 17.

and thus there was always a lingering doubt—indeed, a necessary doubt. “Even after he reached the stage of assurance,” Edmund S. Morgan has observed, “his doubts would continue. If they ceased, that would be a sign that he had never had faith to begin with, but had merely deluded himself and had not really entered into the covenant of grace.”<sup>18</sup> The cessation of doubt was a sign of “security,” of that false assurance of which, in 1629, Arthur Hildersam wrote,

for one that Sathan hath overthrowne by desperation, there are twenty whom he hath overthrowne with this false assurance. Wee are therefore to be exhorted to examine our assurance. . . . For, as the true assurance of Gods favour, is a comfortable thing; so is a false peace and assurance one of the most grievous judgements that can befall a man. . . . Of the two, it were better for a man to be vexed with continuall doubts and feares, than to be lulled asleepe with such an assurance. For, besides that it keeps a man from seeking to God, it will not hold, but faile him, when he shall have most neede of it.<sup>19</sup>

The roots of this necessary doubt were deep, and the effects wide-ranging and persistent. C. C. Goen has called it “one of the most vexing problems in the Great Awakening.” As Jonathan Edwards commented, over a century after Hildersam, on the behavior of “the greater part” of newly converted Saints:

They generally have an awful apprehension of the dreadfulnes and undoing nature of a false hope; and there has been observable in most a great caution, lest in giving an account of their experiences, they should say too much, and use too strong terms. And many after they have related their experiences, have been greatly afflicted with fears, lest they have played the hypocrite, and used stronger terms than their case would fairly allow of; and yet could not find how they could correct themselves.<sup>20</sup>

Thus for as long as he lived even the most apparently obvious candidate for Sainthood did not dare take his election for granted; there was no way in this world of knowing with certainty whether one was saved or not. The best sign of assurance, in other words, was to be unsure. As a result the devout Puritan constantly examined himself and assailed every evidence of impurity, filling journals and diaries with interminable exhortations on the depravity of all men, most of all himself. For as Jonathan Edwards put it, “There is no man on earth, that is so just, as to have attained to such a degree of righteousness, as not to commit any sin.” Wickedness, “if divine grace does not prevent . . . may as truly be said to be the effect which man’s natural corruption tends to, as that an acorn in a proper soil, truly tends by

<sup>18</sup> Edmund S. Morgan, *Visible Saints* (Ithaca, 1963), 69. Cf. Norman Pettit, *The Heart Prepared: Grace and Conversion in Puritan Spiritual Life* (New Haven, 1966), 18–19.

<sup>19</sup> Arthur Hildersam, *Lectures upon the Fourth of John* (London, 1629), 311.

<sup>20</sup> Jonathan Edwards, *A Faithful Narrative* [1737], reprinted in *The Works of Jonathan Edwards*, ed. C. C. Goen, 4 (New Haven, 1972): 186. The Goen quote is from *ibid.*, 47; but see also his *Revivalism and Separatism in New England, 1740–1800* (New Haven, 1969), 44–54.

its nature to become a great tree." The Puritan faith, upon one tenet of which John Preston had pronounced in 1633, "this is a very comfortable doctrine, if it be well considered," was instead a faith marked by a never-ending, excruciating uncertainty. "Surely if ever a theology tortured its votaries," Perry Miller has written, "it was that taught by New England divines, and if ever mortal was driven to distraction it was the mother who, as Winthrop tells, drowned her child that it might escape damnation."<sup>21</sup>

An equally critical ambivalence was built into the Puritan view of death. On the one hand, life was seen as but a "vapour," a fleeting "pilgrimage"—the latter word apparently taken from the eleventh chapter of Hebrews, where it is applied to Abraham and his descendants.<sup>22</sup> It is common to come upon such references in Puritan writing as Increase Mather's mention of "the dayes of my pilgrimage now drawing to their close," of his wife as "the Dear Companion of my Pilgrimage on Earth," or of John Collins offering a book to the reader as being "peculiarly suited to the support and consolation of the Saints in this their wayfaring and afflictive pilgrimage." It was in the afterlife that the Saints were to be rewarded and the sinful punished. As Collins wrote: "Death is only sweetened to us as we can look upon it our priviledge; as an out-let from sin and misery, and an in-let to *Glory* both in Holiness and Happiness."<sup>23</sup>

Collins's attitude was neither uncommon nor new. More than a century earlier William Perkins described death as "a blessing . . . as it were a little wicket or doore whereby we passe out of this world and enter into heaven." Indeed Increase Mather once wrote after contemplating the beauty of the soul's flight to heaven, "the thought of this should make the Believer long for death."<sup>24</sup> The desirability of such longing was given Biblical justification, most frequently 2 Corinthians 5:6-8: "Being therefore always of good courage, and knowing that, whilst we are at home in the body, we are absent from the Lord (for we walk by faith, not by sight); we are of good courage, I say, and are willing rather to be absent from the body, and to be at home with the Lord."

All of this was, to be sure, consistent with the long Christian tradition already observed. The Bible does, however, have other things to say about

<sup>21</sup> Jonathan Edwards, *Original Sin*, in Edwards, *Works* (New York, 1881), 2: 328; John Preston, *The New Creature* (London, 1633), 23; Perry Miller, *The New England Mind: The Seventeenth Century* (Boston, 1961), 56. See also, Michael McGiffert, ed., *God's Plot: The Paradoxes of Puritan Piety* (Amherst, Mass., 1972), esp. 10-29, for an insightful outline of what the author calls the "dialectic of contraries" involved in the phenomenon of Puritan Sainthood.

<sup>22</sup> For a full discussion of the term, see the research of Albert Mathews in *Colonial Society of Massachusetts, Publications*, 17 (1915): 300-92.

<sup>23</sup> Increase Mather, *The Mystery of Christ Opened and Applied* (Boston, 1686), 2; Mather, *A Sermon Concerning Obedience & Resignation to the Will of God* (Boston, 1714), 38; John Collins, "To the Reader," in Jonathan Mitchel, *A Discourse of the Glory* (2d ed.; Boston, 1721), 2, 4-5.

<sup>24</sup> William Perkins, *Salve for a Sicke Man* (Cambridge, 1597), 5; Increase Mather, "Preface," in Mitchel, *Discourse of the Glory*, iv.

death, things present but less emphasized in earlier Christian thought. Among the more important passages is Romans 5:12: "Therefore, as through one man sin entered into the world, and death through sin; and so death passed unto all men, for that all sinned." The reference to "one man," of course, is to the story of Genesis. It was not lost on the Puritans. Whenever they listed the "stings of death," as Leonard Hoar did in 1680, the very first was that death is punishment for sin. "The first sting of death," Hoar wrote, "is that it came into the world through man's own fault. . . . It is sin brought in death as a curse and punishment. Death comes from God, not as instituting the course of Nature at first; but as revenging sin." Indeed death, Hoar writes, "is the greatest evil in the world." Again and again the image of death as a dreadful punishment for the sin of Adam surfaces in Puritan writing, from the simple tombstone carving: "Death which came on man by the fall, / cuts down father child and all" to the eloquence of Jonathan Edwards:

For death, with the pains and agonies with which it is usually brought on, is not merely a limiting of existence, but is a most terrible calamity; and to such a creature as man, capable of conceiving of immortality, and made with so earnest a desire after it, and capable of foresight and of reflection on approaching death, and that has such an extreme dread of it, is a calamity above all others terrible, to such as are able to reflect upon it. . . . It is manifest, that mankind were not originally subjected to this calamity. . . . Sin entered into the world, and death by sin, as the apostle says.<sup>25</sup>

The ambivalence inherent in such a dual concept—that death is in a sense both punishment and reward, and that those facing it should regard it as one or the other—is evident in virtually every Puritan funeral sermon or other discourse on the subject.<sup>26</sup> Somehow, although the formal resolution of this conflict through the intercession of the death of Christ was acknowledged by Puritans at least as early as Perkins,<sup>27</sup> their constant references to the viability of the earlier view of death as punishment clearly belies any total, exclusive satisfaction with either image.

Whatever the specific image of the moment, death as a phenomenon seems to have been one of the more important preoccupations of the devout Puritan, as it was in the late Middle Ages. Although not exposed to an inordinate amount of death when compared with his relatives in England—in fact, it is probable that he lived a comparably healthier and longer life<sup>28</sup>—

<sup>25</sup> Leonard Hoar, *The Sting of Death* (Boston, 1680), 4, 3; epitaph from the Hull stone, Cheshire, Connecticut, cited in Allan I. Ludwig, *Graven Images* (Middletown, Conn., 1966), 88; Edwards, *Original Sin*, 372.

<sup>26</sup> For a particularly clear example of the confusion wrought by this ambivalence, see Urian Oakes's long poem, *An Elegie Upon the Death of the Reverend Mr. Thomas Shepard* (Boston, 1677).

<sup>27</sup> Perkins, *Salve for a Sicke Man*, 4-5; see also his *Golden Chaine* (Cambridge, 1597), 168-72.

<sup>28</sup> Cf. C. W. Chalkin, *Seventeenth-Century Kent* (London, 1965), 33-41; Peter Laslett, *The World We Have Lost* (New York, 1965), ch. 5; John Demos, *A Little Commonwealth* (New York, 1970), 65-66, 192-93; Philip J. Greven, Jr., *Four Generations* (Ithaca, 1970), 24-29, 106-10, 269; Kenneth A. Lockridge, *A New England Town* (New York, 1970), 67-69.



it was the unquestioned duty of every right-thinking Puritan to keep the thought of death ever on his mind. "A prudent man," Cotton Mather wrote, "will *Dy Daily*; and this is one Thing in our doing too: Tis to *live Daily* under the power of such Impressions, as we shall have upon us, when we come to Dy. . . . Every Time the *Clock Strikes*, it may *Strike* upon our Hearts, to think, *thus I am one Hour nearer to my last!* But, O mark what I say; That *Hour* is probably *Nearer* to None than to such as *Least* Think of it." Later, in *Death Made Easie & Happy*, he urges the reader to remind himself daily "that he is to die shortly. Let us look upon everything as a sort of Death's-Head set before us, with a *Memento mortis* written upon it."<sup>29</sup> Even children were early immersed in the required preoccupation of their elders, whether learning the alphabet and encountering such rhymes as: "G—As runs the *Glass* / Man's life doth pass; T—*Time* cuts down all / Both great and small; X—*Xerxes* the great did die, / And so must you & I; Y—*Youth* forward slips / Death soonest nips,"<sup>30</sup> or obeying the advice of an esteemed teacher like Joseph Green: "Remember Death; think much of death; think how it will be on a death bed."<sup>31</sup> One has only to leaf casually through the pages of Samuel Sewall's *Diary* to see repeated examples of this preoccupation in action.

But what of it? What do these tensions and preoccupations add up to? What do they mean? Few writers have had anything to say about the Puritan encounter with death, but this should not be surprising; neither has there been, until very recently, much psychoanalytic work on the subject of death in any sense. One psychiatrist who has investigated the subject at some length suggests that "psychiatrists, no less than other mortal men, have a reluctance to consider or study a problem which is so closely and personally indicative of the contingency of the human estate."<sup>32</sup> Whether he is accurate in this assessment, or whether it applies to historians, is of minor importance here; what is important is that, for whatever reason, there is a definite paucity of literature on the subject. Nevertheless, within the small body of writing that does exist there appears the general impression that the Puritans confronted death optimistically, with neither doubts nor fears. Perry Miller, for example, directs himself to what he terms the "cosmic optimism" of the Puritans in facing all manner of adversity, then notes the relatively "few sermons specifically devoted to immortality compared with the tremendous number drawing out the lessons of depravity or analyzing in minute detail the processes of regeneration. Perhaps the expectation of immortality was so

<sup>29</sup> Cotton Mather, *The Thoughts of a Dying Man* (Boston, 1697), 38–39; see also his *Awakening Thoughts on the Sleep of Death* (Boston, 1712), 16–20, for similar thoughts and phrasing; Mather, *Death Made Easie & Happy* (London, 1701), 94.

<sup>30</sup> *The New England Primer* [1727] in facsimile copy of the reprinted 1897 edition, ed. Paul Leicester Ford (New York, 1962).

<sup>31</sup> Joseph Green, *The Commonplace Book of Joseph Green* (1696), ed. Samuel Eliot Morison, Colonial Society of Massachusetts, *Publications*, 34 (1943): 204.

<sup>32</sup> C. W. Wahl, "The Fear of Death," in Feifel, *Meaning of Death*, 19.

axiomatic," he surmises, "that little discussion was needed, but I am inclined to suspect that because their energies were so intensely concentrated upon the problems in hand they had few left for doubts about those to come." Or, as Allan I. Ludwig has more recently, and somewhat more dramatically, put it: "In the midst of darkness and confusion there was light, the triumph of Death was overcome by eternity. The fear of death gave way to the thrill of spiritual pleasures yet to come as archangels trumpeted the glorious day."<sup>33</sup>

The evidence does not confirm this interpretation. Instead it suggests that the Puritans were gripped individually and collectively by an intense and unrelenting fear of death, while simultaneously clinging to the traditional Christian rhetoric of viewing death as a release and relief for the earth-bound soul. Increase Mather provides a clear-cut example of this duality. Mather was fond of the kind of declaration cited earlier, indicating that believers should long for the deliverance of death. Indeed in one of his sermons published in 1715 he cried: "I know that the time of my departure out of this World is now very near at hand. . . . And now that I am *Preaching Christ*, how glad should I be, if I might dye before I stir out of this pulpit!"<sup>34</sup> But eight years later, when death was in fact near at hand, his reaction was quite different. As his son Cotton relates and explains it:

And in the Minutes of the Darkness wherein he lay thus *feeble and sore broken*, he sometimes let fall expressions of some *Fear* lest he might after all be Deceived in his *Hope* of the *Future Blessedness*. His Holy Ministry having very much insisted on that Point, that *no care could be too much to prevent our being Deceived in that Important Matter*; tis no wonder, that as the *Dark Vapours* which assaulted and fettered his Intellectual Powers, broke in upon him, his Head should run much upon the Horror of being *Deceived at the last*. Yea, had there not been anything at all of a *Natural Debilitation* and *Obnubilation* in it, yet it were a very *Supposeable* thing, and not at all to be wondered at, if the *Serpent* be let loose to vex a *Servant of GOD* in the *Heel* of his *Life*; and if the *Powers of Darkness*, knowing the *Time to be short*, fall with *Great Wrath* on the Great Opposers of their *Kingdom*, and make a very *Dark Time* for them just before the *Break* of the *Eternal Day* upon them. And how justly might it awaken the rest of us to *Work out our own Salvation with Fear and Trembling*, when we see such a Man as Dr. *Mather*, concerned with so much *Fear and Trembling*, lest he should be *Deceived at the last*? . . . The best Judges of Things have agreed in this Judgment; That going to Heaven *in the way of Repentance*, is much safer and surer than going *in the way of Extasy*.<sup>35</sup>

This passage not only illustrates the difference between Increase's earlier pronouncements and his actual deathbed behavior; with equal force it

<sup>33</sup> Miller, *The New England Mind*, 37-38; Ludwig, *Graven Images*, 108.

<sup>34</sup> Increase Mather, *Several Sermons* (Boston, 1715), 59-60.

<sup>35</sup> Cotton Mather, *Parentator*, 207-08.

points out the dissonant nature of the father's experience of death and the son's "rhetorical" interpretation of it. Cotton, after all, remained clearly convinced of his father's salvation, despite the force of his father's despair.

Increase Mather had an exceptional recording secretary in his son, and such meticulous descriptive passages are more the exception than the rule among extant materials. Still, the elder Mather's experience was not unique, nor was it contrary to what was expected of devout Puritans. More than forty years before Increase Mather's birth, William Perkins had opened one of his works with a quote from Ecclesiastes—"The day of death is better than the day that one is borne"—and then proceeded to observe that "not only wicked and loose persons despaire in death, but also repentant sinners, who oftentimes in their sickness, testifie of themselves that being alive and lying in their beds, they feele themselves as it were to be in hell, and to apprehend the very pangs and torments therof."<sup>36</sup>

One of the earliest extant funeral sermons preached in New England was that of Samuel Wakeman for the departed soul of John Tappin of Boston, a victim of death at the age of eighteen. That Tappin was a godly youth is testified to by Wakeman and appears evident from the entire middle section of the sermon, a warning to the "rising generation" to make haste in "setting their hearts seriously God-ward," which was written by the young man for delivery upon his death. Still, when in the end Wakeman describes Tappin's final moments, he notes that Tappin

looked upon himself an undone man without an interest in Jesus Christ; yet he was not without some hope that he was at peace with God in him, yet not without fears, bemoaning himself in respect of his hardness of heart and blindness of minde, and that he had been no more thoroughly wrought upon by the Means that he had formerly enjoyed. O Sirs, Dying times are Trying times.<sup>37</sup>

Writing seven years later, Leonard Hoar sounds almost jealous of what he sees as the comparative ease with which too many of the ungodly depart the world:

I acknowledge its an error in the saints and people of God to be so much affrighted at death, and to goe so mournfully out of this world: surely they have not learn'd to look off this world, and to look up to that which is to come; yet I am sure it is a greater error, and a damning error in the wicked and ungodly when they are not afraid of Death at all, when they look upon Death as a common and usual event and have slight thoughts of it, all their complaints are out of sympathy with their friends, or from their bodily pains, or their distracted thoughts about worldly matters, but never consider what a weighty thing it is to dye well, or what a dreadful thing it is to miscarry in their latter end. And O the wretched stupidity of some that can be then most secure, and are even like the beasts which perish, *Psal.* 49.12. O then blame not any that have woeful apprehensions of death, and beware of indulging your selves in a stupid secure

<sup>36</sup> Perkins, *Salve for a Sicke Man*, 6.

<sup>37</sup> Samuel Wakeman, *A Young Man's Legacy to the Rising Generation* (Boston, 1673), 45.

frame, for as Solomon saith in another case, *Prov.* 23.32. It will sting like an Adder and bite like a Serpent.<sup>38</sup>

As might be expected, Cotton Mather also had something to say on the subject. "I have seen Persons Quaking on their Death beds," he writes, "and their very Beds therewith Shaking under them; From whence their first Shriek unto me, has been, *O! Sir, The wrath of a Dreadful God, makes me Tremble; I Tremble, I Tremble, at that Wrath.*" Further, he notes:

I knew, a very stout man, who, as he was going to Dy, said, *I have been among Drawn Swords, and Armed men; I have stood before the mouths of Roaring Canons, and where the Bullets have come as Thick as Hail about me; and yet I never knew what it was to be afraid: But now I am apprehensive of my being Exposed unto the Anger of the Almighty God, my Heart is overwhelmed with the Dread of it, my Heart even Dies within me, at the Thought of That!*<sup>39</sup>

And then there is the testimony of the tombstone. The epitaph on the James Hickox stone reads:

Great God, how oft thy wraith appears  
And cuts off our expected years  
Thy wraith awakes our humble dread  
We fear the Tyrant which strikes us dead.<sup>40</sup>

The reason for this apparent dread of death among godly Puritans—"a King of Terrors," as it was often called<sup>41</sup>—seems rooted in a combination of their theology and their everyday sense of reality. Their theology taught them of their utter and total depravity, of their helplessness in securing their own salvation, and reinforced this pessimism with the doctrine of assurance; as noted earlier, doubt of salvation was essential to salvation and that Puritan who, for so long as he breathed, became at any time secure and comfortable in the knowledge of his salvation, was surely lost. A striking example of the kind of emotional stress created by this doctrine is the case of the deathbed scene of that formidable personality, John Knox, as related by William Perkins:

He [Knox] lay on his death bedde silent for the space of foure hours, very often giving great sighes, sobbes, and grones, so as the standers by well perceived that he was troubled with some grievous temptation: and when at length he was raised in his bedde, they asked him how he did, to whome he answered thus: that in his life he had indured many combates and conflicts with Satan, but that now most mightily the roaring lyon had assaulted him: often (said he) before he set my sinnes before mine eyes, often he urged me to desperation, often

<sup>38</sup> Hoar, *Sting of Death*, 11–12.

<sup>39</sup> Cotton Mather, *Thoughts of a Dying Man*, 37, 35–36.

<sup>40</sup> Ludwig, *Graven Images*, 82.

<sup>41</sup> See, for example, Samuel Willard, *A Compleat Body of Divinity* (Boston, 1726), 234 (from Sermon 66, Oct. 31, 1693); and Cotton Mather, *Euthanasia* (Boston, 1723), 7.



Fig. 1. The temptation of despair. Fifteenth-century block print, from *Ars Moriendi*.



Fig. 2. Triumph over the temptation of despair. Fifteenth-century block print, from *Ars Moriendi*.



*Fig. 3. Detail of the Susanna Jayne gravestone, 1776, Marblehead, Massachusetts. Death, crowned with the laurel of victory, holds the sun and moon in either hand. Angels of heaven and bats of hell adorn each corner; the hooped serpent, symbol of eternity, surrounds Death. Photograph from Allan I. Ludwig, *Graven Images: New England Stonecarving and Its Symbols, 1650-1815* (Middletown, Conn.: Wesleyan University Press, 1966).*





*Fig. 4.* Detail of the Joseph Tapping gravestone, 1678, King's Chapel, Boston, Massachusetts. An allegorical rendering of the fateful interplay between Time and Death. Photograph from Ludwig, *Graven Images*.



he laboured to intangle me with the delights of the world, but beeing vanquished by the sword of the spirit, which is the word of God, he could not prevaile. But now he assaults me another way: for the wily serpent would perswade me that I shall merit eternall life for my fidelitie in my ministrie. But blessed be God which brought to my minde such Scriptures whereby I might quench the fierie darts of the devill, which were, *What hast thou that thou hast not received: and, By the grace of God, I am that I am: and, not I but the grace of God in me:* and thus being vanquished he departed.<sup>42</sup>

A break from the established Christian tradition of dealing with death can be clearly perceived here. In the *Ars Moriendi*, for example, the dying man is assaulted by a variety of demons tempting him with infidelity, despair, impatience, vainglory, and avarice, but the victim, aided by a battery of saints and angels, attains salvation by resolutely clinging to his optimistic belief in his own goodness and the justness of God.<sup>43</sup> Knox, on the other hand, though subject to similar temptations during his life, was tempted in precisely the reverse manner when death grew near—tempted, that is, by security founded on the heretical premise of good works—and attained salvation by denying it.

In order for the doctrine of assurance to be effective, it was essential that believers have an unquestioning faith in the reality of the contrasting terrors and bliss of the afterlife. At a time when, as D. P. Walker has pointed out, the doctrine of eternal torment for the damned was beginning to come under attack in England and on the Continent, among the New England Puritans there remained no plainer reality.<sup>44</sup> And while it is true that in the past the fire and brimstone nature of the Puritan sermon and tract has been greatly exaggerated, it is equally true that the New England ministry did not hesitate to conjure up explicit pictures of the terrors of hell when it suited them, with the result that, in the words of Increase Mather's uncommon understatement, "oft times there were more weepers than sleepers in the Congregation."<sup>45</sup> Thus, "If their Strength were the Strength of Stones," Solomon Stoddard wrote of the unregenerate in 1713,

or their Flesh of Brass, they could not endure their Misery. They will have *Anguish of Spirit*, not know what in the World to do; there will be dreadful Wailing, Mat. 13.42. They will lament their Sins, they will bewail the loss of Opportunities; they will condemn their Folly, they will curse themselves, they will wish they had never seen such things as now their Hearts dote upon . . . they will wish they had no Senses; their Hearing and Seeing and Feeling will be their Misery, their Memory, their Understanding, their Conscience will be their Torment; they will wish they had no Bodies, and wish they had no Souls, their *Bodies and Souls will be Vessels of Wrath*.

<sup>42</sup> Perkins, *Salve for a Sicke Man*, 55.

<sup>43</sup> *The Ars Moriendi* ([ca. 1450]; reproduction, London, 1881).

<sup>44</sup> D. P. Walker, *The Decline of Hell* (Chicago, 1964).

<sup>45</sup> In Mitchel, *Discourse of the Glory*, viii. Mather is here specifically referring to Mitchel's ministry.

Further, he reminds his audience, all the sufferings of this place where "the Worm dyeth not," where "the Fire is not quenched," are eternal:

The duration of their Misery cannot be measured: We may measure the breadth of the Earth, and the circuit of the Heaven, but can't measure Eternity. Add thousands to thousands, and multiply Millions by Millions; fill Quires of Paper with numbers, and you can't measure Eternity; It cannot be divided into Days, or Years, or Ages; make never so many Parts of it, one will be Eternal: When Men have suffered never so long, there is an Eternity remaining: it don't grow shorter and shorter. This makes every part of their Misery Infinite, their pain will be Infinite, the Terroure Infinite. If Miseries End, there is an opportunity for Comfort afterwards; but Eternity cuts off opportunities for Comfort: Men may well say, *Who can dwell with everlasting Burnings?*<sup>46</sup>

In the middle of the seventeenth century the Anglican bishop, Jeremy Taylor, had assured his readers that "God knows that the torments of hell are so horrid, so insupportable a calamity that He is not easy and apt to cast those souls which he hath taken so much care and hath been at so much expense to save, into the eternal, never-dying flames of hell lightly, for smaller sins, or after a fairly begun repentance, and in the midst of holy desires to finish it." And a century later Marie Huber was writing tracts with a wide European influence, arguing that "the Doctrine of the Eternity of Hell-Torments was not so incontestable, as not to be called in question by a great number of judicious men."<sup>47</sup> But at the same time Jonathan Edwards was traveling about New England, drawing and spellbinding crowds with such passages as:

How dismal will it be, when you are under these racking torments, to know assuredly that you never, never shall be delivered from them; to have no hope: when you shall wish that you might but be turned into nothing, but shall have no hope of it; when you shall wish that you might be turned into a toad or a serpent, but shall have no hope of it; when you would rejoice, if you might but have any relief, after you shall have endured these torments millions of ages, but shall have no hope of it; when after you shall have worn out the age of the sun, moon, and stars, in your dolorous groans and lamentations, without any rest day or night, or one minute's ease, yet you shall have no hope of ever being delivered; when after you shall have worn out a thousand more such ages, yet you shall have no hope, but shall know that you are not one whit nearer to the end of your torments; but that still there are the same groans, the same shrieks, the same doleful cries, incessantly to be made by you, and that the smoke of your torment shall still ascent up forever and ever; and that your souls, which shall have been agitated with the wrath of God all this while, yet will still exist to bear more wrath; your bodies, which shall have been burning and roasting all this while in these glowing flames, yet shall not have been consumed, but will remain

<sup>46</sup> Solomon Stoddard, *The Efficacy of the Fear of Hell to Restrain Men From Sin* (Boston, 1713), 24, 26.

<sup>47</sup> Taylor, *Holy Dying*, 85; Marie Huber, *The World Unmask'd* (London, 1736), 262.





Fig. 6. Detail of the left panel of the William Dickson gravestone, 1692, Cambridge, Massachusetts. This imp of the underworld carries an hourglass and is armed with the arrow of Death. Photograph from Ludwig, *Graven Images*.



Fig. 7. Detail of the right panel of the Rebekah Bunker gravestone, 1709, Cambridge, Massachusetts. An example of the erotic imagery that sometimes is found in Puritan gravestones. Photograph from Ludwig, *Graven Images*.

Fig. 5, facing page. Detail of the Thomas Kendel gravestone, ca. 1678, Wakefield, Massachusetts. Winged death-head supported by a pillar probably signifies the inevitable triumph of death. Photograph from Ludwig, *Graven Images*.

to roast through an eternity yet, which will not have been at all shortened by what shall have been past.<sup>48</sup>

Even Charles Chauncy, Edwards's determined Puritan rival and a well-known religious "liberal," was not outside the mainstream. As he warned his congregation in the summer of 1741:

There is nothing betwixt you and the place of blackness of darkness, but a poor frail, uncertain life. You hang, as it were, over the bottomless pit, by the slender thread of life, and the moment that snaps asunder, you sink down into perdition. . . . Who has bewitched you, O sinners, that you are thus lost to all sense of your own safety and interest! Be convinc'd of your danger. You are certainly in a state of dreadful and amazing hazard.<sup>49</sup>

As death drew near for the Puritan the tension normally built into the doctrine of assurance became increasingly more intense, for now the time of decision was at hand, the time when the Puritan's sin-riddled soul would be judged and either admitted to heaven or cast into the fiery pit of hell. And if he or she was not properly prepared there was no way out, no such thing, as the Catholics and some early Christians believed, as a last-minute sacramental reprieve. Whereas centuries earlier the dying Empress Matilda, mother of the future Henry II, had supposedly "distributed her treasures to widows, orphans, and the poor and so escaped the peril of death," to the Puritan such deathbed gestures of repentance were of little value.<sup>50</sup> "There is no Real Conversion in it," Cotton Mather argued, "Men are then only like Iron softened in the Fire; they soon Return to their former Hardness if God spare them from going down into the *Unquenchable Fire*."<sup>51</sup> At death the Puritan knew there was nothing he could do but wait, hope—and doubt.

It is hardly surprising, then, to read James Fitch, in the earliest New England funeral sermon we have, explaining the Puritan's frequent lack of "the sweetness of that unspeakable peace in his dying hour"—the inherited prescription for Christian deathbed behavior—as the result of the fear of "the misery of falling short, for none can be so sensible of that, as those who know experimentally the preciousness of Christ and heavenly things: and though also many times the very thought, what if deceived? what if fall short at last? that thought would make the flesh tremble."<sup>52</sup> Or, to cite once again the vivid language of Leonard Hoar, who after recognizing the historical tradition with such typical phrases as "the day of ones Death is better than the day of ones Birth," and rhetorically describing death as "a loosing from

<sup>48</sup> Jonathan Edwards, "The Eternity of Hell Torments," Sermon 11 in Edwards, *Works*, 4 (New York, 1843), 278.

<sup>49</sup> Charles Chauncy, *The New Creature Describ'd* (Boston, 1741), 20. For a thorough, if somewhat more pedestrian, summing up of the Puritan view of hell, see John Bunyan, *Sighs From Hell or Groans of a Damned Soul* (Boston, 1708).

<sup>50</sup> Quoted in Boase, *Death in the Middle Ages*, 124.

<sup>51</sup> Cotton Mather, *Thoughts of a Dying Man*, 40–41.

<sup>52</sup> James Fitch, *Peace the End of the Perfect and Upright* (Boston, 1672), 6.

this troublesome Shore," becomes much more specific, much truer to the Puritan vision:

So it may be said of every inhabitant of this earth when he comes to dye, the weight of sin, the unsupportableness of Gods anger, the terrors of hell, the nearness of the danger, the difficulty of salvation will all appear nakedly to the naked soul. When God makes darkness and it is night, then the beasts of the forest creep forth: every frog will be croaking towards the evening, every puddle will send up a stinking vapor in a foggy night; all the several shadows of things will unite, every [illegible] will concur to make up and compleat the misery of the poor sinner. . . . *Hence learn why men dread and are so afraid of death*, yes there are many causes why a natural man should fear Death, because of the sting that is in it.<sup>53</sup>

The acute awareness that man is both powerless to affect the matter of his salvation and morally crippled by his natural depravity caused, Cotton Mather writes, much distress and retrospective agonizing as death closed in. "Tis very certain," he notes in *Thoughts of a Dying Man*, "that at the Last, when you are taking your leave of this World, you will be full of Disdainful Expressions concerning it, and Express your selves to this purpose: *Vain World! False World! Oh! that I had minded this World Less, and my own Soul more, than I have done!*" Later in the same work Mather warns that when, on your death bed, you are provided with reminders of your past sinful life, "the sight of them will smite thee with more Horror, than if so many *Rattle Snakes* were then horribly crawling about thee." And still later:

Tis no rare Thing, for eminent *Saints*, when they lay a *Dying*, to profess, as we find in the History of their *Lives*, that some of them have done; *The Loss of Time, is a Thing, that now Sits heavy on this Poor Soul of mine!* . . . Men ordinarily Dy, with words, like those of that Great Person, Sir Henry Wotten, uttered with Tears, *How much Time have I to Repent of! and how Little Time to do it in!*<sup>54</sup>

The New England Puritans, despite their traditional, optimistic rhetoric, were possessed of an intense, overt fear of death—and for three very good and very rational reasons: their belief in their own utter and unalterable depravity, in the omnipotence and justness of God, and in the unspeakable terrors of hell. Unlike Bacon, they did not fear death merely "as children fear to go in the dark"; they feared it because they knew precisely what to expect from it. One cannot leaf through Increase Mather's *Remarkable Providences*, or Cotton Mather's *Wonders of the Invisible World*, or even the relatively pragmatic John Winthrop's *Journal*—in which, for example, he relates in great detail the birth of a monster to a heretic as an example of

<sup>53</sup> Hoar, *Sting of Death*, 10–11.

<sup>54</sup> Cotton Mather, *Thoughts of a Dying Man*, 9, 15–16, 27–28.

God's "instruction of the parents"<sup>55</sup>—without sensing the sober reality of the spiritual world to the mind of the devout Puritan. There was thus nothing extraordinary in their emotionally turbulent reaction to death; rather, given the essence of their belief, it was only the extraordinary, and possibly suspect, Saint who could face death calmly.

"THE POWER OF RELIGION depends, in the last resort," Peter L. Berger writes, "upon the credibility of the banners it puts in the hands of men as they stand before death, or more accurately, as they walk, inevitably, toward it."<sup>56</sup> If the Puritans experienced what may seem inordinate difficulty in facing death, it was not merely because of the fearful images of irresistible destiny conjured up in their collective mind's eye; nor, certainly, can the answer be found in the continued presence of the rhetoric of Christian optimism, of the *Migratio ad Dominum* theme, in their formal thoughts on the matter. The difficulty lay rather in reconciling these two contradictory "banners," which they carried simultaneously. As Berger's observation suggests, the discomfort they experienced was principally due to a lack of credibility in the answers they gave to questions concerning the proper way of viewing death and of experiencing dying. By clinging to the rhetorical tone and style (the ritual) of a Christianity equipped with a variety of mechanisms whereby man might affect his fate and secure his own salvation, a Christianity that had little application to their own deterministic concept of reality, the Puritans trapped themselves between conflicting belief systems—more specifically, in this case, between conflicting schemes of ethos and world view.

The functional effects of such conflicts have long interested theorists in the behavioral sciences. The concept of *anomie* or "normlessness," for example, has had a profound effect on modern sociology and psychology.<sup>57</sup> Although the subject of a great deal of controversy, the term in its simplest sense has generally been recognized as referring to the confusion and apprehension that may result from "a clash between belief-systems or, more precisely, a conflict between the *directives* of belief-systems."<sup>58</sup> Similarly, Leon Festinger's "cognitive dissonance" theory rests on the assumption that "the human organism tries to establish internal harmony, consistency, or congruity" in its cognitive structure; the loss of such consistency, Festinger argues, results in a state of psychological discomfort (or cognitive dissonance), the

<sup>55</sup> John Winthrop, *Journal* (New York, 1908), 1: 267–68.

<sup>56</sup> Peter L. Berger, *The Sacred Canopy: Elements of a Sociological Theory of Religion* (Garden City, N.Y., 1969), 51.

<sup>57</sup> For a concise discussion of both the sociological and psychological uses of the term, see Robert K. Merton, *Social Theory and Social Structure* (3d ed., enlarged; New York, 1968), 215–18.

<sup>58</sup> Sebastian De Grazia, *The Political Community: A Study of Anomie* (Chicago, 1948), 72.

intensity of which varies in relation to the importance of the incompatible and competing cognitions.<sup>59</sup>

It is not my intent here to try to pin either of these or any other such specific labels on the Puritans' handling of the problems of death and dying. Such an exercise would properly be subject to both philosophical and substantive criticisms that are beyond the scope of this article. Nevertheless, what appears to be at work here is a phenomenon so basic that it transcends disciplinary and chronological boundaries. Whatever label we may care to give it, virtually all cultures and individuals experience an almost constant, if generally slight, changing of structure or focus as adjustments are made to cognitive and conceptual conflicts. On those occasions when such accommodation does not take place, particularly in relatively closed cultures when the matter at hand is of primary cultural significance, the reverberations of the continuing conflict may be felt in the very roots of the body in question.

Death and dying were matters of critical importance to the Puritans; indeed they constantly urged themselves to direct their lives toward that moment when their earthly pilgrimage would end. At the same time, however, their concept of death and its attendant and deserved terrors was at odds with their inherited Christian advocacy of displaying what James Fitch called "the sweetness of that unspeakable peace" when death approached. The result was a kind of cultural dissonance, an uncomfortable tension, that pressed for resolution. A culture, no less than an individual, cannot long endure such pressure. One of the principal ways of reducing such tension, Festinger and others agree, is "by changing one or more of the elements involved in dissonant relations,"<sup>60</sup> and this, it now seems clear, is what happened in New England in the eighteenth century.

Precisely when such a change may have taken place remains a matter of conjecture. Indeed, as with most important changes in the structure of intellectual and cultural phenomena, it may well be impossible ever exactly to locate a watershed point. But by the latter half of the eighteenth century, when the emerging New England orthodoxy was of an increasingly liberal bent, there is little evidence that the anxiety-riddled tension between death and dying that so beset devout Puritans a century earlier remained an active force.

This is certainly not to suggest that the Puritans' successors ceased to fear death. They did fear it, and still do; but in ways very different from the Puritans.<sup>61</sup> Just as Christians in the Middle Ages and earlier fitted their fear

<sup>59</sup> Leon Festinger, *A Theory of Cognitive Dissonance* (Evanston, 1957), 260.

<sup>60</sup> *Ibid.*, 264.

<sup>61</sup> For a concise summary of recent thought on the contemporary relationship between death and religion, see Irving E. Alexander and Arthur M. Adlerstein, "Death and Religion," in Feifel, *Meaning of Death*, 271-83. Cf. many of the articles in Hendrik M. Ruitenbeek, ed., *Death: Interpretations* (New York, 1969), and a recent sweeping criticism of most such studies to date, Barbara Chasin, "Neglected Variables in the Study of Death Attitudes," *Sociological Quarterly*, 12 (1971): 107-13.



into a fairly harmonious system of belief, so have most post-Puritan Christians, with the result that the fear is at least tempered by theological rationalization which is tolerably consistent. The Puritans inherited part of that rationalization, the part that counsels a peaceful death for the regenerate, and it recurs like an unthinking, though obviously important slogan throughout their writing; but it was not consistent with their sense of reality. It did not "fit." And so, although the need for such comforting counsel was apparently present, the reassurance it should have afforded the regenerate individual was denied, both by the determinism of the faith and by the excruciatingly difficult balance that had to be maintained between assurance and security. To return to Peter Berger's imagery, there was little real confidence possible in the credibility of the religious banners the Puritans carried as they walked inevitably toward death. The result was a vision of death and an attitude toward dying that were locked in perpetual conflict, a conflict that brought extraordinary discomfort to bear on the life of the devout Puritan, a conflict that could not be indefinitely endured.

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# The *Encyclopédie* Wars of Prerevolutionary France

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ROBERT DARNTON

THE PUBLICATION OF THE *Encyclopédie* has long been recognized as a turning point of the Enlightenment. In permitting Diderot's text to appear in print the state, however reluctantly and imperfectly, gave the philosophes an opportunity to try their wares in the market place of ideas. But what was the result of this break-through in the traditional restraints on the printed word in France? By concentrating on the duel between the *encyclopédistes* and the French authorities, scholars have told only half the story. The other half concerns some basic questions in the social history of ideas: how did publishers plan and execute editions in the eighteenth century? How well did works like the *Encyclopédie* sell? And who bought them? This essay is addressed to those questions. By recounting the life cycle of one book, it is intended to suggest some of the possibilities in the history of publishing, a field that has lain fallow too long despite its attractive location at the crossroads of intellectual, social, economic, and political history.<sup>1</sup>

<sup>1</sup> This essay, which is intended as a preliminary sketch for a full-length study of the quarto editions of the *Encyclopédie*, is based almost entirely on the papers of the Société Typographique de Neuchâtel (hereafter STN) in the Bibliothèque de la Ville de Neuchâtel, Switzerland. All citations are to those manuscripts unless specified otherwise. Any researcher concerned with the later editions of the *Encyclopédie* is bound to feel indebted to the painstaking scholarship of two men: George B. Watts and John Lough. See especially Watts's articles, "Forgotten Folio Editions of the *Encyclopédie*," *French Review*, 27 (1953-54): 22-29, 243-44; "The Swiss Editions of the *Encyclopédie*," *Harvard Library Bulletin*, 9 (1955): 213-35; and "The Genevan Folio Reprinting of the *Encyclopédie*," *Proceedings of the American Philosophical Society*, 105 (1961): 361-67; and see Lough's book, *Essays on the Encyclopédie of Diderot and D'Alembert* (London, 1968). As far as the circulation of books within France is concerned the first edition of the *Encyclopédie* was relatively unimportant, but it has attracted most of the attention of scholars because its publication became the crucial episode in the liberalization of the Direction de la Librairie and in the battles between the philosophes and their opponents during the 1750s. A decree of the king's council suppressed the first two volumes of the *Encyclopédie* in 1752; and the council revoked the privilege for the book in 1759, when it had come under attack by the pope, the Jesuits, the Jansenists, the Parlement of Paris, and other enemies of the philosophes. But C. G. de Lamoignon de Malesherbes, the enlightened director of the Librairie, unofficially permitted the last ten volumes of text to appear in 1765. The last two volumes of plates were published in 1772. For an excellent synthesis of the scholarship on this aspect of the history of the *Encyclopédie*, see Arthur M. Wilson, *Diderot* (New York, 1957, 1972), and Jacques Proust, *Diderot et l'Encyclopédie* (Paris, 1967).

WHEN DIDEROT AND HIS PUBLISHERS brought out the last volume of the *Encyclopédie* in 1772, they had won more than a moral victory over the system for controlling French publishing. The first edition probably produced about 2,500,000 livres in gross profits. But the government refused to let the book sell openly, and most of the 4,225 sets went to customers outside France.<sup>2</sup> The second edition also seems to have been primarily a non-French affair. It was a folio reprint of the original text, produced in Geneva by a consortium of publishers allied with Charles Joseph Panckoucke of Paris. Its sales records have not survived, but its publishers originally hoped to market half of their 2,200 sets in France; and they had sold 1,330 sets throughout Europe when they settled their accounts in June 1775.<sup>3</sup> So by that date only 3,000 copies of the first two editions, at the very most, existed in France. The country had not been inundated with *Encyclopédies*, despite the semi-legal status granted to the book.

But the publishing of the next editions—the three quarto and the two octavo printings of the original text—is a very different story; and unlike the publishing history of the first two editions, it can be told in detail, thanks to the papers of the Société Typographique de Neuchâtel in Neuchâtel, Switzerland. The story begins with Panckoucke, the extraordinary entrepreneur known as “the Atlas of the book trade,”<sup>4</sup> and his system of alliances and alignments within the world of publishing and politics.

In December 1768 Panckoucke bought from the original publishers the plates of the *Encyclopédie* and the rights to future editions of it. Precisely what these rights were is difficult to say. Panckoucke used the terms “droits” and “privilège” throughout his correspondence, but the government had revoked the formal privilege of the *Encyclopédie* in 1759, and the registers of privileges in the Bibliothèque Nationale give no indication that it was ever restored. They do reveal that Panckoucke received a twelve-year *privilège général* on March 29, 1776, for a “Recueil des planches sur les sciences, arts et métiers,” which may have been enough to substantiate his claim to possess a kind of copyright.<sup>5</sup> In any case, he asserted that claim in the most abso-

<sup>2</sup> On the economic aspects of the first edition, see, in addition to the works cited above, Norman L. Torrey, “L’*Encyclopédie* de Diderot, une grande aventure dans le domaine de l’édition,” *Revue d’histoire littéraire de la France*, 51 (1951): 306–17; John Lough, “Luneau de Boisjermmain v. the Publishers of the *Encyclopédie*,” *Studies on Voltaire and the Eighteenth Century*, 23 (1963): 115–73; and Ralph H. Bowen, “The *Encyclopédie* As a Business Venture” in Charles K. Warner, ed., *From the Ancien Régime to the Popular Front: Essays in the History of Modern France in Honor of Shepard B. Clough* (New York, 1969), 1–22. What little is known about the sales of the first edition comes from the papers of Luneau de Boisjermmain, which are too polemical to be trustworthy and which justify estimates of the foreign sales ranging from somewhat more than one-half to three-quarters of the edition.

<sup>3</sup> Lough, *Essays on the Encyclopédie*, 103.

<sup>4</sup> George B. Watts, “Charles Joseph Panckoucke, ‘l’Atlas de la librairie française,’” *Studies on Voltaire and the Eighteenth Century*, 68 (1969): 67–205.

<sup>5</sup> *Privilège* no. 613, Mar. 29, 1776, Bibliothèque Nationale, fonds français, MS 21967. On February 10, 1776, Panckoucke had received a twelve-year *privilège général* (*privilège* no. 365, *ibid.*) for “un ouvrage qui a pour titre *Nouveau dictionnaire des arts et des sciences etc.*,” but nothing proves that this work was the *Encyclopédie* or had any connection with it. None of

lute manner, citing not only the contract by which he bought out the original publishers but also the sanction of the French government; and he sold portions of his "privilege" to a whole series of partners, periodically buying them back and reselling them again to new associates for new editions.

Panckoucke's first *Encyclopédie* was the second edition, the folio reprint of 1771–76. Those were hard years in the book trade, owing to the repressive measures of the "triumvirate" ministry of Maupeou, Terray, and d'Aiguillon, so Panckoucke had the edition printed in Geneva by his partners, who included Voltaire's publisher, "the angel Gabriel" Cramer. It was a stormy affair, involving quarrels among the associates, conflict with a rival, a "Protestant" *Encyclopédie* being produced by Barthélemy de Félice in Yverdon, and a losing battle with the French government, which had confiscated six thousand volumes that Panckoucke had originally printed in Paris. Whether Panckoucke ever had much success in cracking the French market with this edition cannot be known, but his difficulties did not discourage him. By the accession of Louis XVI he remained convinced that there was still a fortune to be made in *Encyclopédies*, and the liberal character of the new ministry swelled his hopes. He found doors opening for him everywhere within the government. His coach carried him into Versailles "like an official with a portfolio."<sup>6</sup> And his letters burgeoned with assurances of "protections" from lieutenants of police, directors of the book trade, and ministers.

On July 3, 1776, Panckoucke sold an interest of fifty per cent in his newly consolidated "rights and privileges" in the *Encyclopédie* for 143,000 livres to the Société Typographique de Neuchâtel, one of the most important publishers of French books during the twenty years before the Revolution. After toying with a plan to publish another folio reprint, this new association decided to produce a completely revised edition. The text was to be rewritten by a whole stable of philosophes—including Marmontel, Morellet, La Harpe, D'Arnaud, St. Lambert, and Thomas—under the direction of Suard, with D'Alembert and Condorcet as associates. Panckoucke did not enlist Diderot, "*une mauvaise tête*, who demanded 100,000 écus and would have driven us to despair."<sup>7</sup> But he counted heavily on D'Alembert, who was to solicit the protection of Frederick II and perhaps even to persuade him to accept the dedication of the new work. D'Alembert also considered writing a history of the *Encyclopédie* for the new edition, but that essay died

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the other registers of *privilèges* or *permissions tacites* contain any references to the *Encyclopédie* for the period 1768–88: see MSS 21964–67, 21983, 21984, 21989, 22000–02, 22013, and 22073. On September 8, 1759, after the revocation of the original privilege of the *Encyclopédie*, its first publishers received a privilege to produce a collection of its plates, which they probably construed as a "copyright" for the entire work and sold to Panckoucke.

<sup>6</sup> D.-J. Garat, *Mémoires historiques sur la vie de M. Suard, sur ses écrits et sur le XVIII<sup>e</sup> siècle* (Paris, 1820), 1: 274. For more details about the second edition, see Lough, *Essays on the Encyclopédie*, 52–111.

<sup>7</sup> Panckoucke to STN, Aug. 4, 1776. In this letter Panckoucke explained that he was referring to an interview with Diderot that had occurred eight years earlier. For Diderot's version of this famous encounter, see Wilson, *Diderot*, 578–79.

stillborn, like other potential classics of the Enlightenment—a history of French Protestantism by Raynal, a history of Turgot's ministry by Voltaire—that never got beyond the stage of projects knocked about in negotiations between authors and publishers. In the end this new *Encyclopédie* itself miscarried, despite the grandiose plans of its backers, because it was undercut by a quarto edition of the original text, which was launched in 1776 by Joseph Duplain of Lyons, the antihero of this story and one of the most intrepid buccaneers in the era of “booty capitalism.”

Like many provincial bookdealers Duplain built his business on the demand for cheap, pirated works, often of a racy or philosophical character, which were produced in the printing houses flourishing beyond the fringes of France's borders, thanks to the system of privileges and thought control that stifled innovative publishing within the kingdom. Duplain smelled a fortune in cut-rate *Encyclopédies*. He announced the opening of a subscription for a cheap quarto edition, which would incorporate the five-volume supplement in the original text. He protected himself by attributing the edition to Jean Léonard Pellet, a Genevan printer who received three thousand livres for acting as straw man. And when the flow of subscriptions proved strong enough, Duplain contracted the printing to several Genevan shops, keeping the financial and administrative work to himself. He counted on getting the books into France either by smuggling—he had great influence in the booksellers' guild of Lyons, although he had powerful enemies in the Parisian guild—or by winning the benevolent neutrality of the French authorities. But he had not reckoned with Panckoucke.

Panckoucke could choose either to beat Duplain or to join him. The first alternative appealed to Panckoucke because he was convinced that he could use his protections effectively enough to block the channels of the underground book trade. But the success of the subscription created a greater temptation. Panckoucke knew “every step that Duplain takes,”<sup>8</sup> thanks to secret reports from an allied Lyonnais bookseller called Gabriel Regnault. Regnault learned that the subscription was selling spectacularly, and corroborative information “from everywhere”<sup>9</sup> made it look as though the quarto *Encyclopédie* could turn into the most profitable publication of the century. So Panckoucke shelved the project for the revised edition and entered into negotiations, bartering his monopoly on legality against a cut of the subscriptions. On January 14, 1777, he and Duplain signed what later became known as the “Treaty of Dijon.” Each took a half interest in the quarto enterprise, which they subsequently divided among their own associates (the Société Typographique de Neuchâtel eventually came to own five twenty-fourths of the entire enterprise). Duplain committed himself to administer the production, distribution, and financing of the edition accord-

<sup>8</sup> Panckoucke to STN, Dec. 26, 1776.

<sup>9</sup> Panckoucke to STN, May 13, 1777.

ing to conditions specified in great detail by the contract. And Panckoucke promised to supply half the capital, the three volumes of plates, and the covering protection of his privilege. The last item was no small advantage. In August 1777 Panckoucke wrote that Le Camus de Neville, the director of the Librairie, "will protect our great affair" and had even given permission for Panckoucke to import the books directly to his warehouses in Paris, bypassing the customs, the booksellers' guild, and the censorship.<sup>10</sup> At the same time, writing as if he were himself a minister, Panckoucke directed the inspector of books in Lyons to give clear passage to the crates being shipped from Switzerland.<sup>11</sup> In fact Panckoucke pulled strings so effectively that the Swiss printers began to stuff their shipments of *Encyclopédies* with prohibited books. Far from drawing the fire of the established authorities, as it had done in the 1750s, the *Encyclopédie* circulated under the protective covering of their patronage; and that protection served as camouflage for the diffusion of works that the state wanted to suppress.

Panckoucke and Duplain had no idea that a small smuggling operation had grafted itself onto their enterprise. They gave all their attention to the maximization of profits, and the quarto proved to be extraordinarily profitable: orders poured in from everywhere, traveling salesmen reaped unheard-of harvests, and booksellers marveled at a hunger for the *Encyclopédie* that had remained dormant among clients who had not been able to buy the folio editions. "There is no other work so universally widespread," wrote Dufour of Maestricht. "Our streets are paved with it," said Resplandy of Toulouse, echoing exactly the observation of a Lyonnais salesman: "Our town is paved with it." And Panckoucke exulted, "The success of this quarto edition passes all belief."<sup>12</sup> In opening the subscription Duplain had set his sights high: he hoped to sell 4,000 copies. The subscription filled to overflowing with astonishing speed; so Duplain opened another, for 2,000 more copies. It, too, filled rapidly, and Duplain opened a third, making a total of 8,000 sets of thirty-nine quarto volumes each—an extraordinary amount for an era when printings of single-volume works normally ran to 1,000 copies or so.

This succession of subscriptions explains the mystery of the missing second quarto edition, which has plagued bibliographers who have been able to locate only the first, or "Pellet," edition and the third, or "Neuchâtel," edition of the quarto *Encyclopédie*.<sup>13</sup> Duplain committed himself to print

<sup>10</sup> Panckoucke to STN, Aug. 5, 1777.

<sup>11</sup> "Je vous serai obligé de donner vos ordres pour que ces volumes passent sans difficulté et d'accorder toute votre protection à cet ouvrage. M. de Neville est prévenu de tout ce que j'ai fait à ce sujet." Panckoucke to La Tourette, July 18, 1777, Bibliothèque publique et universitaire de Genève, MS supp. 148.

<sup>12</sup> Dufour to STN, Aug. 2, 1780; Resplandy to STN, Jan. 2, 1778; D'Arnal to STN, Nov. 12, 1779; Panckoucke to STN, Sept. 9, 1777.

<sup>13</sup> George B. Watts made a good guess as to the explanation of the "second" edition, but he mistakenly believed that Pellet directed the whole affair: see "Swiss Editions of the *Encyclo-*

the second subscription when the printers had reached sheet "T" of volume 6, working at a press run of 4,000 copies. He directed them to reprint 2,000 copies of everything they had completed and then to continue at a run of 6,000. So there was no distinct second edition. The third subscription coincided with a separate "third edition," because each sheet was reset and run off at 2,000 copies, and the title page of each volume proclaimed it to be "troisième édition, à Neuchâtel, chez la Société typographique." In fact this imprint was a ruse devised by Duplain to inveigle subscriptions from persons who had been put off by the slipshod quality of the Pellet editions. The Société Typographique actually printed only one volume of "its" edition and four of the volumes that appeared under Pellet's name. In every case Duplain subcontracted the printing and remained hidden behind his typographical false fronts.

Duplain used printers in Neuchâtel, Geneva, Lyons, Trévoux, and Grenoble, putting more than forty presses at work to turn out about 300,000 volumes. To produce and distribute books on such a scale required assembling one of the largest operations in premodern printing and strained resources throughout the publishing industry. For two and a half years the *Encyclopédie* dominated printing in the region around Lyons. "Except for a few liturgical works, nothing else is being printed here, in all the shops, only the *Encyclopédie*," an agent reported in 1778.<sup>14</sup> The Société Typographique took five months, using about half the capacity of its twelve presses and its work force of about thirty-five men, to print a press run of 6,000 copies of one of the huge, double-column tomes. Financing 8,000 copies of thirty-six such volumes required so much capital that Panckoucke and Duplain fell back on consortia of French and Swiss bankers, and the same agent in Lyons observed, "Whoever had a little money to put into books every month or every year has placed it on the *Encyclopédie* quarto."<sup>15</sup> The *Encyclopédie* consumed so much paper that in December 1777 a buyer for the Société Typographique could not find a single sheet of the requisite kind in Lyons. The Société managed to continue printing only by sending paper scouts throughout France and western Switzerland in search of every last ream of *fin*, twenty-pound (Lyonnais measure) *carré* or *raisin*. Founders could not supply type rapidly enough to satisfy the demand (the quarto was printed, appropriately, in a type called "Philosophie"), and so some Genevan printers failed to begin work on schedule in 1777. The Neuchâtelois had to suspend printing at a crucial moment because they received a barrel of bad ink, and the inkmaker, a Parisian called Langlois who had a stranglehold on the quality-ink trade, kept inching up his prices, while lamenting

*pédie*," 228. Lough agrees with Watts's version of this bibliographical imbroglio: *Essays on the Encyclopédie*, 36-38.

<sup>14</sup> Jean-François Favarger to STN, July 21, 1778.

<sup>15</sup> *Ibid.*

about his own increased costs, which he attributed to poor olive harvests in the Midi. Wagoners also took advantage of increased orders to force up their rates. And the *Encyclopédie* produced chaos in the labor market of printing. Not only did the printers have to send hundreds of miles for workers, but the supply was so scarce that they took to raiding each other's shops through the use of industrial spies like Louis Marcinhes, a down-and-out watchmaker in Geneva, who wrote to the Société Typographique in July 1777.

Pellet and Bassompierre have by inflated promises seduced many workers and drained off the printing shops of the surrounding area. But they only want to pay them 15 florins 9 sols of our money per sheet. So a good number want to leave, because they are asking for 17 florins per sheet. The man leaving this week [for Neuchâtel] is one of those. He is called Caisle. Two pressmen, who have promised to come talk with me, also should leave. . . . I won't lose sight of any occasion to send to you the discontented from the shops of Pellet, Bassompierre, and Nouffer.<sup>16</sup>

In short, the quarto *Encyclopédie* sent repercussions into the remotest sectors of the economy. For it to come into being a whole world had to be set in motion: ragpickers, olive growers, financiers, and philosophers collaborated to create a work whose corporeal existence corresponded to its intellectual message. As a physical object and as a vehicle of thought, the *Encyclopédie* synthesized a thousand sciences, arts, and crafts; it represented the Enlightenment, body and soul.

Its publishers probably spent too much time calculating costs and profits to entertain such lofty thoughts. The Société Typographique estimated the total revenue of the enterprise at 2,454,092 livres, the total cost at 1,117,354 livres, and the gross profit at 1,336,738 livres: a return of one hundred twenty per cent on expenditures. No wonder they considered this affair "the most beautiful ever to be done in publishing,"<sup>17</sup> or that it touched off a series of fierce commercial wars.

Duplain, who had originally floated the quarto as a privateering venture, had no way, once he turned legitimate, of burying his treasure. Other pirates got wind of it and raced to the attack. First came announcements of rival counterfeit editions from Geneva and Avignon. Panckoucke read them as bluffs and counseled his associates to ride them out, since "I have arranged everything here in such a manner that none of those editions can enter France, and without France no success."<sup>18</sup> He was right: the announcements were a way of holding the quarto publishers up for ransom by threatening to undersell them unless they paid a certain sum in protection money.

<sup>16</sup> Louis Marcinhes to STN, July 11, 1777.

<sup>17</sup> STN to Panckoucke, Aug. 20, 1778. Because the accounts became extremely embroiled, it is impossible to know the exact costs and profit of the enterprise.

<sup>18</sup> Panckoucke to STN, Sept. 9, 1777.



The danger in this game was that one could not distinguish between a fake and a real attack until he saw the whites of his assailant's eyes. After the quartos of Geneva and Avignon had disappeared over the horizon, J. S. Grabit and J. M. Barret of Lyons announced plans to publish another quarto *Encyclopédie*, and they proved that they meant business by actually printing a few volumes. In this case Duplain and Panckoucke agreed that it would be wiser to capitulate. They bought out Grabit and Barret for 27,000 livres—the rough equivalent of a lifetime's wages for one of their printers—and received in return only a legalized promise to abstain from further counterfeiting. Then they learned that a consortium of publishers in Lausanne and Bern planned to produce an even smaller, even cheaper *Encyclopédie*, an octavo edition that would sell for approximately 200 livres. This time Duplain and Panckoucke decided to stand and fight.

At first the quarto publishers hoped that the octavo venture would simply collapse. They joked that the small type of "cette miniature" would blind its readers, and Panckoucke proclaimed "that octavo edition may cause some alarm, but it won't hurt us. . . . It is folly to print the *Encyclopédie* in such a small text. Moreover, we will be defended here. I am waiting for the magistrate [Le Camus de Neville] to return so that I can reveal everything to him. I promise you firmly that that *Encyclopédie* will never enter France."<sup>19</sup> The Société Typographique replied, "You hold the keys to the kingdom."<sup>19</sup> But reports from provincial booksellers indicated that the octavo subscriptions were selling as spectacularly as the quarto had done. So the quarto group began pourparlers—not with any serious intention of making peace but rather to delay the execution of the octavo until the quarto could be completed and the new, revised edition announced, thereby stealing the octavo market. The publishers of Lausanne and Bern, who were veterans of pirate publishing, detected this strategy after a few rounds of negotiation and resolved to proceed with their printing. Duplain then attempted to overwhelm them with a frontal assault: he published an announcement that the quarto group would produce its own octavo edition at an even cheaper price than the octavo of Lausanne and Bern. On November 1, 1777, Lausanne and Bern retaliated with an ultimatum: withdraw your announcement within fifteen days, or we will drop the price of our octavo to the level of yours, and we will undermine your quarto by producing a still cheaper quarto of our own.

You will have to give in to us or lower your own price. In this way we will cut each other's throats, but you have set the example and are forcing this necessity upon us. And don't think that this is an idle threat. The prospectuses are ready,

<sup>19</sup> STN to Panckoucke, Dec. 18, 1777; Panckoucke to STN, Nov. 19, 1777; STN to Panckoucke, Dec. 7, 1777.

and we have the same type, the necessary presses etc. at our disposition in Yverdon.<sup>20</sup>

This maneuver forced Duplain to retreat, but it also resulted in open war; for although negotiations continued intermittently—the usual style in eighteenth-century warfare—each side campaigned fiercely, attempting to destroy the other's market.

The octavo group relied on a strategy of smuggling. They filled their subscription and counted on reaching their clients through the underground circuits of the clandestine book trade. The quarto group calculated on blocking those circuits. Panckoucke promised his partners, "I guarantee that they will not penetrate France. The magistrate promised me so. . . . You understand, Messieurs, that being armed with a privilege, you should not concede your rights any more than I. Because of our contracts, our privilege, Duplain had to come make terms with us. The Lausannois will have to do the same."<sup>21</sup> The system of privilege and protection that had nearly destroyed the first edition of the *Encyclopédie* was being used as the main line of defense in the effort to save its successor. So much had conditions changed from the reign of Louis XV to that of Louis XVI that the government treated *encyclopédisme* more as a commercial than an ideological matter. This new attitude suggests that enlightened ideas permeated the government itself, but it does not necessarily imply weakness; in fact the contest between the strategy of smuggling and the strategy of policing provides a test case of the government's ability to control the printed word.

In mid-1778 the Société Typographique de Neuchâtel sent an agent, Jean-François Favarger, on a tour of southern and central France. Favarger's first assignment was to check the society's supply lines along the French-Swiss border. In Saint Sulpice, the last town on the Swiss side of the border, he learned that the smuggling outfit of Meuron Frères had recently taken care of five 500-pound crates containing volume 1 of the octavo *Encyclopédie*. The Meuron brothers told him so themselves, with more than a hint of professional pride, because they handled the society's own smuggling but only as occasional substitutes for Pion of Pontarlier, the society's first-string smuggler, whom they wanted to replace. On the other side of the border, in Pontarlier, Pion told Favarger that he had seen five *acquits à caution*—a customs permit used by the French state to control imports of foreign books—that had been fraudulently discharged by Capel, syndic of the booksellers' guild in Dijon. Since Capel was officially required to confiscate the books that he forwarded, Dijon now promised to surpass Besançon as the main entrepôt of this underground route, as Favarger announced triumphantly in notifying his employers that the octavo had passed from Bern to Saint Sulpice to

<sup>20</sup> Société Typographique de Lausanne to Pellet of Geneva, Nov. 1, 1777, copy included in a letter from the Société Typographique de Lausanne to STN, Nov. 20, 1777.

<sup>21</sup> Panckoucke to STN, Dec. 22, 1777.

Dijon and was now headed toward Paris. The Société Typographique hid Capel's name in the hope that "for money he will provide us with the same service"<sup>22</sup> and relayed the rest of the information to Panckoucke, who alerted the French authorities, who eventually captured the crates. The authorities engineered other confiscations on their own—in Toulouse, for example, where a big bust inflicted huge casualties on the octavo group. By August Favarger's field reports showed that subscribers were deserting the octavo in droves throughout the Midi. And in early 1779 the octavo publishers sued for peace.

The negotiations dragged on for a year, while the quarto group finished a mopping-up operation in France and the octavo group tried to repair its losses through sales in Central and Eastern Europe. Finally in February 1780 Panckoucke sold the entry into France to the Lausanne-Bern consortium for 24,000 livres. That was a steep price—roughly eight per cent of the octavo's current manufacturing cost—and it shows how strong the demand for *Encyclopédies* remained, at least in the calculations of publishers who had discovered a new, undernourished public. Thinking they were safe at last, the octavo group increased their printing to 6,000 copies—hence the explanation of another "missing" edition<sup>23</sup>—and promptly fell into another of Panckoucke's traps. Because they had not been able to pay off Panckoucke in cash, they had persuaded him to accept his ransom in kind—that is, in 24,000-livres' worth of octavo *Encyclopédies*. Panckoucke dumped his octavos on the French market at a reduced price and then compounded the damage to the octavo group's future sales by spreading the word that he would soon produce an *Encyclopédie* to end all *Encyclopédies*—not the revised edition that he had originally planned with the Société Typographique de Neuchâtel, but the *Encyclopédie méthodique*, which he was then organizing with the support of a consortium from Liège. That was not the last low blow in this battle, because four years later the old members of the octavo group joined by none other than Panckoucke's former ally, the Société Typographique, announced a plan to pirate the *Encyclopédie méthodique*.<sup>24</sup> It did not get far beyond the drawing board, however; so the

<sup>22</sup> Favarger to STN, July 8, 1778; STN to Favarger, July 11, 1778.

<sup>23</sup> In the case of this second bibliographical mystery Watts ("Swiss Editions of the *Encyclopédie*," 230–32) and Lough (*Essays on the Encyclopédie*, 40–41) differ slightly, but each did a good job of guessing at the solution, considering that they did not know who was behind the quarto enterprise or how it came into conflict with the octavo edition.

<sup>24</sup> By this time the quarto association had been dissolved and the STN had split with Panckoucke and had formed the Confédération Helvétique with the sociétés typographiques of Bern and Lausanne. The plan of the Confédération Helvétique was to reprint all the articles of the *Encyclopédie méthodique* that had not appeared in the earlier editions, to arrange them in alphabetical order—rather than according to subject, as they appeared in the *Encyclopédie méthodique*—and then to sell them, in all three formats, as supplements to the early editions. In this way the owners of the first sets could acquire all the new material of the *Encyclopédie méthodique* by purchasing only a few volumes of supplements, and the Swiss printers could badly damage Panckoucke's potential market.

quarto-octavo war may be said to have ended in the defeat of Lausanne and Bern.

The publishing wars did not cut off the supply of relatively inexpensive *Encyclopédies* to France. On the contrary they show how fiercely publishers struggled to satisfy the French market and how important that market must have been. They also illustrate the aggressive, entrepreneurial character of Enlightenment publishing in contrast to the conservative publishing industry that was dominated by the guild structure within France.<sup>25</sup> And finally they expose the inadequacy of the common view that the Enlightenment and the regime were locked into a fight to the death; for the quarto group captured the market by enlisting the state on its side—a strategy of protection and privilege that typifies the ways of the Old Regime and that also suggests a shift in the tone of government in the mid-1770s. The book that had barely survived persecution under Louis XV became a best seller under Louis XVI—with the blessing of the government.

THE LAST EPISODE in the *Encyclopédie* wars was purely domestic, a civil war between Duplain and his associates. In February 1779 they met in Lyons to assess their affairs. Contrary to all expectations, Duplain gave a pessimistic account of the sales. The first two subscriptions had done splendidly, he explained, but that very success had tempted the associates to over-extend themselves, and the third edition now looked like a disaster. They might rescue it, however, if they divided up one thousand unsold sets so that each associate could market them in areas where his sales were normally strongest. Panckoucke accepted this proposal, because the Parisian territory was reserved for him and, anyway, he would allot almost half of his five hundred sets to the Société Typographique. Six months later, in a still gloomier report, Duplain warned that this maneuver had not sufficed to save the third edition. Hundreds of volumes would rot in their warehouses unless they took drastic measures. Fortunately Duplain had found a merchant, a certain Perrin, who had caught the *Encyclopédie* fever, and they could dump their unsold copies on him. To be sure, Perrin demanded extraordinary terms—a fifty-per-cent reduction—but they would be lucky to get rid of their excess stock at any price, and Perrin would take a huge number: 422 sets, as well as 160 from Panckoucke's share of the thousand that had been split between him and Duplain in February. Panckoucke accepted the proposal, but soon after signing the Perrin contract he began to harbor suspicions. He learned that Duplain had tried to involve a mutual friend in a secret conspiracy to raid his reserved quarto market in Paris, and he found that Duplain's letters sounded disturbingly vague about Perrin,

<sup>25</sup> For further information on these contrasting types of publishing, see Robert Darnton, "Reading, Writing and Publishing in Eighteenth-Century France: A Case Study in the Sociology of Literature," *Daedalus*, Winter 1971, pp. 214-56.

whom they described as "a commercial agent in Strasbourg, who has a business in Lyons, or rather, I believe, in Paris, anyhow an extremely rich man for whom I can reply."<sup>26</sup> By September 1779 Panckoucke confided to the Société Typographique, "I am quite persuaded that this Perrin is only an imaginary being or, at most, a straw man. Duplain is avaricious and makes no pretense about being delicate."<sup>27</sup> He had become convinced that Duplain was "a vile soul," "a voracious man, who loves money with a fury"; "his rapacity has no limits."<sup>28</sup> And he advised the Société Typographique to slip a spy into Duplain's shop. They needed no prompting, for they had done so long ago. In fact all the associates spied on each other. Panckoucke had his own man watching Duplain; the Neuchâtelois received secret reports on Panckoucke; they kept an agent in Geneva; and their man in Lyons spun such a web of industrial espionage that they finally trapped Duplain in February 1780.

The Lyonnais network managed to track down the elusive Perrin, who indeed turned out to be a straw man in Duplain's pay, and then it made an even bigger catch: it got hold of a copy of a secret subscription list, Duplain's record of the actual number of *Encyclopédie* sales. The list made no reference to the Perrin sale; instead it contained 978 more subscriptions than Duplain was later to report at the final settling of accounts in February 1780. The Société Typographique suspected the fraud before this meeting and verified it, once Duplain made his report, by writing to the booksellers whose subscriptions had been falsified, according to a comparison of the reported subscriptions and the secret list. So it discovered that the flow of orders never had dried up, as Duplain had claimed. On the contrary, the entire third edition had been sold at the normal price, except for the five hundred sets that Duplain had dumped on Panckoucke. Duplain had hidden the sales in order to collect the full amount from them, while paying nothing for five hundred of the *Encyclopédies* that he sold and paying for the rest at half price through the phony intermediary of Perrin.

Instead of contenting himself with this spectacular double swindle, a matter of more than 200,000 livres, Duplain piled fraud on fraud in combinations too complex to be fully explained here. His role as general administrator of the enterprise offered enormous opportunity for speculation, because the quarto association allotted him set amounts for all his expenses. He therefore contracted the printing to the lowest bidder, pocketing the difference between what he was allotted and what he paid. He also cheated on the costs of paper and transport and even collaborated in a technique of fraudulent spacing and paragraphing worked out by a Genevan printer—an item that might have seemed trivial to a lesser embezzler but that ex-

<sup>26</sup> Panckoucke to STN, Sept. 10, 1779, citing a letter he had received from Duplain.

<sup>27</sup> Panckoucke to STN, Sept. 27, 1779.

<sup>28</sup> Panckoucke to STN, Nov. 6, 1778, Mar. 7, 18, 1779.

panded volume 19 by 96 unnecessary pages, worth 744 livres. Panckoucke and the Société Typographique calculated that Duplain's kickbacks and rake-offs came to 127,000 livres, but that was only an estimate, one that probably did not do justice to his genius. His intentionally unintelligible accounts could have concealed far more speculation, because they scrambled more than three million livres of expenses and revenues, and Duplain seems to have cheated at every possible point. For example, he attributed 494 subscriptions to the Lyons firm of Audambron and Jossinet at the usual reduced price for booksellers of 294 livres plus one free set for every twelve subscribed, which brought their total up to 535 subscriptions. The anti-Duplain network discovered that Audambron and Jossinet operated as a false front to hide the fact that Duplain had sold all 535 sets at the full subscription price of 384 livres, thereby robbing the association of 60,204 livres.

Since the quarto enterprise had been conducted like a conspiracy from the beginning, it exploded in the end like the denouement of a *drame bourgeois*—or an “English cockfight,” as the Société Typographique put it.<sup>29</sup> The anti-Duplain forces had concealed their suspicions while they accumulated enough ammunition to destroy Duplain at the final meeting for the settling of accounts at Lyons in February 1780. This strategy of counterdissimulation had not been easy, as the Société Typographique confessed to Panckoucke: “You have wisely counseled us to dissimulate with him until the very end and not to reveal our just discontent, but by devil it gets more and more difficult every day.”<sup>30</sup> When the showdown came, therefore, Duplain's associates surprised him with a barrage of accusations that they had been preparing for almost a year. They produced a correct version of the accounts, exposing a spectacular string of embezzlements. They unveiled the Perrin affair; they stripped the camouflage from Audambron and Jossinet; and they produced the secret subscription list with letters from booksellers testifying to the enormity of the swindles in sales. Even then Duplain refused to break down and confess. So they raided his office with a police commissioner, an attorney, and a bailiff, demanding confiscation of his papers; and they turned his family and friends against him, threatening to ruin the family's name by revealing the entire affair to the public. Finally Duplain surrendered. He agreed to compensate his partners with 200,000 livres, if they would sweep everything under the rug, where it has remained until today.

What sort of a man was this Duplain? The question has a certain fascination, both for economic history and for the history of the human soul. Duplain was a robber baron of the book trade, a gambler who played off high risks against high profits and who made a business of Enlightenment. He decided to stake everything on the quarto *Encyclopédie*. He sold his shop,

<sup>29</sup> F.-S. Ostervald and Abram Bosset DeLuze, codirectors of the STN, writing to the STN from Lyons, Jan. 29, 1780.

<sup>30</sup> STN to Panckoucke, Mar. 14, 1779.

his stock of books, his house, and his furniture and moved into a furnished room in order to concentrate exclusively on the great affair. Then he hit the jack pot; for this supreme gamble made him a rich man, even after the settlement of 200,000 livres. And once he knew he was wealthy, Duplain began to buy. First he acquired a wife, a beautiful young Lyonnaise who dazzled Panckoucke; then an estate in the provinces; finally the office of *maître d'hôtel du Roi*—that is, nobility. He began signing his letters “de St. Albine.” He served the king for the requisite time in Versailles and lived with his bride in offensive luxury in Paris before carrying her off to his château.

What is the moral of this story? It is a Balzacian drama: the tale of a bourgeois entrepreneur who clawed his way to the top and then consumed his fortune conspicuously, in aristocratic abandon. It is a saga of fortunes made and *illusions perdues* in publishing. In a way it is the story of French capitalism. And its supreme irony is that the vehicle for Duplain's rise into France's archaic hierarchy, only a few years away from destruction, was Diderot's *Encyclopédie*. Perhaps Duplain's story may also serve as a warning against placing too much confidence in sociological analysis of the sort that follows; for even if you can put a man perfectly in some socioeconomic category, his heart may be elsewhere. Duplain, the perfect bourgeois capitalist, turns out to be a pseudonoble—or was pseudonobility the essence of the French bourgeoisie?

THE INSIDE STORY of the warfare among the men who produced the *Encyclopédie* may reveal something of the spirit of entrepreneurial capitalism in early modern France, but it does not answer the larger question of what the battles were all about. Of course “booty capitalism” was waged for booty. Panckoucke and the pirates, Duplain and the Swiss, and their supporting cast of financiers, smugglers, and traveling salesmen all realized that they could make a fortune by satisfying the vast market in France for a “popular” edition of the supreme work of the Enlightenment. The ferocity of the competition to supply that demand suggests that the interest in enlightened ideas had spread very widely throughout France—to a *grand public* if not a mass audience. But what was the character of that public? That question, like so many problems in the sociology of literature, is difficult to resolve, but one can measure the outside boundaries of the readership of the *Encyclopédie*. First it is necessary to review the basic facts about all the editions of Diderot's text; then it should be possible to calculate the economic limits to their different consumption patterns; and finally one can attempt to chart the geographical and social distribution of the quarto editions, which were by far the most numerous in prerevolutionary France.

Aside from the Italian editions published (in French) in Lucca and Leghorn, the expurgated Protestant *Encyclopédie* published in Yverdon by Barthélemy de Félice, and the *Encyclopédie méthodique*—a completely

reorganized work that ran to 202 volumes and was not completed until 1832—Diderot's text went through four main metamorphoses.<sup>31</sup>

(1) The first edition (1751–52): this was a folio edition consisting of 17 volumes of text and 11 plates, followed by a five-volume *Supplément* and a two-volume *Table Analytique*. There were 4,225 sets printed, but only half, or perhaps merely a quarter, of them were sold in France. The subscription price was 980 livres, and the market price in the 1770s varied from 1,200 to 1,500 livres.

(2) The Genevan reprint (1771–76): it had the same number of folio volumes in a printing of 2,200 sets. The subscription price was 794 livres, but by June 1777 it was selling at 700 livres, owing to competition from the quarto editions.

(3) The three quarto “editions” (1771–81): these correspond to Duplain's three subscriptions and appeared under the names of Pellet and the Société Typographique de Neuchâtel, as explained above. The quartos contained 36 volumes of text and three volumes of plates. They included 8,011 sets in all and were almost entirely sold out at the subscription price of 384 livres—the price paid by individual subscribers; booksellers subscribed at a reduced price of 294 livres and received one free copy for every dozen they ordered.

(4) The two octavo “editions” (1778–82): these were really one expanded edition representing two subscriptions, published at Lausanne and Bern. The octavos consisted of 36 volumes of text and three of plates. They included 6,000 sets in all, and each sold at a subscription price of 231 livres.

This enumeration of facts and figures suggests a surprising conclusion: there were far more *Encyclopédies* in prerevolutionary France than anyone—except eighteenth-century publishers—has ever suspected. Although the subscription figures in the publishers' papers make it difficult to calculate precisely how many copies remained in the kingdom, they permit a safe estimate: between 14,000 and 16,000 *Encyclopédies* existed in France before 1789, and half of them can be traced. So without pretending to know how many of those *Encyclopédies* were read, or in what way the readers responded to them, it seems legitimate to hypothesize that *encyclopédisme*

<sup>31</sup> The following information comes from the sources cited above (nn. 1, 2), except for the figures on market prices and on the sizes of the quarto and octavo printings, which come from the papers of the STN. The extraordinary richness of those papers makes it possible for the first time to estimate accurately the total volume and cost of the *Encyclopédie* trade in prerevolutionary France. The octavo publishers originally announced that their edition, which followed the quarto page by page, would sell at a subscription price of 195 livres—6 livres down, 5 livres for each volume of text, and 15 livres for each volume of plates. When they learned that the quarto would run to 36 volumes of text instead of 29, as was originally planned, they had to follow suit and therefore charged 231 livres for their subscriptions—contrary to what has been affirmed by Watts (“Swiss Editions of the *Encyclopédie*,” 231) and by Lough (*Essays on the Encyclopédie*, 40). On the subscription prices of the octavo, see also the *Gazette de Berne*, Nov. 19, 1777, and Apr. 8, 1780.



could have spread far more widely through French society than is generally believed.

As the *Encyclopédie* progressed from edition to edition its format decreased in size, it contained fewer plates, its paper declined in quality, and its price went down. And as the publishing consortia succeeded one another, they cast their nets more and more widely, reaching out with each new edition toward remoter sections of the reading public. The price differential set some rough limits to this ever-broadening sales pattern: the quarto edition cost a little more than one-fourth and the octavo edition about one-fifth of the market price of the first folio in the 1770s. But what were the social boundaries of *Encyclopédie* "consumption"? The question may seem impertinent, since economics offers no explanation of what it is to "consume" a book and since book buying and book reading are quite different activities. Nonetheless, the purchase of a book is a significant act when considered culturally as well as economically. It provides some indication of the diffusion of ideas beyond the intellectual milieu within which cultural history is usually circumscribed. And as there has never been a study of the sales of any eighteenth-century book, a sales analysis of the most important work of the Enlightenment ought to be worthwhile.

One can estimate how closely the *Encyclopédie* came into contact with the lower classes by translating its price into bread, the key commodity of the Old Regime and the basic element in the diet of most Frenchmen.<sup>32</sup> A first folio *Encyclopédie* was worth about 3,500 loaves of bread and a quarto 960 loaves, the standard of measurement being the "normal" price of 8 sous for a four-pound loaf of rye bread in prerevolutionary Paris. An unskilled laborer with a wife and three children would have to buy at least 18 loaves a week to keep his family alive. In good times he would spend half his income on bread. A "cheap" quarto *Encyclopédie* therefore represented more than a year of his family's precarious nutriment. It would have been as inconceivable for him to buy it—even if he could read it—as for him to purchase a palace. Skilled laborers—locksmiths, carpenters, and printers—made 15 livres in a good week. The first folio would have cost them 93 weeks' wages, the quarto 26 weeks' wages, and the octavo 15½ weeks' wages. So even the upper strata of the working classes, artisans like the men who printed the book, could never have afforded to buy it.

But the men who wrote it, the "Gens de Lettres" invoked on its title page, could have purchased the cheaper editions. Diderot himself made an average of 2,600 livres a year for his thirty years of labor on the *Encyclo-*

<sup>32</sup> The following information on artisans' "budgets" and the price of bread comes from the work of Ernest Labrousse, George Rudé, and Albert Soboul. For a convenient summary of their findings, see Rudé, *The Crowd in the French Revolution* (Oxford, 1959). The "Banques des ouvriers" in the STN papers, contain full information on the wages of the printers of the Société Typographique. Oddly enough, they corresponded exactly to the wages of skilled workmen in Paris, where the cost of living was higher.

*pédie*.<sup>33</sup> A quarto would have cost him 7½ weeks of his wages and an octavo 4½—not an extravagant sum, considering that he had other sources of income. Many writers were wealthier than Diderot, thanks to patrons and pensions. B. J. Saurin, a typical figure from the upper ranks of the Republic of Letters, now deservedly forgotten, made 8,600 livres a year in pensions and “gratifications.”<sup>34</sup> He could have treated himself to a quarto, the equivalent of 2½ weeks’ income. The octavo was for hack writers like Durey de Morsan, a literary adventurer who lived off the crumbs from Voltaire’s table and who wrote as one of the octavo’s “zealous subscribers” to the Société Typographique de Neuchâtel:

The number of poor literary men far surpasses that of rich readers. I myself am delighted that this work, too expensive until now, does not exceed the means of the semi-indigent such as myself. I would like the door of the sciences, of the arts, and of useful truths to be open, day and night, to every human who can read.<sup>35</sup>

It is impossible to produce typical figures for the wide variety of incomes among the middling classes of the provinces, but the following calculations should give some idea of the expensiveness of the *Encyclopédie* for persons located well below the great noblemen and financiers and well above the common people. Although curés received only 500 livres as their *portion congrue* after 1768, their annual income often amounted to 1,000–2,000 livres.<sup>36</sup> So a quarto *Encyclopédie* represented ten weeks’ income for a prosperous curé. Magistrates of the baillage courts stood at the top of the legal profession among provincial bourgeois and often earned 2,000–3,000 livres a year: a quarto *Encyclopédie* was worth six or seven weeks of their income. To live “noblement” a bourgeois had to count on at least 3,000–4,000 livres a year in *rentes*: the purchase of a quarto *Encyclopédie* would have taken five weeks of his revenue.<sup>37</sup>

In strictly economic terms, therefore, the first two editions were so expensive that they cannot have penetrated far beyond the restricted circle of courtiers, salon lions, and progressive *parlementaires* who made up the cultural avant-garde. The cheaper editions were luxury items, but with some squeezing they could have been made to fit into many middle-class budgets, rather as encyclopedias do today. The cost, like the content, of the quarto and octavo *Encyclopédies* appealed to a wide variety of small-town notables

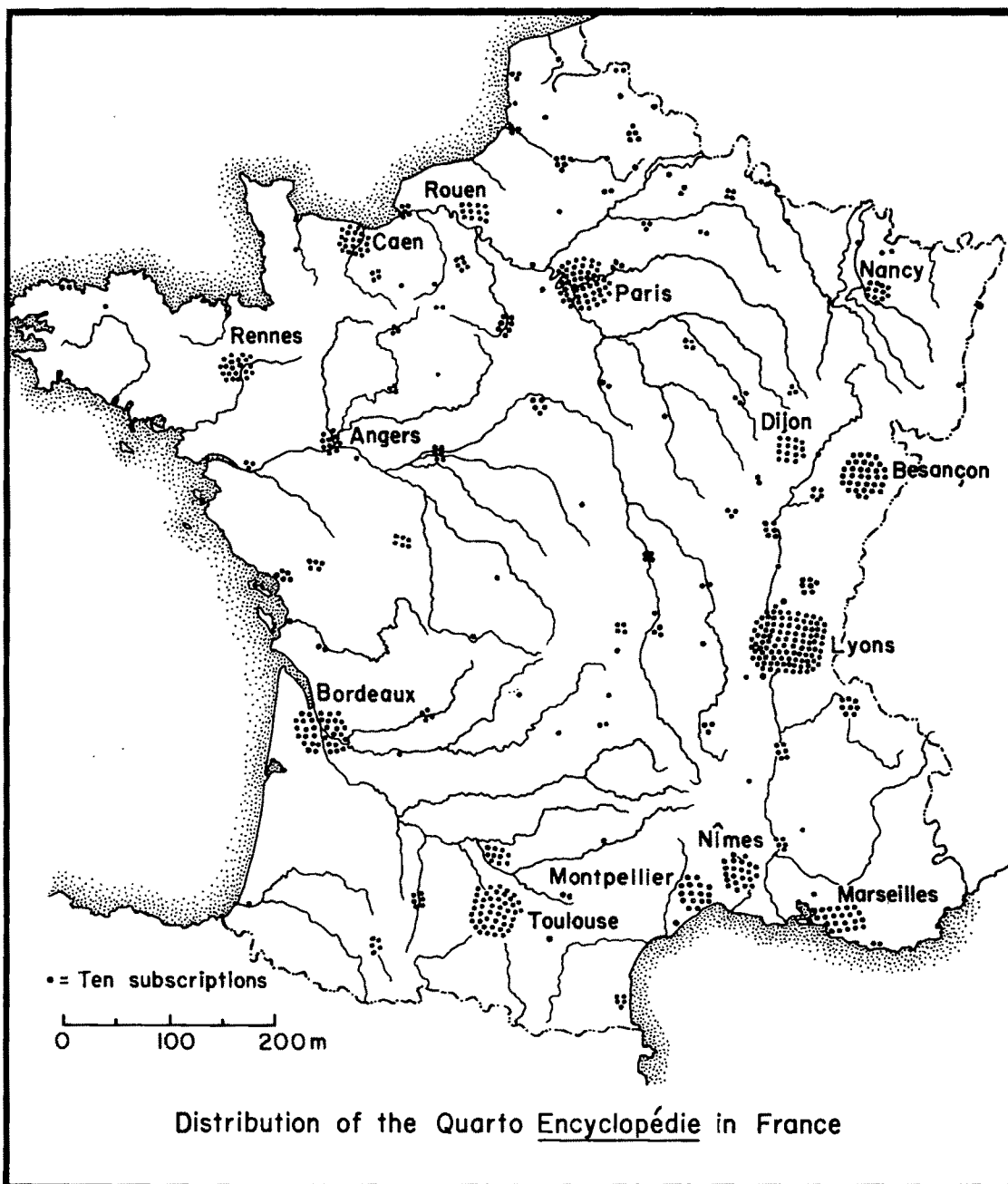
<sup>33</sup> Proust, *Diderot et l'Encyclopédie*, 59; for a detailed study of Diderot’s income, see pages 81–116.

<sup>34</sup> Robert Darnton, “The High Enlightenment and the Low Life of Literature in Prerevolutionary France,” *Past and Present*, no. 51 (1971): 87.

<sup>35</sup> Durey de Morsan to F.-S. Ostervald of the STN, Apr. 17, 1778.

<sup>36</sup> Marcel Marion, *Dictionnaire des institutions de la France aux XVII<sup>e</sup> et XVIII<sup>e</sup> siècles* (Paris, 1923), 446; Henri Sée, *La France économique et sociale au XVIII<sup>e</sup> siècle* (Paris, 1933), 64–66. The *portion congrue* allotted to every curé from the revenue of the *dîme* (tithe) was increased in 1786 to 700 livres.

<sup>37</sup> Philip Dawson, *Provincial Magistrates and Revolutionary Politics in France, 1789–1795* (Cambridge, Mass., 1972), ch. 3; Sée, *La France économique et sociale*, 162.



This map is drawn up from Duplain's secret subscription list, MS 1220 in the Bibliothèque de la Ville de Neuchâtel. The list covers all but one of the 8,011 sets printed. Of these 828 were foreign and so do not figure on the map. The map also excludes 76 sets that were sold to unidentified individuals and 25 sets that were given away—either as compensation for employees and associates or to procure protections; at least 10 of these went to Lyons, and Panckoucke dispensed 4 in Paris. The unidentified sales all involved single sets, except in four cases, which appear on the list simply as "Ollier 6," "Vasselier 4," "La Flèche 39," and "Berage 8." "La Flèche" could have been a person but probably represents La Flèche, Maine, where there was a famous school, originally founded for the Jesuits by Henry IV. The large number of copies sold in Lyons as compared with Paris resulted in part from the way the business was handled: Duplain directed the marketing operations from Lyons, while Panckoucke's many affairs kept him too busy to be much of a salesman in Paris. Also, the Parisian market was probably pretty well supplied by earlier editions. This map therefore should not be taken to prove that the capital of the Enlightenment absorbed relatively few *Encyclopédies*. What it provides is a fairly accurate picture of *Encyclopédie* diffusion in the provinces.

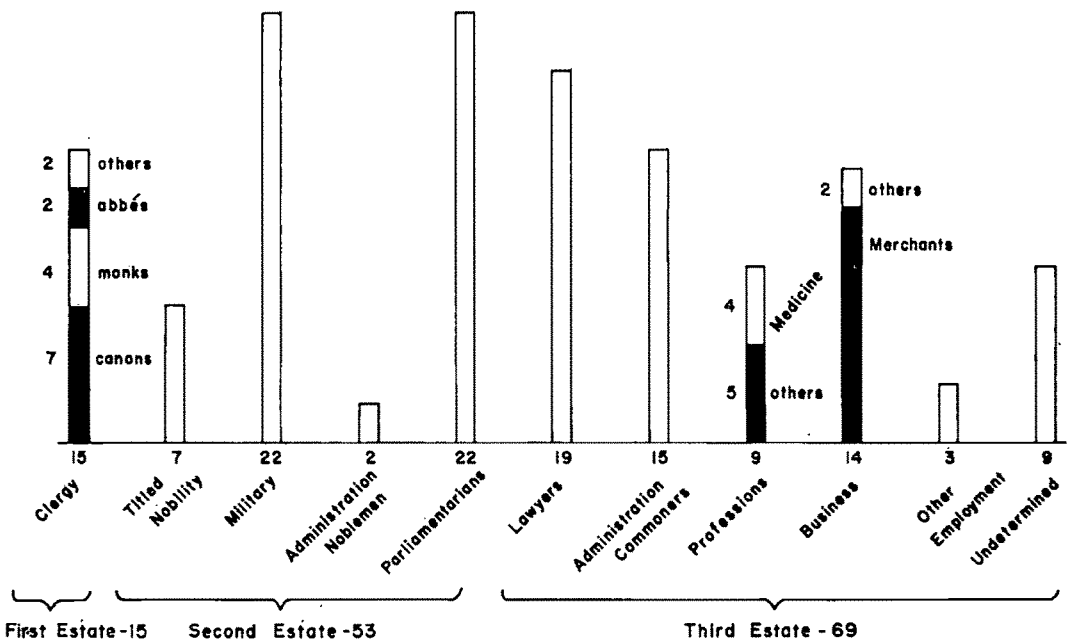
SUBSCRIPTIONS TO THE QUARTO *ENCYCLOPÉDIE* IN FRANCE

Abbéville	26	Castelnaudary	27	Macon	17	Rethel	40
Aire	8	Castres	28	Mantes	8	Roquemaure	7
Aix	6	Chalon-sur-Saône	67	Marseilles	228	Rouen	125
Alençon	34	Châlons-sur-		Meaux	30	St. Chamond	2
Amiens	59	Marne	1	Metz	22	St. Didier	1
Angers	109	Champagne	2	Montargis	26	St. Etienne	13
Argentin	3	Chartres	75	Montauban	105	St. Flour	24
Arras	26	Chatillon	39	Millau	8	St. Lô	7
Auch	65	Clermont	13	Montbrisson	6	St. Omer	5
Aurillac	13	Colmar	1	Montpellier	169	St. Quentin	16
Autun	39	Dijon	152	Morlaix	1	Saintes	26
Auxerre	10	Dôle	52	Mortagne	22	Saumur	1
Auxonne	1	Embrun	3	Moulins	52	Sedan	2
Avignon	55	Evreux	65	Nancy	120	Sète	13
Bayonne	16	Falaise	45	Nantes	38	Soissons	52
Beaune	26	Douai	14	Nîmes	212	Strasbourg	2
Beauvais	8	Grenoble	80	Niort	58	Tarbes	52
Bergerac	13	Gueret	19	Noyon	26	Thiers	39
Bergues	1	La Fère	15	Orléans	52	Toulon	21
Besançon	338	Langres	26	Paris	487	Toulouse	451
Billom	2	Laon	17	Perigueux	36	Tours	65
Bordeaux	356	La Rochelle	56	Peronne	15	Troyes	53
Boulogne-sur-Mer	34	Le Havre	52	Perpignan	52	Tulle	4
Bourg	91	Le Mans	40	Poitiers	65	Valence	65
Bourg-Saint-		Le Puy	39	Reims	24	Valenciennes	13
Andéol	4	Lille	28	Rennes	218	Verdun	12
Bourges	20	Limoges	3	Riom	46	Versailles	4
Brest	20	Lisieux	27	Roanne	26	Vichy	2
Caen	221	Lunéville	1	Rochefort	27	Villefranche	37
Cambray	57	Laigle	3				
Carpentras	2	Lyons	1078				

and country gentlemen but not to anyone below the bourgeoisie. As the publishers remarked—and they knew their clientele—“The in-folio format will be for grands seigneurs and libraries, while the in-quarto will be within the reach of men of letters and interested readers [*amateurs*] whose fortune is less considerable.”<sup>38</sup> The *Encyclopédie* entrepreneurs realized that they could widen their profit margin as they broadened their market. They had discovered a gold mine of untapped literary demand, and their scramble to exploit it shows how advanced culture reached the general reading public. But where were those readers located, and who were they?

The map (see p. 1348), drawn up from Duplain's secret subscription list, shows the geographical distribution of almost all the quarto copies, that is approximately half the *Encyclopédies* that existed in prerevolutionary France. It demonstrates that the *Encyclopédie* reached every corner of the country and that its distribution coincided fairly well, as far as one can tell, with the distribution of population. Subscriptions in the Parisian area and the northwest were few, perhaps because those markets were sated by other editions. Beyond Rennes, Brittany looks like an intellectual desert, which might have been the case, but a surprising fertile crescent of *Encyclopédies*

<sup>38</sup> STN to Rudiger of Moscow, May 31, 1777.

Subscribers to the Quarto *Encyclopédie* in Besançon

First Estate-15      Second Estate-53

Third Estate-69

This bar graph is drawn from the list of individual purchasers of the quarto edition in Lough, *Essays on the Encyclopédie*, 466-73. It contains the names and *qualités* of 253 subscribers from the Franche Comté, of whom 137 were from Besançon. Duplain's secret list shows there were 390 subscriptions sold in the province, a figure that is confirmed by letters from the two booksellers who collected them. Therefore the representativeness of the Comtois list, which was drawn up according to the order in which the subscriptions arrived, is far from being complete—it amounts to two-thirds of the subscriptions sold. But the last third of the subscribers probably tended to come from outlying areas of the large, mountainous province, and so the bar graph probably gives a fairly accurate picture of the subscription pattern within Besançon. The military category seems to have been made up entirely of noblemen—most had titles but are not entered under "titled nobility"—but the "parlementaires" probably included an undetermined number of commoners, so the second estate appears somewhat larger on the graph than it was in reality. The same may be true of the third estate, because some of the "undetermined" category could have been noblemen. The three men represented by "other employment" were identified on the list as "intendant du Prince de Bauffremont," "Conseil de Mgr. le Duc du Châtelet," and "garde-magasin," presumably an army position.

curves through the Midi, from Lyons to Nîmes, Montpellier, Toulouse, and Bordeaux. Even the Massif Central shows a fairly high density of subscriptions. So there is little evidence here for the hypothesis that France was divided into a backward south and a progressive north by the "Maggiolo line" of literacy, running from Mont St. Michel to Geneva.<sup>39</sup> The *Encyclopédies* seem to have sold best in towns where there were parlements and academies, but it sold very well everywhere: that is probably the main conclusion to be drawn from the map. Once reincarnated in a comparatively cheap edition, Diderot's text traveled farther and wider than has been appreciated.

<sup>39</sup> Michel Fleury and Pierre Valmary, "Les Progrès de l'instruction élémentaire de Louis XIV à Napoléon III," *Population*, no. 1 (1957): 71-92. Of course there is no reason to expect that the diffusion of the *Encyclopédie* should coincide with the primitive level of literacy indicated by Fleury and Valmary.

Duplain's secret subscription list does not identify all of the subscribers; it contains only the names of booksellers, who generally bought lots of a dozen or more sets, which they retailed among their local clients. But there is one list of individual purchasers of the quarto edition in the Franche Comté. It has been translated into the bar graph (see p. 1350), which covers Besançon, a judicial, administrative, ecclesiastical, and military center, where sales were unusually strong. The graph shows a high percentage of purchasers in the legal profession, both lawyers and members of the parlement of Besançon. The *Encyclopédie* sold well in the first two estates, and especially among noblemen in the army, as might be expected in a garrison town. Royal administrators, almost all of them nonnoble, also bought the book in large number, and so did bourgeois professional men, particularly doctors, though to a lesser extent. Fourteen of the 137 sets went to merchants and manufacturers—a large proportion in comparison with Daniel Roche's statistics on provincial academicians and Jacques Proust's analysis of the contributors to the *Encyclopédie*.<sup>40</sup> Approximately one-half of one per cent of the people in Besançon bought the quarto *Encyclopédie*—a high percentage, but one that seems credible, given the above economic analysis of cost and clientele. The town's two main booksellers, Lépagnez and Charmet, had not expected to sell more than a dozen or so sets and were astounded at the book's success, especially as their trade had fallen into a slump since 1777. "Please don't believe that I enjoy any great consumption of books here," Lépagnez wrote to the Société Typographique. "I swear to you that after *L'Histoire universelle*, *L'Histoire ecclésiastique*, that of the Gallican Church, the Bible of Vance, the *Encyclopédie*, and the Rousseau, everything else has given me no business at all for the last two years."<sup>41</sup>

The sales pattern of Besançon may not have been typical of France as a whole, but nonstatistical information shows a similar enthusiasm for the *Encyclopédie* in other provincial centers. In Toulouse, at the other extremity of the kingdom, a bookbinder called Gaston sold 182 quartos in three weeks and expected to place 400 octavos. And in general, when French booksellers mentioned their quarto clients in their correspondence they named lawyers, royal officials, and local noblemen—unlike their counterparts in Northern, Central, and Eastern Europe, who referred only to courtiers. So all the evidence points in the same direction: in prerevolutionary France the *Encyclopédie* worked its way into the world of the provincial notables who assumed the leadership of the Revolution and who continued to dominate the countryside throughout the nineteenth century.

No one can pretend to know what message "took" in the minds of those readers. Many of them must have bought the *Encyclopédie* for what it

<sup>40</sup> Daniel Roche, "Milieux académiques provinciaux et société des lumières," in François Furet et al., *Livre et société dans la France du XVIII<sup>e</sup> siècle* (Paris, 1965), 93–185; Proust, *Diderot et l'Encyclopédie*, ch. 1.

<sup>41</sup> Lépagnez to STN, Aug. 30, 1780.

claimed to be: a compendium of all knowledge, rather than philosophic propaganda. As Panckoucke put it, "The *Encyclopédie* will always be the first book of any library or cabinet"<sup>42</sup>—but it could have been a book to display on shelves, not to read. In fact Panckoucke reported that some subscribers in Lyons could not read at all. But it is difficult to believe that a high proportion of its owners never got through even its Preliminary Discourse, which is a manifesto of the Enlightenment. And far more people must have read the *Encyclopédie* than owned it, as would be common in an era when books were liberally loaned and when *cabinets littéraires* were booming. It therefore seems legitimate to conclude that the biography of this book—the protection accorded it by French authorities, the struggle to exploit it among bookdealers, and its diffusion among a clientele of middle-range notables everywhere in the country—that this extraordinary success story reveals an Enlightenment that had spread far beyond the elite of court and capital and had penetrated throughout the upper echelons of the Old Regime. As the Société Typographique wrote to a customer in August 1779:

Never has an enterprise of this kind and this scope had a greater success, nor has one been conducted with such speed. In less than two and a half years, and after having renewed the subscription twice, we have printed 8,000 copies of this *Encyclopédie*, of which we have only a small number yet to sell. The public seems to have waited impatiently to be served by publishers less rapacious than the producers of the first edition [a dubious statement]. We and our associates pride ourselves in having satisfied it in this respect; and you will observe, Sir, that if Enlightenment [*lumières philosophiques*] lacks in this best of all possible worlds, it will not be our fault.<sup>43</sup>

<sup>42</sup> Panckoucke to STN, Aug. 4, 1776.

<sup>43</sup> STN to J. G. Bruere of Homburg, Aug. 19, 1779.

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# The Historical Problem of Generations

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ALAN B. SPITZER

EACH GENERATION writes its own history of generations. Or perhaps, when contemporary generational differences force themselves on the consciousness of historians they rediscover significant age-specific relationships in the past. Given our recent past, the current preoccupation with past generations was predictable, but as is appropriate for historians we shall probably run somewhat behind events, flooding the market with histories of generations just when our present generational crisis has evaporated.<sup>1</sup> There is nothing wrong with this—our responsibility does not lie in being up-to-date but in the effective application of what has been a vague, ambiguous, and stretchable concept to the explanation of past events.

<sup>1</sup> A sampling of recent works in the history of (predominantly European) generations includes: Anthony Esler, *Bombs, Beards and Barricades. 150 Years of Youth in Revolt* (New York, 1971); Esler has also mined this theme in *The Aspiring Mind of the Elizabethan Younger Generation* (Durham, 1966) and in his chapter, "Youth in Revolt: The French Generation of 1830," in Robert Bezucha, ed., *Modern European Social History* (Lexington, 1972), 301-34; Lewis S. Feuer, *The Conflict of Generations: The Character and Significance of Student Movements* (New York, 1969); Daniel R. Browder, "Fathers, Sons and Grandfathers: Social Origins of Radical Intellectuals in Nineteenth-Century Russia," *Journal of Social History*, 2 (1969): 333-55; Herbert Moller, "Youth as a Force in the Modern World," *Comparative Studies in Society and History*, 10 (1968): 237-60; Phyllis H. Stock, "Students versus the University in Pre-World War Paris," *French Historical Studies*, 7 (1971): 93-110; Phillipe Bénéton, "La Génération de 1912-1914. Image, mythe et réalité?" *Revue française de science politique*, 21 (1971): 981-1009; Peter Lowenberg, "The Psychohistorical Origins of the Nazi Youth Cohort," *AHR* 76 (1971): 1457-1502; Herbert Butterfield, *The Discontinuities Between the Generations in History* (Cambridge, 1972); William J. McGrath, "Student Radicalism in Vienna," *Journal of Contemporary History*, 2, no. 3 (1967): 183-201; the entire issue of *ibid.*, 5, no. 1 (1970) is devoted to "The Conflict of Generations"; Michael A. Ledeen, "Fascism and the Generation Gap," *European Studies Review*, 1 (1971): 275-83. Somewhat earlier efforts include: Marvin Rintala, *Three Generations: The Extreme Right Wing in Finnish Politics* (Bloomington, 1962); Rintala, "The Problem of Generations in Finnish Communism," *American Slavic and East European Review*, 17 (1958): 190-202; Rolland Ray Lutz, Jr., "Fathers and Sons in the Vienna Revolution of 1848," *Journal of Central European Affairs*, 12 (1962): 161-73; John Eros, "The Positivist Generation of French Republicanism," *Sociological Review*, new series 3 (1955): 255-77; Yves Renouard, "La Notion de Génération en Histoire," *Revue Historique*, 209 (1953): 1-23; Sigmund Neumann, "The Conflict of Generations in Contemporary Europe: From Versailles to Munich," *Vital Speeches of the Day*, 5 (1939): 623-28. In my opinion the best historical treatment of a particular generation is still Louis Mazoyer, "Catégories d'âge et groupes sociaux. Les Jeunes Générations françaises de 1830," *Annales*, 10 (1938): 385-423.



The parallel development by sociologists, political scientists, and demographers of a literature devoted to a systematic analysis of contemporary generations provides a methodological resource for historians, although the social scientists also fall into the tendency, characteristic of generational studies, of a slippery, ambiguous usage that blurs distinctions which should be clarified.<sup>2</sup> It will be my contention that clarity can be preserved and useful explanations developed if instead of asking how long a generation really is, or how many generations usually coexist, or what points in the individual's life cycle are decisive, or whether aging has more profound political consequences than early socialization, we ask whether, and in what respects, age-related differences mattered in a given historical situation.

Modern empirical studies of generations proceed from the theoretical contributions of Karl Mannheim. Most historians accept Mannheim's classic formulation: "The social phenomenon of 'generations' represents nothing more than a particular kind of identity of location, embracing related 'age-groups' embedded in a historical-social process." They follow Mannheim and other pioneers such as François Mentré and José Ortega y Gasset, in distinguishing between generations identified through familial succession—the biological chain from father to son to grandson—and generations conceived as groups of coevals, people of roughly the same age whose shared experience significantly distinguishes them from contemporaries in other age groups. Some demographers prefer to reserve the term "generation" for the familial succession and apply "cohort" to the group of coevals, but historians have generally retained the traditional term with a qualifier that indicates a significant shared experience, writing of "social" or "political" or "literary" generations.<sup>3</sup>

<sup>2</sup> Some of the standard approaches from the perspective of the social sciences are: Karl Mannheim, "The Problem of Generations," in his *Essays on the Sociology of Knowledge* (London, 1959), 276–322; S. N. Eisenstadt, *From Generation to Generation: Age Groups and Social Structure*, (Glencoe, 1956); Rudolf Heberle, *Social Movements: An Introduction to Political Sociology* (New York, 1951); Bennett M. Berger, "How Long Is a Generation?" *British Journal of Sociology*, 11 (1960): 10–23; Marvin Rintala, "A Generation in Politics: A Definition," *Review of Politics*, 25 (1963): 509–22; see also Julián Marías, "Generations: The Concept," and Martin Rintala, "Generations: Political Generations," in the *International Encyclopedia of the Social Sciences* (1968), 6: 88–96. There is a recent immense accumulation of literature on youth as such, distinguished by contributions from Erik Erikson, Kenneth Keniston, Richard Flacks, Seymour Lipset, and others. See the bibliographical article by John Somerville, "Toward a History of Childhood and Youth," *Journal of Interdisciplinary History*, 3 (1972): 439–47.

<sup>3</sup> Mannheim, *Essays on the Sociology of Knowledge*, 292; François Mentré, *Les Générations sociales* (Paris, 1920); José Ortega y Gasset, *The Modern Theme* (New York, 1961); Ortega, *Man and Crisis* (New York, 1958); Norman R. Ryder, "The Cohort as a Concept in the Study of Social Change," *American Sociological Review*, 30 (1965): 843–61; the recent translation of Julián Marías's *Generations. A Historical Method* (University, Alabama, 1970) provides the best survey of the theory of generations in Comte, Mill, Wilhelm Dilthey, Ortega, and so forth. It also covers the classic works on literary generations. On the latter, see also, Julius Petersen, "Die Literarischen Generationen," in Emil Ermatinger, ed., *Philosophie der Literaturwissenschaft* (Berlin,

Such adjectives provide a certain focus for the application of the concept but do not resolve fundamental problems of definition, or the essential problem of establishing the boundaries of any presumed generation. That problem is defined by the most telling argument against any historical explanation based on generations—to wit, "There's one born every second." This observation seemingly disposes of all the theories that divide centuries into three generations, or substitute generational spans for traditional periodization, or discover a dialectical alternation of types of generations. Johan Huizinga provides a classic statement of the objection:

A triad of generations 1700-33, 1734-69, and 1770-1800 is proposed, by means of which a number of historical phenomena, together constituting the history of the eighteenth century, are considered in the sequence rise, maturity and decline—or action, reaction and synthesis. But there can just as easily be a series of generations marked by the years 1701-1734, 1735-70, and 1771-1801, and so on for every year, and actually for every day. . . . The theory is more valid when applied to one specific and well-defined cultural phenomenon. But even then its validity is deceptive, for the generation in itself, considered biologically, is always quite arbitrary, and can never be held responsible for an evolutionary phase of a specific historical phenomenon.<sup>4</sup>

Another formidable statement of dissent was contributed by Lucien Febvre, who showed what little comfort was left in confining the theory to "one specific and well-defined cultural phenomenon"; for this very limitation is an admission that any general chronological definition cuts across significant particular age groups. There is no *a priori* guarantee that a literary generation, for example, will be chronologically congruent with a political generation which can be identified in roughly, but not precisely, the same time span. Even if such generations happen to be perfectly congruent chronologically they may not share the attributes that set them apart from their predecessors and successors, for "there is no guarantee that the *political* generations of 1660 and 1690 are set apart by the differences and for the reasons that divide the literary generations of 1660 and 1690."<sup>5</sup> Furthermore, the historical treatment of generations invariably refers to only a segment of the age group under consideration. The attributes of a "youth" composed of French intellectuals will have little relation to their Chinese coevals, or more to the point, to French peasants and workers of the same age.

These objections have been recognized by theorists of generations, who usually apply some variant of Mannheim's "generation unit" to

1930), 130-87; Detlev W. Schumann, "Cultural Age-Groups in German Thought," *PMLA*, 51 (1936): 1180-207; and Henri Peyre, *Les Générations littéraires* (Paris, 1948).

<sup>4</sup> Johan Huizinga, *Men and Ideas* (New York, 1965), 73-74.

<sup>5</sup> Lucien Febvre, "Générations," in *Bulletin du centre internationale de synthèse. Section de synthèse historique*, no. 7, p. 41, published in *Revue de synthèse historique*, 47 (1929).

the social group or cultural phenomenon they wish to isolate with reference to birth dates. According to Mannheim, "Youth experiencing the same concrete historical problems may be said to be part of the same actual generation; while those groups within the same actual generation which work up the material of their common experiences in different specific ways, constitute separate generation units."<sup>6</sup> We must all make do with something like Mannheim's distinctions whenever we wish to generalize about age-specific behavior without asserting the identity of all those within the relevant cohort. Yet it can be argued, as Febvre did argue, that the identification of political, intellectual, religious, economic, social, and some number of other variables that might plausibly differentiate one generation unit from another requires distinctions so fine and complex as to reduce the ambitious concept to a "useless" and "parasitical" notion.<sup>7</sup>

THE MOST SYSTEMATIC attempt to meet these and other serious objections to any historical theory of generations is in the recently translated book of Julián Marías, *Generations. A Historical Method*. It contains a thorough survey of the literature on the subject, and an attempt, through a consolidation and exposition of Ortega y Gasset's fragmentary writings on generations, at a definitive resolution of the issues raised in the literature. Marías ranges over the entire history of the concept of generations, but the core of his argument is concentrated in the pages where he shows how generations can be reconstructed "empirically" through the application of Ortega's principles.<sup>8</sup> Ortega and Marías begin with the definition of a generation as a group born within a zone of dates and sharing "a structure of *vigencias*"—the binding customs, collective usages, traditions, and beliefs that define the real social existence of each individual. The dimensions of each zone of dates approximately correspond to the fifteen-year span that Ortega assigns to each overlapping but historically distinct age group. These age groups are characterized by the fairly familiar categories of childhood, youth, initiation, dominance, and old age. The age of dominance, for example, subsumes those aged forty-five to sixty, who usually run the world and who share a not completely separate but appreciably different structure of *vigencias* from the preceding and succeeding generations.

<sup>6</sup> Mannheim, *Essays on the Sociology of Knowledge*, 304. There are two gradations in this concept: only those coevals who shared significant experience would comprise a generation, and only those who worked out their problems in the same way would be members of the same generation unit.

<sup>7</sup> Febvre, "Généérations," 42.

<sup>8</sup> Marías, "Ortega's Theory of Generations," in *Generations*, 69-106.

Mariás is aware that this approach is vulnerable to Huizinga's objection. What justifies the arbitrary selection of one zone of dates instead of another? Why refer to an age of dominance for those aged forty-five to sixty in 1965, rather than those forty-five to sixty in 1964 or 1966? The answer is revealed in the concept of "the decisive generation," the one that "for the first time thinks the new thoughts with full clarity and with complete possession of their meaning, a generation that is neither still a precursor nor any longer bound by the past." The decisive generation is identified or reconstructed through the discovery of the individual who "most clearly represents the essential characteristics of a period," when the "full bloom of a new era" occurs.<sup>9</sup> Thus Descartes is identified as the "eponym" of a decisive generation, and the date of his thirtieth birthday becomes a tentative point of departure from which other generations can be fixed by adding or subtracting multiples of fifteen. The generational center of the decisive generation might actually fall on the twenty-eighth or the thirty-fourth rather than on the thirtieth birthday of Descartes, but empirical investigation will reveal the appropriate birthday.<sup>10</sup>

Mariás and Ortega recognize major objections to this approach but dispose of them with reference to "the empirical content of the human past." Their brand of empiricism consists of magisterial assertions about significant individuals in the history of ideas who are characterized as anomalous, or representative, or eponymous with regard to their epoch. If one does not choose to widen one's historical lens to Ortega's focus on two hundred years of "historical crisis" resolved by a spiritual renaissance commencing with Galileo and culminating in Descartes one is not likely to be persuaded of the eponymous individual in the decisive generation. But even if one believes that a new era blossomed between 1600 and 1650 and that its essential characteristics were represented by Descartes, one need not agree that whatever was particularly significant in Descartes was substantially shared by his coevals, or that the essential contributions of Descartes had much to do with generational phenomena which cannot be subsumed under intellectual history, or that the fundamental transformation separating the generation of Descartes from its predecessors would subsequently be significantly modified at something like

<sup>9</sup> Mariás, *Generations*, 100. Ortega y Gasset, *Man and Crisis*, 62. Cf. *ibid.*, p. 61: "Take a great historic ambit within which a change in human living has been brought about which is fundamental, visible and unquestionable."

<sup>10</sup> Mariás (*Generations*, 172-76) presents a tentative variation on the Ortegian approach for periods in which it is difficult to locate the decisive generation or the representative figure. He locates more or less representative figures born fifteen years apart, clusters the names of other important coevals around each, and then adds chronological layers to each core, year by year, until an age group seems anomalous in one of the original categories but appropriate to its predecessor or successor, at which point he has established the boundary between two generations. It is difficult to see the need for this, for if Mariás believes in the decisive representativeness of Descartes, and the permanent validity of the fifteen-year intervals, he can simply add on fifteen-year layers from the seventeenth to the nineteenth century.

fifteen-year intervals. Some of this might be rendered plausible by research or even demonstrated; in Ortega and Mariás it is merely asserted.<sup>11</sup>

Furthermore there is a circularity in the Ortegian approach characteristic of those generational theories which define a phenomenon in a way that provides an explanation of its historical effects. It is perfectly acceptable to identify *vigencias*, or generation units, as clusters of attributes that distinguish groups chronologically, but it is something else to explain the behavior of those groups with reference to a chronological definition constructed out of the evidence of that very behavior. That is, if a historically significant cohort is defined as all those whose experience of the First World War decisively affected their political behavior in 1939, questions about the generational consequences of World War I are answered by definition.

I do not present these criticisms as a counsel of despair because I believe that a certain methodological modesty can disarm the standard objections put with such clarity by Huizinga and Febvre. The problem posed by Huizinga: how to specify the boundaries of generations in the seamless continuum of daily births? is a problem for anyone who chooses to mark off categories in any continuum. In this sense specifying generations is no more arbitrary than specifying social classes, or ideologies, or political movements where there is inevitably a shading off or ambiguity at the boundaries of categories. Indeed the most chaste behavioralism often creates arbitrary categories—as tall, medium, or short; extremely anti-imperialist, moderate, extremely proimperialist; and so forth. I would even argue that such categories as 5'9" or 160 lbs. suffer from the same defects as "the generation born between 1792 and 1802." Demographers, after all, feel no qualms in manipulating categories presented to them by the arbitrary decisions of the Bureau of the Census, inserting in their pyramids the cohort of "males aged 25-30 in 1960," without wondering whether they might not have used "left-handers aged 27-31 in 1958." Where we suspect that age-specific differences are historically significant we can quite appropriately cut age groups out of the continuum to see whether observations of their documented collective behavior and their relation to other groups can contribute to plausible explanations. Of course it may be that their behavior is not sufficiently distinct to set them off from older or younger groups in any useful way.

The same considerations apply to the class of objections raised by Lucien Febvre. His criticism is actually directed against two different ways in which units of generations are used to identify the entire age group. The first has to do with generation units suggested by categories of collective behavior that presumably can be distinguished along

<sup>11</sup> For a historian who accepts the Ortegian method with certain qualifications see, Renouard, "La Notion de Génération en Histoire."

generational lines, as in literary or political generations. The second refers to minorities of age groups which are presumed to characterize the entire generation, such as a "youth" consisting of a radical minority of college students.

The first method of identifying generations presents a problem only because of slovenly usage or of familiar habits of expression. When one identifies a literary generation that persists for some fifty years, one is really saying that despite the differences in the socialization and life experience of individuals who were not coevals there were no significant age-specific differences with regard to literature during the period. A fifty-year generation makes people uneasy so they try to chop it up into decent fifteen- or thirty-year intervals.

While slovenly, the chronological stretching of the term "generation" is often perfectly intelligible. When Alexander Portnoy says that he belongs to the generation of network radio and eight teams to a league he is locating himself in a population born perhaps between 1890 and 1935, but he has identified a cultural category in which age differences do matter without foreclosing other ways of slicing up the population. On the other hand the chronological stretching of the term sometimes obscures significant age-specific experiences and blurs useful historical distinctions. One might identify a Positivist generation in France extending from 1850 to 1900, but this is little help in understanding the persistent differences among Positivists, Cousinian spiritualists, and devout Catholics that characterized the intellectual life of the period. However the attack on Positivism after 1900 was manifested along generational lines, at least in contemporary polemic.<sup>12</sup>

Febvre's correct remark that such a unit as a literary generation may not be substantially identical with a coeval political generation need not inhibit us if we can document significant age-specific differences in the particular subject or field of collective behavior under consideration. But this does not dispose of the objection to the presumption that some minority incarnates or represents an entire generation. The question often has a polemical edge—for example, when the characterization of a "youth" depends on one's response to the assumption that a radical intelligentsia expresses the general will of its coevals.

This issue is endemic in many areas of political and social analysis. Identifying radical college students with youth in general raises the same questions as characterizing the entire black population by urban militants, or all Protestant churches by Prohibitionists. The point is that we can reject false claims to identity or even representativeness

<sup>12</sup> Eros (in "The Positivist Generation of French Republicanism") does identify a specific generation of the young Republican politicians of the 1870s whose Positivist formation distinguished them from their predecessors. They constituted the aging establishment attacked by the anti-Positivists of the turn of the century.

without denying that significant distinctions may depend on the relevant minority. We might discover both that Prohibitionists were only a minority of Protestants and that Prohibitionism separated the Protestants from other sects. George Rudé's investigations of the social composition of the revolutionary crowd cannot demonstrate that his militant workshop masters, craftsmen, wage earners, shopkeepers, and petty traders actually represented their social groups, but he can show that insofar as such groups played a significant political role they did it through that militant minority.<sup>13</sup>

WE ARE AFTER THE way in which the unit contributes to an explanation of collective differences. We want to avoid the imposition of categories in ways that blur or obscure significant differences. Blurring and obscuring are chronic to the generational approach because distinct age-specific phenomena are often jumbled into the same historical generation. This problem is recognized in the recent research of political scientists and sociologists who will, for example, distinguish between life cycle and generation behavior—the first referring to recurrent behavior appropriate to the chronological phases of every individual's life span, and the second, as emphasized by Mannheim, Marías, Rudolf Heberle, and others, reflecting the distinct collective experiences of given age groups, which stamp those age groups with a permanent separate identity as they move through time. The first approach is as old as the conception of the Ages of Man; the second is often applied by historians with reference to a social trauma or a "Great Divide," as in the identification of a World War or Depression generation. Neither of these categories need be identical with what occurs when, as Mannheim puts it, "individual members of a generation become conscious of their common situation and make this consciousness the basis of their group solidarity."<sup>14</sup> The articulation of this shared consciousness is more or less what is meant by a generational ideology.<sup>15</sup> If the division (however perceived) between generations is greater than that normally attributed to life-stage differences we have what is currently called a generation gap.

<sup>13</sup> George Rudé, *The Crowd in the French Revolution* (Oxford, 1959). A different way of looking at this issue is by identifying and controlling relevant variables. For instance, in evaluating the significance of age for collective behavior, social scientists often control for education; see, for example, Samuel A. Stouffer, *Social Research to Test Ideas* (Glencoe, 1962), 121–24. It is also possible to examine generational identities as phenomena of more profound social divisions; see, for example, Georg Lukács, "Balzac: Lost Illusions," in *Studies in European Realism*, (New York, 1964), 47–64.

<sup>14</sup> Mannheim, *Essays on the Sociology of Knowledge*, 290.

<sup>15</sup> Eisenstadt (*From Generation to Generation*, 102, 311) defines a "youth ideology" that affirms "youth culture" as a distinct type of social and cultural life. Collective affirmation of a generational ideology has usually been embodied in a youth movement, but could in principle just as well be articulated by older cohorts.

These historically overlapping but conceptually distinct generational categories by no means exhaust the ways in which collective behavior might correlate with age. When, for example, we have identified age-specific differences in political attitudes that constitute a significant generation gap we have not necessarily demonstrated that these differences "will endure and transform culture."<sup>16</sup> We are more like our fathers than we like to think, and dramatic generational conflicts have often been softened or eroded by time and the stamp of culture until the rebellious youths assume in maturity the commitments and lifestyles of their predecessors. This is sometimes the case even when a generation has suffered a historical trauma presumed to mark it for life—the cataclysms of the First World War, the Second World War, the German occupation, and the Liberation did not liberate Frenchmen from the characteristic political institutions of the Third and Fourth Republics.

On the other hand, there are fundamental changes, manifested first as a generational break, that become permanent and are transmitted through successive age groups until they characterize the entire population. Many observers believe that the differences between older and younger cohorts of French farmers represent such a turning point, or more broadly, that the most fundamental change in French life since 1789 lies in the patterns of social and economic behavior that have distinguished those born after 1930 from their predecessors.<sup>17</sup>

Just as specific investigation is required to differentiate the presumed historical consequences of being young, it is called on to verify assertions regarding the effects of aging on collective behavior. American political scientists have been especially concerned with the relationship between aging and political attitudes.<sup>18</sup> The same easy generalizations and hidden complexities obtain for received opinions associating aging with increased conservatism as for those identifying youth and rebellion. To cite a familiar example: When one has identified a correlation between old age and conservative attitudes one still has to establish whether that cohort has become increasingly conservative with age or has retained attitudes,

<sup>16</sup> This is an insight of the sociologist Philip Abrams. For the concept of the life cycle Abrams coins the term *age span*, the "culturally defined phases of the individual life cycle which may be empirically observed in any society"; for the phenomenon of generational solidarity he introduces *age groups* shaped by the "collective consciousness crystallized within an age span . . . creating meaningful (linking or disassociating) relationships between it and other age spans"; and he assigns the term *generations* to age groups that "not only repudiate norms established by their seniors but carry that repudiation with them through life and seek to transmit it through their successors." Philip Abrams, "Rites de Passage," *Journal of Contemporary History*, 5, no. 1 (1970): 175-90.

<sup>17</sup> See, for example, John Ardagh, *The New French Revolution* (New York, 1969), 67-68.

<sup>18</sup> For an early proposal to explore this relationship, see, John Schmidhauser, "The Political Behavior of Older Persons: A Discussion of Some Frontiers of Research," *Western Political Quarterly*, 11 (1958): 113-24.



considered relatively liberal or radical in its youth, that have come to rest at the Right of a shifting political spectrum.

An age cohort may, then, be differentiated from the rest of the population because its attitudes persist while those of the majority change. In such a case differences that begin as political or ideological may end as generational. A political elite that wrests power from its coeval enemies may hold it long enough to become a gerontocracy increasingly distinguished from the mass of the population by attitudes preserved from its heroic receding past. Perhaps this is the phenomenon to which Chou En-lai referred when he commented on the relative youth of President Nixon's entourage.

There are many ways in which age differences of no particular significance are transformed and sharpened by changing objective realities. Where we find a developing correlation between old age and resistance to tax-supported education, we may be observing an appropriate collective response to the deteriorating financial situation of older people rather than some constitutional crabbedness inevitably associated with aging. The introduction of military conscription creates an immediate, vital age- (and sex-) linked distinction that virtually imposes a generational self-identity on those of draft age.

The actual historical situation of any age group is defined in practice by its relationship to other cohorts, even with regard to size. Norman Ryder's observation that "a cohort's size relative to the sizes of its neighbors is a persistent and compelling feature of its lifetime environment"<sup>19</sup> has been tragically verified in this century by the effects of the virtual obliteration of entire generations. As Sigmund Neumann suggested over thirty years ago, the demographic consequences of the First World War are not exhausted by the skewed pyramids of the demographers. On the eve of the Second World War he emphasized the "over-age" of the political leaders of France and Britain who "had to maintain positions which should have been filled by millions of young men lost in the World War and cheated of their share in making a new world."<sup>20</sup> This suggestive insight into the complex, remote consequences of an erosion that separated generations by something like a demographic trench was rather blurred by Neumann because he fused it into a discussion of the crucial conflict between the prewar political cohorts and the surviving members of the wartime generation. He was concerned to advance what has become one of

<sup>19</sup> Ryder, "The Cohort as a Concept in the Study of Social Change," 845. From a somewhat different angle Bennett Berger emphasized the difference in conceptions of relative age associated with different occupations—a baseball player is "old" at thirty-five, a presidential candidate "young" at fifty. Berger, "How Long Is a Generation," 15.

<sup>20</sup> Neumann, "The Conflict of Generations in Contemporary Europe," 627; he covers roughly the same ground in *The Permanent Revolution* (New York, 1965), 230–56.

the familiar interpretations of the Nazis—as a political generation separated from their elders not because they were decimated by the war but because they were socialized in the trenches.

Neumann's approach reflects the classic distinction between contemporaries and coevals argued by generational theorists such as Mannheim and Ortega who emphasize that generations at different phases of the life cycle experience the same events in different ways.<sup>21</sup> Young soldiers fight and die while older cohorts mourn and rule. However, one cannot deduce the historical significance of a particular generational relationship from the fact that "every moment in time . . . is always experienced by social generations at various stages of development."<sup>22</sup> It remains to be established by research and analysis.

Indeed any assumption of a relationship between age groups and behavior of interest to historians needs to be established by investigation of each particular case. Marvin Rintala's assertion that "no shared destiny is more fundamental than that of the same generation" cannot be refuted (or verified) if it is a statement about human essence, but it is of no help in understanding specific historical context. There have been many situations in which class, racial, sexual, religious, or linguistic differences were far more significant than those related to age. The questions to be put to the data are suggested by Philip Abrams's remarks on so-called political generations: "We must ask in what circumstances differentiation springing from the social organization of age will crop into age-linked political conflict, and finally we must ask in what particular circumstances such conflicts will be defined in terms of a conflict of generations rather than anything else."<sup>23</sup>

In answering such questions the familiar generalizations about the recurrent characteristics of phases of the life cycle are not always helpful. To explain the alienation of young Frenchmen in the early 1830s and young Americans in the late 1960s by the chronic tropism of the young for radicalism, idealism, frustrated mobility, oedipal hostility, and so forth, contributes little to explanations of youthful passivity and careerist pragmatism in the 1850s or the 1950s. Again Mannheim cleared the conceptual ground with the observation that factors presumed to be present in every situation cannot explain "the particular features of a given process of modification."<sup>24</sup> Of course

<sup>21</sup> This concept is persuasively applied in Claude Digeon's *La Crise Allemande de la pensée française (1870-1914)* (Paris, 1959), where characteristic responses to the catastrophe of 1870-71 are identified for generations of 1830, 1850, 1870, and 1890.

<sup>22</sup> Mannheim, *Essays in the Sociology of Knowledge*, 283.

<sup>23</sup> Rintala, "A Generation in Politics," 509; Abrams, "Rites de Passage," 181.

<sup>24</sup> Mannheim, *Essays on the Sociology of Knowledge*, 312. In *ibid.*, 297 n., Mannheim characterizes "the fundamental thesis of this essay . . . that biological factors (such as youth and age) do not of themselves involve a definite intellectual or practical orientation (youth cannot be automatically correlated with a progressive attitude and so on); they merely *initiate* certain formal tendencies, the actual manifestations of which will ultimately depend on the prevailing

one might construct an explanation out of the clichés about youthful alienation by assuming that the periods of rebellion were the norm, other things being equal, and that the task is to identify the intervening variables that precipitated those abnormal eras when youth was apolitical and acquiescent—as if parents were to ask, “What is the matter with that well-behaved boy?”

There are times, as at the present, when significant generational differences seem confined to the conflicts between youth and everybody else. This encourages the tendency to consider those historical developments that are linked to age groups solely in relation to the generation gap.<sup>25</sup> Significant generational differences are then reduced to the conflict between father and son, the biological succession of generations is confused with the historical succession of age cohorts, and assumptions regarding patterns of behavior common to youth at any time and place are fused with descriptions of specific experiences that stamp a permanent collective identity on a given generation.<sup>26</sup>

A RECENT WIDELY DISCUSSED attempt to specify the historical circumstances in which the life-stage of youth attains a particular, and malignant, coherence is Lewis S. Feuer's *The Conflict of Generations*. Professor Feuer ranges widely across time and space to identify and explain the implications of dynamic student movements from the German *Burschenschaften*, through a century and a half of generational rebellions in Europe, Africa, and Asia, to the Berkeley student uprising of the late sixties. Feuer's concentration on student movements is consistent with his title, because he argues that a politically dynamic student movement always reflects a conflict of generations. His concern is not merely to demonstrate where and how youth movements have mattered, but to identify the recurrent ele-

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social and cultural context. Any attempt to establish a direct identity or correlation between biological and cultural data leads to a *quid pro quo* which can only confuse the issue.”

<sup>25</sup> See, for example, Anthony Esler's *Bombs, Beards and Barricades*, which is subtitled *150 Years of Youth in Revolt*. Esler does distinguish the nonrevolting youth of other times and places from those in the Western world who have been revolting ever since the impact of the Democratic and Industrial Revolutions, *ibid.*, 34. This is virtually the conclusion of Konrad Lorenz, who finds modern youth extraordinarily revolting in “The Enmity between Generations and its Probable Causes,” *Psychoanalytic Review*, 57 (1970): 334-404. Lorenz believes that the process of family disintegration that began with the Industrial Revolution has deprived youth of the indispensable transmission of tradition, except in “certain lucky old-fashioned peasant families.” Herbert Moller (in “Youth as a Force in The Modern World”) makes a demographic distinction between periods when youth is a relatively small proportion of the population and things are reasonably quiet, and periods when a large proportion of the population is young and things go to hell in a hand basket.

<sup>26</sup> For the observation that the concept of youth as a distinct life stage between adolescence and adulthood is not universal, that it is both historically contingent and confined to a minority of the age group, see Kenneth Keniston, “Youth: A New Stage of Life,” *American Scholar* 39 (1970): 631-54; see also the influential if controversial views of Philippe Ariès, *Centuries of Childhood*, tr. R. Baldeck (New York, 1962).

ments that give them a characteristic stamp and predictable consequences. These elements might be summed up by the terms "idealism" and "irrationality," attributes explicable, according to Feuer, by the psychological matrix of all such generational conflict. Without repeating the criticisms of Feuer's tendentiousness in selecting, ordering, and interpreting evidence to confirm his antipathies,<sup>27</sup> I wish to comment on those flaws in his conceptual apparatus that suggest recurrent problems in historical explanations of generational conflict.

The major conceptual flaw in *The Conflict of Generations* is in the causal model that provides the explanatory force and interest of the work. In principle at least, Feuer's method allows him to discriminate, and to explain the differences, between rebellious and conformist youthful generations. Universal characteristics of youth contribute to the characteristic form of youth movements but do not guarantee the rise of those massive and militant student movements that only appear with the "de-authorization" of the older generation "as a collective whole."<sup>28</sup> In the particular occasions of the de-authorization of the fathers an explanation is found not only for the radical alienation of the sons but for the recurrent political expressions of this alienation. The recognition of the oedipal springs and the parricidal guilt of the generational rebellion helps us to understand the self-sacrificing idealism, the populism, and the murderous and suicidal irrationalism of militant youth movements.

As Feuer travels across his immense blighted generational landscape he traces a somewhat circular path because he begins with a definition of a student movement as a combination of students moved by "disillusionment with and rejection of the values of the older generation."<sup>29</sup> He would probably answer that he has in fact identified a locus of collective parental loss of authority for each case of destructive youthful rebellion—fixing historically the psychological antecedents of behavior that could never be understood in strictly ideological or sociological terms. But Feuer cannot establish that a militant, irrational youth had reason to reject its fathers' authority unless he can demonstrate that the generation's irrational militants actually experienced the psychological process of parental de-authorization. Feuer does this with selected individuals such as Mao Tse-tung or Karl Follen, the leader of the German student movement after the Napoleonic Wars. However, these individual examples—which themselves do not

<sup>27</sup> For example, Richard Flacks's review article in *Journal of Social History*, 4 (1970-71): 141-53; Marshall Meyer's review in *American Journal of Sociology*, 75 (1969): 293-95; for a review which admits the flaws but is fundamentally sympathetic, see Henry A. Murray, *The American Scholar*, 38 (1969): 710-16.

<sup>28</sup> Feuer, *The Conflict of Generations*, 184.

<sup>29</sup> A point made in a review by Arthur Liebman, in *American Sociological Review*, 34 (1969): 1012.

bear careful scrutiny—cannot validate an explanation of collective behavior.<sup>30</sup>

Feuer's variant of the "Oedipal-rebellion" hypothesis is rejected by all those who hold what Kenneth Keniston calls the "red-diaper-baby" theory, based on evidence that today's young rebels are characteristically the children of yesterday's radicals. Of course it is perfectly possible that contemporary studies which find "continuity with parental values to be the rule and discontinuity the exception"<sup>31</sup> cannot be generalized to other times and other places, but in any case generalizations about the individual antecedents of collective behavior cannot be verified solely with reference to the collective behavior. That is, one cannot explain collective behavior with reference to specific antecedent experiences when one cannot provide evidence for the antecedent experiences.

Attempts to do this often fall into the ecological fallacy—the assumption that the relationships of properties of groups are identical with the relationships of properties of individuals within the groups. This is particularly tempting when one wishes to emphasize the generational consequences of early socialization, where it is assumed that because a large percentage of a given age group has had a certain experience at time 1, and a large percentage of the same age group engaged in a certain form of behavior at time 2, the two percentages represent the effects of the experience in time 1 on the behavior in time 2.

A stimulating example of this fallacy is Peter Lowenberg's article, "The Psychohistorical Origins of the Nazi Youth Cohort," which asserts a correlation between the political behavior of young Nazi voters in 1932 and their childhood experience of nutritional deprivation, absence of parents, and failure of public authority during the First World War. Applying Freudian conceptions of fixation and regression, Lowenberg argues that the traumatic wartime experiences of those born roughly between 1900 and 1910 resulted in a "weakened character structure manifested in aggression, defenses of projection and displacement, and inner rage," which revealed itself politically as a result of the renewed trauma of the Great Depression, in the preference

<sup>30</sup> Feuer's treatment of Follen is certainly unpersuasive. To support the characterization (in *The Conflict of Generations*, 59) of Follen's prototypical conflicts with his father, Feuer quotes a passage about Follen's resentment at having been teased by his father, without giving an inkling of the following passage from the same text: "It was during this period, that the strict and tender union commenced between Charles and his father, which combined all the holiness of a natural affection with all the peculiar pleasures of a tender friendship. . . . This tender, this unlimited indulgence established a peculiar feeling of intimacy and of confiding love between him and his father, such as few boys are blessed with." What the entire passage seems to establish is that Follen's relations with his father were deeply affectionate but not without friction. Charles Follen, *The Works of Charles Follen* (Boston, 1841), 1: 5-9.

<sup>31</sup> Kenneth Keniston, *Youth and Dissent* (New York, 1971), 273-74.

of the cohort for "extremist paramilitary and youth organizations and political parties."<sup>32</sup>

Lowenberg likes the generational approach because it "deals with probabilities—with the law of averages on a macroscale—thus canceling out any of the many individual variables that determine conduct."<sup>33</sup> But the law of averages on a macroscale does not eliminate the *post hoc* element in his model or guarantee that those individuals who actually experienced the relevant early trauma disproportionately voted Nazi in 1930 or 1932, or that the young Nazi voters had not enjoyed a significantly more secure, stable, and well-fed childhood than that of the young voters whose relative mental health led them to join the paramilitary organizations of the Social Democrats.

It is conceivable that the younger cohorts of northern Protestant agricultural, lower-middle-class, and self-employed voters who swung to nazism, or the young urban workers who voted Communist, suffered greater childhood deprivation than the young inhabitants of Berlin and Hamburg working-class districts who stuck with the SPD, but Lowenberg presents no evidence to that effect. Nor does he present evidence that would impel one to prefer his model to standard explanations of the propensity of young people in general, and students in particular, to turn to extreme solutions when traditional alternatives have failed. He does more or less feed this interpretation into his explanation, along with the thesis that assimilates the political responses of young Germans, especially students, to other groups vulnerable to economic dislocation, chronic underemployment, and the threat of proletarianization. He also grants some force to the thesis emphasizing continuities between the prewar youth movement and Nazi appeals to a postwar youth. Presumably he believes that these relationships are not sufficient conditions for such pathological political behavior as voting Nazi or Communist in 1932, without the additional variable of the early trauma.<sup>34</sup> My argument is not that this view is impossible, but that Lowenberg fails to demonstrate that traumatized youth voted in a manner significantly different from nontraumatized youth, or to explain why the "second trauma" of the depression and the other factors that impelled older cohorts to vote for the Nazis do not sufficiently account for the Nazi sympathies of youth.

A somewhat different psychohistorical origin is identified by Anthony Esler in his study of the rebellious French youth of 1830. They characteristically experienced a pattern of parental overindulgence fol-

<sup>32</sup> Lowenberg, "The Psychohistorical Origins of The Nazi Youth Cohort," 1501.

<sup>33</sup> *Ibid.*, 1464.

<sup>34</sup> I am not concerned here with Lowenberg's debatable assumptions that to vote Nazi or Communist in 1932 was in some sense pathological or neurotic behavior and that such behavior was psychodynamically congruent with the early trauma.

lowed by extreme repression at school, and "this pattern of permissiveness in childhood yielding abruptly to repressiveness in adolescence surely helped to create the smoldering sense of injustice that burst out at last in the generational rebellion of the 1830's."<sup>35</sup> Without dwelling on the psychodynamic assumptions buried in Professor Esler's "surely," one might still point out that his model does not help to distinguish this rebellious generation from any other cast in the same familial mold; nor does it establish a significantly different background for the nonrevolutionary generation units of 1830.

Something rather like the ecological fallacy is argued in Rolland Lutz's attempt to relate social class to the political role of the generation of the Viennese "sons" who were at the radical core of the revolution of 1848. Noting the lower-class origin of a considerable minority of students at the University of Vienna he remarks, "What could be more natural than the assumption of leadership over the Vienna masses by the educated sons of provincial shopkeepers and artisans?"<sup>36</sup> This assumption is fortified by no evidence that the poor students were the most militant. There seems no reason to prefer Lutz's assumption to the contemporary American discovery that militant students are more likely to be drawn from upper-class educated families than from the ranks of the ambitious poor.

In arguing these points it is not my intention to smother fruitful hypotheses under a blanket of methodological Pyrrhonism. This article proceeds from the assumption that age-specific relationships are sometimes of great historical significance. The problem is to decide which kind of age relationship is specifically relevant and, therefore, to identify explanatory model and criteria of verification that are appropriate to the specific relationships under investigation.

BECAUSE ATTEMPTS to verify generalizations about the effects of aging, early socialization, or other generational phenomena have not come to much, some social scientists have concluded that there is little to be gained from a generational approach.<sup>37</sup> Yet others have assembled evidence indicating that age differences do matter for certain groups under certain circumstances. Such evidence is usually drawn from survey research, which can be useful, despite various limitations, to historians of the recent past. My purpose, however, is not to remind those historians of something they already know or to suggest that historians imprison themselves in the methodological preferences of sociologists and political scientists. The point is that surveys of generational phe-

<sup>35</sup> Esler, in Bezucha, ed., *Modern European Social History*, 308.

<sup>36</sup> Lutz, "Fathers and Sons in The Vienna Revolution," 167.

<sup>37</sup> See, for example, H. Hyman, *Political Socialization* (Glencoe, 1959), 139-54.

nomena, particularly when periodically readministered to age cohorts, suggest and clarify the various and distinct ways in which age categories relate to collective behavior. It might be instructive, therefore, to contemplate some examples of cohort analysis in the social sciences as well as certain methodological issues raised in the literature.<sup>38</sup>

The most familiar and least informative ordering of data related to age is a cross-sectional survey with the age group as the independent variable and an attitude expressed at the time of the survey as the dependent variable, as, for example, in table 1. V. O. Key cites this table to suggest some relation between fundamental shifts in opinion and subsequent generational differences, but he grants that one can draw few firm conclusions from it: "The numbers of the sample do not suffice to permit analysis to tie these differences definitely to age, but if they are so connected, the more conservative views of those of the earlier generation may reflect a strong attachment among them to the values of an earlier era."<sup>39</sup> But even if the sample were adequate, the only discernible connection would be that differences of opinion at the moment of the survey over the proper scope of government welfare were to some extent related to age.

We cannot say whether the more restrictive views of older white-collar workers reflect a stronger attachment to an earlier era, a natural consequence of aging, the permanent effect of early socialization, an unusual temporary generational difference, or the attitudes of the same age cohorts in other occupations. Furthermore the age categories

TABLE 1. AGE IN RELATION TO OPINIONS ON PROPER  
SCOPE OF GOVERNMENT WELFARE ACTIVITY AMONG  
WHITE-COLLAR RESPONDENTS

<i>Opinion</i>	<i>Per Cent under 35</i>	<i>Per Cent 35-55</i>	<i>Per Cent over 55</i>
Should do more	28	28	23
Doing about right	51	43	34
Should do less	15	25	39
Don't know	6	4	4
	100	100	100
N	148	226	106

Source: V. O. Key, *Public Opinion and American Democracy* (New York: Alfred A. Knopf, Inc., 1967), 255.

<sup>38</sup> There is still some point in Mannheim's observation: "The present status of the problem of generations thus affords a striking illustration of the anarchy in the political and cultural sciences where everyone starts out afresh from his own point of view." *Essays in the Sociology of Knowledge*, 287.

<sup>39</sup> V. O. Key, *Public Opinion and American Democracy* (New York, 1967), 255.



may be too gross for the distinctions we wish to make. Someone interested in the attitudes of postadolescents would find little comfort in such a category as "under 35." However, more finely discriminated cohorts would come no closer to answering the questions posed above if such cohorts were taken from a single cross-sectional survey. What is required is evidence arranged longitudinally—age-related data gathered through time. The most familiar ordering of such data is in arbitrarily defined age cohorts re-examined in successive surveys, which are often separated by uniform intervals.

The systematic manipulation of cohort data is nothing new, at least in the field of demography, but the other social sciences have only recently begun to explore its possibilities. William Evan's article on the cohort analysis of survey data, published in 1959 and often cited as a pioneering effort, was the cutting edge of what has become a fairly substantial literature.<sup>40</sup> Evan's intention was to introduce and illustrate "the cohort technique, which for present purposes will be roughly equated to a generational analysis, as a means of inquiring into the impact of types of historical events on the opinions, attitudes or ideologies of different generations." For his example Evan traced cohort opinions on government control of railroads, through polls successively taken in 1937, 1945, and 1953 (see table 2).

Perhaps the most useful message delivered by Evan's table is that questions about political or other effects of generational differences must be answered empirically with regard to specific situations. As we follow Evan's cohorts A and B through sixteen years we note what might be considered an increasing conservatism about government control, but not a tendency for older groups to become progressively more conservative than their younger contemporaries. The comparison of the opinions of cohort A in 1937 and cohort X in 1953 indicates that the relative interventionism of the younger cohort in the earlier survey was not a permanent attribute of youth as such. Indeed the juxtaposition of the attitudes of all the cohorts with those of the total population suggests that the increased hostility to government control was general and not age-specific in any significant sense.

The various age-related distinctions expressed in small percentage differences offer little food for reflection except perhaps in the tendency of middle-aged cohort B to shift opinion less sharply than did its younger contemporaries in the course of the sixteen years. Evan notes

<sup>40</sup> William M. Evan, "Cohort Analysis of Survey Data: A Procedure for Studying Long-term Opinion Change," *Public Opinion Quarterly*, 23 (1959): 63-72. For a more recent examination of the methodological problems of applying cohort data to generational analysis see Neal E. Cutler, *The Alternative Effects of Generations and Aging upon Political Behavior: A Cohort Analysis of American Attitudes toward Foreign Policy, 1946-1966*. (Oak Ridge, 1968), especially chapter 4.

TABLE 2. OPINIONS OF TWO COHORTS ON GOVERNMENT OWNERSHIP OF RAILROADS, IN 1937, 1945, AND 1953 (IN PER CENT)

<i>Opinion and Year of Cross-sectional Survey</i>	<i>Total Population</i>	<i>Cohort A 24-30 in 1937</i>			<i>Cohort B 47-53 in 1937</i>		
1937 <sup>a</sup>							
Yes	25	27			23		
No	58	54			60		
No opinion	17	19	—	—	17	—	—
	<u>100</u>	<u>100</u>			<u>100</u>		
	(2855)	(618)			(344)		
		<i>Cohort A 32-38 in 1945</i>			<i>Cohort B 55-61 in 1945</i>		
1945 <sup>b</sup>							
Yes	20	21			25		
No	63	64			59		
No opinion	17	15	—	—	16	—	—
	<u>100</u>	<u>100</u>			<u>100</u>		
	(1584)	(252)			(155)		
		<i>Cohort X 24-30 in 1953</i>	<i>Cohort A 40-46 in 1953</i>	<i>Cohort Y 47-53 in 1953</i>			
1953 <sup>c</sup>							
Should	14	15		12	16		19
Should not	74	73		78	74		69
No opinion	12	12	—	10	10	—	12
	<u>100</u>	<u>100</u>		<u>100</u>	<u>100</u>		<u>100</u>
	(1527)	(247)		(233)	(257)		(108)

<sup>a</sup> "Do you believe the government should buy, own, and operate the railroads?"

<sup>b</sup> "Do you think the government should own the railroads in this country?"

<sup>c</sup> "Do you think the United States government should or should not own the following things in this country? How about the railroads?"

Source: William M. Evan, "Cohort Analysis of Survey Data: A Procedure for Studying Long-Term Opinion Change," *Public Opinion Quarterly*, 23 (1959):67.

that differences between age groups were smaller in a given year than were differences between identical age groups in different years; that is, the difference between cohort A and cohort B in 1937 or 1953 is smaller than the difference between cohort A in 1937 and cohort X in 1953, or cohort B in 1937 and cohort Y in 1953. This impels him to

conclude "that the historical situation has a greater impact than aging in opinion change."<sup>41</sup> He might just as well, or better, have said that with regard to government control of railroads age differences didn't matter much.

Despite its rather unexciting substantive results, Evan's essay is a useful, and influential, introduction to the systematic analysis of age cohort phenomena and has been succeeded by more detailed and statistically complex attempts to establish correlations between age and collective behavior.

A STATISTICALLY SIMPLE attempt which suggests, more or less unintentionally, the pitfalls in the manipulation of cohort data is Seymour Lipset and Everett Ladd's examination of the politics of college-educated generations. Their observations are based on the findings summarized in table 3. According to the authors, the most obvious generational phenomenon revealed by this table is "a persistent age association in the voting preferences of the college 'generations,'" so that "the younger the voter the greater the preference for the more liberal nominee," and vice versa. It is this phenomenon that suggests the tentative conclusion: "In so far as we can generalize, Aristotle's emphasis on the moderating effects of growing older turns out to be more predictive than Mannheim's theory of the long-term consequences of the early political experiences of 'generation-units.'"<sup>42</sup> The fact that older generations are more conservative than their successors does not, however, establish the moderating influence of growing older. We cannot know whether the older cohorts became more conservative as they aged unless we have established a base with which to compare their subsequent development. If we rearrange some of the data in a manner that helps us to think diagonally (see table 4), we will see that the information provided by Lipset and Ladd only partially supports their interpretation. To take an example, if we follow the cohort that attended college in 1934-38 we note that, starting with its solid Republican bias in 1948 it fluctuated from Right to Left with the rest of the electorate in 1956 and 1964, to come to rest in 1968 roughly in the position it had held twenty years before.

The authors do recognize that differences related to age may not so much reflect an absolute change in the attitudes of particular generations as a change in the position of the generation relative to the entire population. Since "the historical slope of political attitudes

<sup>41</sup> Evan, "Cohort Analysis of Survey Data," 69.

<sup>42</sup> Seymour Martin Lipset and Everett Carl Ladd, Jr., "College Generations—from the 1930's to the 1960's," *Public Interest*, 25 (1971): 99-113.

TABLE 3. PRESIDENTIAL CHOICES OF THE  
COLLEGE-EDUCATED (GALLUP)

<i>Years of College Attendance</i>	<i>Dewey</i>	<i>Truman</i>	1948 <i>Wallace<sup>a</sup></i>	<i>Thurmond<sup>a</sup></i>
1946-48 (N = 115)	47	41	9	2
1944-48 (N = 247)	53	37	8	1
1939-43 (N = 302)	57	34	5	3
1934-38 (N = 491)	56	34	7	1
1929-33 (N = 518)	64	29	3	3
1919-28 (N = 752)	70	25	2	3
1918 and earlier (N = 574)	69	27	1	2
All college-age cohorts (N = 2999)	62	30	4	3
Actual Presidential vote, total population (48,790,414)	45.1	49.6	2.4	2.4

<i>Years of College Attendance</i>	<i>Eisenhower</i>	1956 <i>Stevenson</i>
1954-56 (N = 40) <sup>c</sup>	70	30
1949-53 (N = 164)	55	44
1944-48 (N = 215)	59	41
1939-43 (N = 292)	66	34
1934-38 (N = 272)	64	35
1929-33 (N = 175)	55	45
1919-28 (N = 274)	75	25
1918 and earlier (N = 241)	75	24
All college-age cohorts (N = 1673)	62	38
Actual Presidential vote, total population (61,825,206)	57.4	42.1

<i>Years of College Attendance</i>	<i>Goldwater</i>	1964 <i>Johnson</i>
1962-64 (N = 159)	27	73
1956-61 (N = 310)	30	70
1950-55 (N = 330)	35	65
1944-49 (N = 321)	42	58
1939-43 (N = 367)	34	66
1934-38 (N = 307)	42	57
1929-33 (N = 191)	30	70
1919-28 (N = 271)	46	53
1918 and earlier (N = 109)	57	43
All college-age cohorts (N = 2365)	37	62
Actual Presidential vote, total population (70,420,910)	38.5	61.1

TABLE 3 (Continued)

<i>Years of College Attendance</i>	<i>Nixon</i>	1968 <i>Humphrey</i>	<i>Wallace</i> <sup>b</sup>
1966-68 (N = 59)	41	48	12
1962-65 (N = 221)	45	42	12
1956-61 (N = 289)	51	36	13
1950-55 (N = 115)	57	31	11
1944-49 (N = 111)	60	32	8
1939-43 (N = 184)	56	30	13
1934-38 (N = 134)	56	35	9
1929-33 (N = 109)	47	44	8
1928 and earlier (N = 235)	67	22	11
All college-age cohorts (N = 1457)	54	33	12
Actual Presidential vote, total population (73,188,253)	43.4	42.7	13.5

<sup>a</sup> Henry A. Wallace (Progressive; J. Strom Thurmond (States Rights)

<sup>b</sup> George C. Wallace (American Independent)

<sup>c</sup> N is too small for reliability.

Source: Seymour Martin Lipset and Everett Carl Ladd, Jr., "College Generations—from the 1930's to the 1960's," *Public Interest*, 25 (1971): 108. Copyright © by National Affairs Inc., 1971.

among American college generations . . . has been toward a more liberal position over time,"<sup>43</sup> the unchanged loyalties of the older generations assign them a more conservative position on the political spectrum. But if this is the case how can one speak of "the moderating effects of growing older" except in the sense that the younger cohorts have grown less moderate?

According to Lipset and Ladd their table does suggest at least one striking instance of the permanent effects of the early political experience of a generation unit. They observe that the college generation of 1929-33, graduating into the "directionless gloom" of the Hoover years, would evince a permanent disproportionate antipathy toward

TABLE 4. REPUBLICAN PREFERENCE

<i>Years of College Attendance</i>	<i>Cohort</i>	1948	1956	1964	1968
44-48 (49)	A				
39-43	B	53			
34-38	C	57	59		
29-33	D	56	66	42	
		64	64	34	60
			55	42	56
				30	56
					47

<sup>43</sup> *Ibid.*, 113.

the Republican party. This disproportion does manifest itself in the elections of 1956, 1964, and 1968, but apparently the early trauma had not yet taken hold in 1948 when the Hoover generation voted substantially to the Right of its successors.

Lipset and Ladd would undoubtedly grant the tentativeness of any conclusions drawn from their limited data. But the conclusions they wish to draw, however tentative, put a burden on the evidence that it cannot support. The fact that younger college graduates are less likely to be Republican than are older cohorts is clear enough, but any persuasive statements about the effects of aging or early experience on the politics of various cohorts should be supported by more precise and detailed evidence, traced longitudinally through time.<sup>44</sup>

Such evidence has recently been assembled by political scientists concerned with the statistical discrimination of the relationships between political behavior and age-specific variables. This literature has now accumulated to the point where it can support methodological controversies such as the running debate in the *Public Opinion Quarterly* over John Crittenden's article on aging and party affiliation.<sup>45</sup> I will briefly review the controversy, not for its substantive contributions, but for what it reveals about the problems of using longitudinal data to distinguish separate age-specific relationships.

Crittenden organized available survey data in a form that conveys the fluctuating political loyalties of age cohorts surveyed at four intervals between 1946 and 1958. On the basis of the data in table 5, Crittenden concludes that in non-Southern states, and irrespective of educational differences, aging was accompanied by an increase in Republican identity. He makes this point in two ways—first, by a vertical comparison of the age cohorts in each row, remarking that the percentages of Republican sympathizers in the older cohorts are uniformly higher than those in the younger cohorts. This of course tells us nothing about the political effects of aging but only about the political preferences of age groups in the given years. The burden of Crittenden's argument is therefore borne by his analysis and comparison of the changing preferences of successive cohorts as they age through the twelve years between 1946 and 1958. In order to fix on a measurement for the general direction of change in all the cohorts Crittenden decided to follow two cohorts from each age group through an eight-year period; for example, he records the shift in party prefer-

<sup>44</sup> For a nice example of the risks of generalizing on the basis of inadequate longitudinal data, see Norval O. Glenn and Richard E. Zody, "Cohort Analysis with National Survey Data," *Gerontologist*, 10 (1970): 237-40.

<sup>45</sup> John Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (1962): 648-57; Neal E. Cutler, "Generation, Maturation, and Party Affiliation," *ibid.*, 33 (1969-70): 582-88, followed by Crittenden's "Reply to Cutler" and Cutler's "Comment," *ibid.*, 589-92; Norval D. Glenn and Ted Hefner, "Further Evidence on Aging and Party Identification," *ibid.*, 36 (1972): 31-47.

TABLE 5. REPUBLICANS BY FOUR-YEAR AGE GROUPS  
AND EDUCATIONAL LEVEL IN NON-SOUTHERN STATES

Age Group	1946		1950		1954		1958	
	Per Cent	(N)	Per Cent	(N)	Per Cent	(N)	Per Cent	(N)
High Education <sup>a</sup>								
21-24	46	(160)	41	(45)	42	(57)	43	(38)
25-28	54	(171)	43	(52)	45	(53)	51	(57)
29-32	51	(145)	44	(53)	39	(87)	49	(71)
33-36	59	(118)	50	(50)	47	(63)	49	(86)
37-40	59	(135)	53	(48)	51	(67)	42	(64)
41-44	70	(87)	58	(30)	56	(52)	44	(50)
45-48	58	(109)	58	(33)	52	(42)	34	(49)
49-52	58	(93)	50	(19)	89	(18)	62	(38)
53-56	60	(74)	60	(15)	66	(32)	47	(36)
57-60	65	(55)	75	(12)	58	(13)	63	(23)
61-64	58	(37)	86	(7)	75	(8)	55	(19)
65-68	70	(20)	60	(5)	90	(5)	66	(19)
21-68	57	(1204)	51	(369)	50	(497)	48	(550)
Low Education <sup>a</sup>								
21-24	36	(81)	14	(28)	38	(34)	41	(17)
25-28	52	(91)	18	(22)	37	(45)	24	(17)
29-32	43	(101)	26	(35)	29	(40)	38	(33)
33-36	42	(115)	32	(45)	27	(52)	30	(38)
37-40	51	(128)	28	(51)	38	(60)	33	(36)
41-44	40	(105)	40	(46)	36	(52)	39	(54)
45-48	54	(118)	44	(35)	38	(56)	34	(47)
49-52	44	(130)	48	(47)	39	(44)	37	(49)
53-56	59	(128)	37	(32)	45	(32)	46	(41)
57-60	59	(118)	42	(43)	50	(54)	47	(30)
61-64	58	(72)	61	(28)	50	(48)	50	(41)
65-68	52	(63)	58	(26)	59	(28)	45	(49)
21-68	49	(1250)	38	(438)	40	(545)	39	(452)

<sup>a</sup> High education: graduated high school or better. Low education: did not graduate from high school. This basis of comparison is used in all subsequent tables employing education breakdowns.

Source: John Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (1962): 651.

ence of the cohort aged 21-24 in 1946 and 29-32 in 1954, and of the cohort aged 21-24 in 1950 and 29-32 in 1958. This gives him four entries for the age group, two in the High Education and two in the Low Education categories. Thus he finds, for the age group 21-24, three cohorts that shifted toward Republicanism and one that was unchanged, as

recorded in table 6. The raw figures do not give Crittenden the balance he finally strikes—an association of aging with increased Republicanism—until he corrects for the general tendency of the entire population, which is in the direction of the Democracy. That is, he counts an increase in Democratic sympathies smaller than that of the average of the entire population as a shift to relative Republicanism. The results of this method and of a similar treatment of Republican voting patterns produce his conclusion that “aging seems to produce a shift toward Republicanism in the period from 1946 to 1958.” Crittenden is quite tentative in suggesting reasons why this might have been the case as well as in granting that the effects of aging might have been complemented by “generational effects . . . that result from the impact of the Great Depression and New Deal.”<sup>46</sup> The modesty of his conclusions have not disarmed his critics.

In his article, “Generation, Maturation and Party Affiliation: A Cohort Analysis,” Neal E. Cutler uses Crittenden’s data as evidence for conclusions the reverse of those in the original article. Cutler correctly dismisses as beside the point Crittenden’s observation that the older groups were characteristically more Republican than the younger and proceeds to the main criticism, which is the inadequacy of the treatment of the longitudinal development of the cohort allegiances between 1946 and 1958. Cutler rearranges the data from Crittenden’s

TABLE 6. EIGHT-YEAR COHORT SHIFTS ON PARTY IDENTIFICATION RELATIVE TO TREND

		<i>Time 2</i>		
<i>Age of Cohort</i>				
<i>Time 1</i>	<i>Time 2</i>	<i>More Republican</i>	<i>More Democratic</i>	<i>Same</i>
(21-24)——	(29-32)	3	0	1
(25-28)——	(33-36)	2	1	1
(29-32)——	(37-40)	4	0	0
(33-36)——	(41-44)	3	1	0
(37-40)——	(45-48)	1	2	1
(41-44)——	(49-52)	3	1	0
(45-48)——	(53-56)	2	1	1
(49-52)——	(57-60)	3	1	0
(53-56)——	(61-64)	2	1	1
(57-60)——	(65-68)	3	1	0
Total		26	9	5

Source: John Crittenden, *Public Opinion Quarterly*, 26 (1962): 652.

<sup>46</sup> Crittenden, “Aging and Party Affiliation,” 654, 657.



table on the political identification of the High Education population to demonstrate that in no instance in the twelve-year existence of a cohort can one find a linear increase in Republicanism (see table 7).

At first glance the controversy has something to do with "Fun with Numbers," or "How to Get Different Results from the Same Statistics." Cutler's emphasis on the issue of a linear pattern toward Republicanism is, however, justified by Crittenden's allegation of its existence. Cutler's longitudinal arrangement of the data shows that there was no regular progression toward increased Republicanism; his reading of the data even argues a progression in the opposite direction.

Cutler's arrangement of the data in table 8 reveals that whether one looks at changes in political allegiance recorded every four years, over an eight-year period, or across the entire span of twelve years, one finds that changes in a Democratic direction outnumber those in a Republican direction. Cutler also introduces calculations to show that, on average, fluctuations in political preferences are greater within "life stages" than within cohorts surveyed at four-year intervals. That is, there is less homogeneity across the age columns than along the cohort diagonals, "more homogeneity, associated with generational cohorts than with aging process or life-stage groups."

TABLE 7. AN EMPIRICAL EXAMPLE OF COHORT ANALYSIS<sup>a</sup>

Age Intervals	Cohort <sup>b</sup> Labels	1946	1950	1954	1958	Lifestage <sup>b</sup> Labels
21-24	A					
25-28	B	46	41	42	43	(1)
29-32	C	54	43	45	51	(2)
33-36	D	51	44	39	49	(3)
37-40	E	59	50	47	49	(4)
41-44	F	59	53	51	42	(5)
45-48	G	70	58	56	44	(6)
49-52	H	58	58	52	34	(7)
53-56	I	58	50	89	62	(8)
57-60		60	60	66	47	(9)
61-64		65	75	58	63	(10)
65-68		58	86	75	55	(11)
Total		70	60	90	66	(12)
		57	51	50	48	

<sup>a</sup> Cell entries are the percentage of each cell which identified with the Republican party in the year indicated. Source: John Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (1962): 651. Data represent the "high education" group in Crittenden's analysis.

<sup>b</sup> Capital letters indicate the cohort diagonals; numbers in parentheses indicate life-stage rows.

Source: Neal E. Cutler, "Generation, Maturation, and Party Affiliation," *Public Opinion Quarterly*, 33, (1969-70): 585.

TABLE 8. ALTERNATIVE TESTS OF THE  
AGING-REPUBLICANISM HYPOTHESIS<sup>a</sup>

Cohort	Four-Year Differences			Eight-Year Differences		Twelve-Year Differences
	1946-50	1950-54	1954-58	1946-54	1950-58	1946-58
A:	—	—	+	—	+	+
B:	—	+	—	—	—	—
C:	—	+	—	*	—	—
D:	—	+	—	—	—	—
E:	—	—	+	—	+	+
F:	—	+	—	+	—	—
G:	—	+	—	+	+	+
H:	+	—	—	*	—	—
I:	+	*	—	+	—	+

\* A — indicates that the percentage of Republicans decreased from the first observation point to the second and fails to support the hypothesis; a + indicates that the percentage of Republicans increased, and supports the hypothesis; an \* indicates no difference. Cell entries derived from table 1.

Source: Neal E. Cutler, *Public Opinion Quarterly*, 33 (1969-70): 586.

Crittenden erects various defenses of his method and conclusions, notably through his concept of correcting for trend, which identifies as relatively more Republican a shift in cohort opinion that is less Democratic than the average shift of the entire population. Cutler's answer to this is merely to remark that if the entire population is shifting away from Republicanism as it ages and if a majority of the cohorts are also shifting in that direction, it is rather odd to conclude that Republicanism increases with age.

In a recent re-examination of the controversy, Norval Glenn and Ted Hefner pose the issue in this way:

If the Crittenden data can be trusted, several important questions arise as to their proper interpretation. For instance, during a period in which the secular trend is away from Republicanism, is an increase in the "relative" Republicanism of an aging cohort evidence for a conservative influence of the aging process or of passage to the later stages of the life cycle? Or does it merely reflect a tendency for party identification in adult cohorts to remain stable?<sup>47</sup>

Glenn and Hefner's assessment of revised and expanded survey data reveals a pattern of change that seems to confirm Crittenden's conclusions. They reject those conclusions, however, because the tendency for which Crittenden corrects—the trend of the entire population away from Republicanism—was significantly affected by the heavy

<sup>47</sup> Glenn and Hefner, "Further Evidence on Aging and Party Identification," 31.

mortality in the older, more Republican cohorts. Glenn and Hefner conjecture that mortality in the higher age brackets entails the disappearance of those least affected by the massive defections from the Republican party during the Great Depression, thus lessening the Republicanism of the entire population without contributing to a trend of the living population toward the Democrats.

The issue of correcting for trend has been re-examined in William R. Klecka's attempt to devise a statistical technique for leaching out the effects of all variations except those related to age—with fairly inconclusive results for the Crittenden controversy. Klecka has also proposed an alternative to the longitudinal analysis of arbitrarily defined uniform cohorts in his attempt to identify "empirically" the chronological dimensions of generations at a specific time and place. That is, he has attempted to construct a statistical device that will uncover the actual boundaries of generations by identifying significant changes in collective attitudes.<sup>48</sup> These are but examples of a substantial literature dedicated to compensating by statistical refinements for the limitations of available data.<sup>49</sup> Such efforts may prove suggestive to the few historians who struggle with the same sort of evidence, but in a broader sense, even the most arcane and least conclusive contributions of the social scientists do expose problems of generalization and inference that are implicit in most discussions of the relationship between age and collective behavior.<sup>50</sup>

AMONG OTHER THINGS these contributions suggest what every good historian knows: that the way that evidence is selected and ordered is the way that questions are posed, and therefore the way that the possible answers are imposed. The very decision to examine arbitrarily defined age cohorts admits of some insights and excludes others. To

<sup>48</sup> William R. Klecka, "Some Strategies for Seeking Age Relationships in Political Behavior," a paper delivered at the annual meeting of the American Political Science Association, in September 1971. See also Klecka, "Applying Political Generations to the Study of Political Behavior: A Cohort Analysis," *Public Opinion Quarterly*, 35 (1971): 358-73.

<sup>49</sup> See for example, Gosta Carlsson and Katarina Karlsson, "Age, Cohorts and the Generation of Generations," *American Sociological Review*, 35 (1970): 710-18.

<sup>50</sup> Some of the fundamental issues are suggested in Glenn and Zody, "Cohort Analysis with National Survey Data," 233-40; see also, Richard E. Zody, "Cohort Analysis: Some Applicatory Problems in the Study of Social and Political Behavior," *Social Science Quarterly*, 50 (1969): 374-80, which deals with the problems of cohort overlap, sample attrition, and design asymmetry. The issue of controlling such variables as sex and education is explored in Norval D. Glenn and Michael Grimes, "Aging, Voting and Political Interest," *American Sociological Review*, 33 (1968): 563-75.

In addition to Klecka's paper cited in note 48 the following papers were delivered at the session of the annual meeting of the Political Science Association in 1971 devoted to research on the problem of generations: Neal E. Cutler, "Generational Analysis in Political Science"; Stephen J. Cutler, "Some Political Consequences of Prestige Loss Among the Aged"; Anne Foner, "Age Stratification and Ideological Cleavages"; T. Allen Lambert, "Generational Factors in Political-Cultural Consciousness."

select a particular statistical relationship is to choose a potential generalization. There was nothing in the logic of Crittenden's research that forced him to measure the political effects of aging by calculating cohort changes over eight-year periods. His even more basic decision to consider the "aging" that occurs between the ages of twenty-one and twenty-eight as functionally equivalent to the aging that occurs between the ages of fifty-seven and sixty-four imposed a range of possible answers to certain questions, but not to all possible questions, about the political effects of aging.

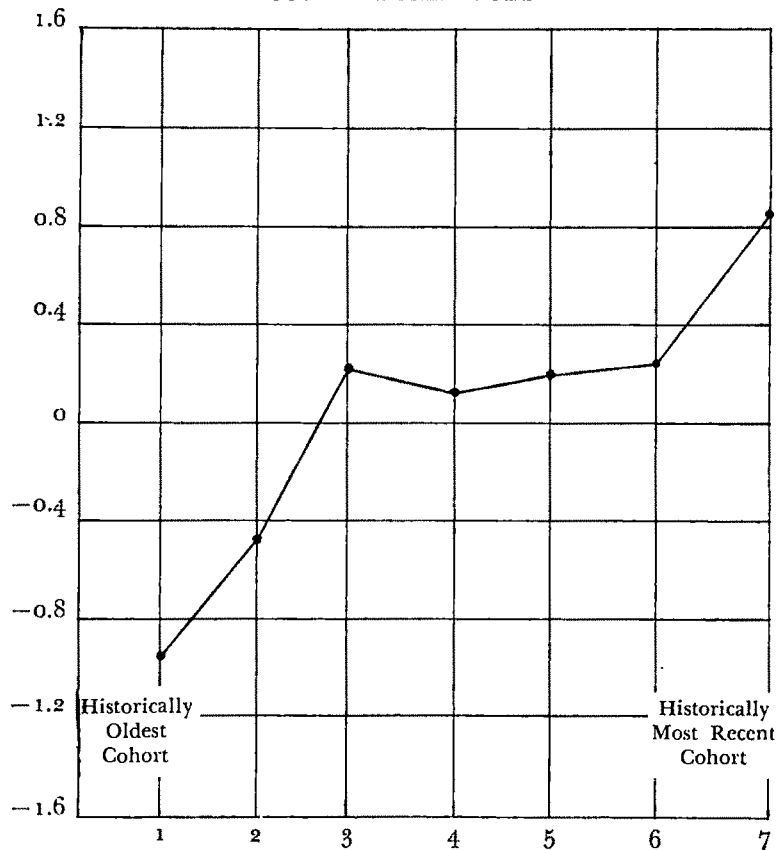
If we wish to compare how successive generations have viewed salient foreign policy issues, we might, as Neal Cutler did, construct a measure of cohort response to questions about foreign policy and then average this measure over the life of each cohort as it responded to four successive surveys administered between 1946 and 1966 (see figure 1).<sup>51</sup> This would enable us to graph and thus compare age-related attitudes to foreign policy, expressed as an average response of the particular "generations," who are experiencing the same events at different stages of their life cycles.

Cutler interprets figure 1 to support the hypothesis that the younger the cohort the greater the support for foreign aid. The method of averaging does plausibly convey the existence of differences between age groups, not at a particular time but over time. However, the method conceals possible rhythms of difference and uniformity that might have been flattened out in the averages; and does not allow of questions regarding the effects of aging, for example, or the degree to which the collective opinion of a given cohort might have been affected by a traumatic event such as the Korean War. Cutler could have organized the evidence to bear on such questions but did not. There is no point in objecting to his choice but some in recognizing the way it shaped the possibilities of his conclusions.

In an even more basic sense the nature of the available evidence shapes the nature of the conclusions—for example, generational analyses based on survey data are limited by the inherent limitations of surveys. The most obvious of these has to do with time and place. Surveys successively applied so as to provide longitudinal data have been collected only for a short time, in a few places, and about a few topics. Social scientists recognize this, of course, but cannot always resist the temptation to draw, or at least suggest, large generalizations out of their narrow data base. Consider for example, Lipset and Ladd's modest, "In so far as we can generalize, Aristotle's emphasis on the moderating effects of growing older turns out to be more predictive

<sup>51</sup> Neal E. Cutler, "Generational Succession as a Source of Foreign Policy Attitudes," *Journal of Peace Research*, 1 (1970): 33-47.

FIGURE 1. FOREIGN AID



Source: Neal E. Cutler, "Generational Succession as a Source of Foreign Policy Attitudes," *Journal of Peace Research*, 1 (1970): 40.

than Mannheim's theory of the long-term consequences of the early political experiences of 'generation-units.'<sup>52</sup> This dubious assumption of generality is not really saved by their disclaimer. There is no point to their guess that what they discovered about college-age cohorts in mid-century America "predicts" such generational patterns in other times and places and with regard to other forms of collective behavior—except to the extent that their findings falsify any formulation of the Mannheim theory as a universal law. Such gratuitous conjectures are usually controlled by the behaviorist super-ego; and a venerable tradition of criticism and self-criticism has instilled in survey practitioners a sophisticated sense of the conceptual limitations and practical flaws in extant survey data. Some of these, such as accumulated sampling error, systematic underrepresentation of certain social groups, changes in the wording of questionnaires applied in successive surveys, might be relevant to a particular generational study but are not germane to our general concerns.

<sup>52</sup> Lipset and Ladd, "College Generations," 113.

One rather technical issue of survey research is, however, central both philosophically and practically to the systematic investigation of generations. This issue might be approached by discussing the difference between a "panel" and the sample of a cohort. When a panel is surveyed the same questions are periodically readministered to the same group of respondents. Longitudinal studies based on survey data do not, strictly speaking, measure the change in attitudes of the individuals originally surveyed, but the change in the proportion of those holding particular attitudes in successive samples of the same cohort.<sup>53</sup> A study that concludes that the surveyed cohort becomes more conservative with age is actually describing an increase in the percentage of those expressing conservative attitudes in the later samples, not the increasing conservatism of particular individuals surveyed in the early samples.

The relevance of this issue has to do with what William Evan calls "the biasing effect of changes in the composition of cohorts."<sup>54</sup> This would be no problem if one could assume that changes in the composition of cohorts through demographic loss, migration, and immigration were randomly distributed along the spectrum of attitudes surveyed. But in the world we precariously inhabit this is not always a safe assumption. There are not only demographic effects related to the normal erosion of the aged population or to the disproportionate erosion of males in the older cohorts, but also the immense age-specific destruction that accompanies war and other social tragedies. Thus unfortunately there is some sense in speaking of the virtual disappearance of an entire generation. One could not assume identity in the internal structure or the interrelationships of cohorts of French males surveyed in 1914 and 1919.

How such considerations might apply in less dramatic and obvious circumstances can be illustrated by a brief discussion of Maurice Zeitlin's treatment of political generations in his *Revolutionary Politics and the Cuban Working Class*.<sup>55</sup> Zeitlin's work is refreshing because it breaks out of the usual class and cultural boundaries of American

<sup>53</sup> For practical reasons the scope of panel studies is quite limited. One often-cited example is Erland N. P. Nelson, "Persistence of Attitudes of College Students Fourteen Years Later," *Psychological Monographs*, 68 (1954): 1-13. Although the time span and population examined are limited, the study reveals the care with which variables should be controlled to be able to draw any conclusions regarding collective shifts in opinion, even about the panel that was resurveyed.

<sup>54</sup> Evan, "Cohort Analysis of Survey Data," 72. Glenn and Zody (in "Cohort Analysis with National Survey Data," 239) argue that cohorts (that is, surveyed samples of a larger population) are preferable to panels "with a local or otherwise restricted sample in which changes in the sample cannot be related to changes in the total population." However, Nelson's study shows how, in principle at least, one might contrive controls for local deviance and for national trends. As I point out above, the relatively stable structure of recent American cohorts, which have no significant emigration or immigration, cannot be assumed for other times and other places.

<sup>55</sup> Maurice Zeitlin, *Revolutionary Politics and the Cuban Working Class* (New York, 1970), 211-41.

public-opinion research to investigate the political self-definition of Cuban working-class generations. He finds that successive age groups of Cuban workers retained the stamp of the historical circumstances prevailing at the time of their entrance into the labor force. Aging did not correlate with decreasing militancy. For example, those who entered the labor force from 1928 to 1935 during a period of militancy and Communist leadership remained, in 1962, significantly more sympathetic to the revolution and to communism than those some fifteen years their junior who received their political baptism during the 1940s under different circumstances.

Zeitlin's research was necessarily based on limited data. Such a problem as the adequacy of his sample is not our concern, but the force of his generational conclusions is weakened by certain other, probably insurmountable, limitations. Since he did not investigate age-related attitudinal shifts for the entire Cuban population, we cannot know the extent to which specific proletarian cohorts deviated from, or merely recapitulated, shifts of the larger population. An even more basic issue has to do with the stability of the sampled cohorts. Even if Zeitlin had been periodically able to survey samples of his cohorts during the entire era 1928-62 he could not have been certain that there was no relevant migration from the cohorts. This is not a completely abstract quibble because it is at least conceivable that the relatively negative response to communism in the age 36-43 cohort reflects the disproportionate erosion or emigration of those who had been Communist sympathizers in the 1940s.

I believe that these reservations qualify but do not vitiate the plausibility of Zeitlin's conclusions. And I certainly subscribe to his view that "failure to use the generational concept because its empirical demonstration is difficult is detrimental to the analysis of political behavior."<sup>56</sup> The commitment to the goal of empirical demonstration does, however, make some approaches to the problem of generations more plausible than others. The attempt to grasp the essence of the historical process through an analysis of age-specific relationships analogous to the Marxian analysis of class relationships has raised more problems than it can hope to solve. Thus we have not gotten very far with the elucidation of Mannheim's, "the phenomenon of generations is one of the basic factors contributing to the genesis of the dynamic of historical development,"<sup>57</sup> but we might well settle for the elucidation of specific historical phenomena, of the sort suggested by Mannheim.<sup>58</sup>

<sup>56</sup> *Ibid.*, 240.

<sup>57</sup> Mannheim, *Essays on the Sociology of Knowledge*, 320.

<sup>58</sup> See *ibid.*, 290 n: "It is a matter for historical and sociological research to discover at what stage in its development, and under what conditions, a class becomes class-conscious, and

I HAVE ARGUED ABOVE that so-called generational phenomena have to do with age-specific relationships that may or may not matter; that such relationships vary in nature; and that the varieties must be distinguished in order to decide how statements about them might be verified. I have identified the following distinct, though sometimes overlapping, categories in which age-linked differences might constitute significant historical variables:

(1) Recurrent collective behavior is associated with a certain phase of the life cycle. This conception of the "ages of man" has most often been applied to behavior peculiar to youth, or to the presumed effects of aging.

(2) Groups of coevals are stamped by some collective experience that permanently distinguishes them from other age groups as they move through time. Social scientists usually characterize this as a generational as opposed to a life-cycle effect.

(3) Different groups of coevals may simultaneously experience the same significant events but respond to them in distinct ways more closely associated with age than with other variables.

(4) Particular circumstances produce extraordinary temporary differences between age groups, constituting a generation gap. A gap that regularly recurs, however, would actually be a phenomenon of stages in the life cycle, as in the first category described above.

(5) Attributes separating a cohort from older age groups may persist in the behavior of all subsequent cohorts. Then what began as a generational difference eventually characterizes the entire population under a certain age.

(6) Changes in the relative size of cohorts may cause significant temporary or permanent differences, linked to age such as those that result from large age-specific demographic losses.

I believe, without insisting on it, that the other distinctions mentioned in this article can be made to fit more or less comfortably into one of the above categories. They are presented, not as a rigid taxonomy, but to suggest that useful generalizations proceed from appropriate distinctions.

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similarly, when individual members of a generation become conscious of their common situation and make this consciousness the basis of their group solidarity."



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## Statesmen Undisguised

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A Review Article by ROY M. MACLEOD

RAY JONES. *The Nineteenth-Century Foreign Office: An Administrative History*. With a preface by W. N. MEDLICOTT. (London School of Economics and Political Science. L.S.E. Research Monographs, Number 9.) London: Weidenfeld and Nicolson. 1971. Pp. 224. £3.75.

WILLIAM C. LUBENOW. *The Politics of Government Growth: Early Victorian Attitudes toward State Intervention, 1833-1848*. (Library of Politics and Society.) [Hamden, Conn.:] Archon Books. 1971. Pp. 237. \$10.00.

HENRY ROSEVEARE. *The Treasury: The Evolution of a British Institution*. [New York:] Columbia University Press. 1969. Pp. 406. \$12.50.

MAURICE WRIGHT. *Treasury Control of the Civil Service, 1854-1874*. New York: Oxford University Press. 1969. Pp. xxxv, 406. \$13.00.

GEOFFREY KINGDON FRY. *Statesmen in Disguise: The Changing Role of the Administrative Class of the British Home Civil Service, 1853-1966*. New York: Humanities Press. 1969. Pp. 479. \$18.50.

HENRY PARRIS. *Constitutional Bureaucracy: The Development of British Central Administration since the Eighteenth Century*. (Minerva Series of Students' Handbooks, Number 23.) New York: Augustus M. Kelley. 1969. Pp. 324. \$11.50.

GILLIAN SUTHERLAND, editor. *Studies in the Growth of Nineteenth-Century Government*. Totowa, N. J.: Rowman and Littlefield. 1972. Pp. viii, 295. \$12.50.

IT HAS BEEN fifteen years since the appearance of Professor Oliver MacDonagh's article on the "Nineteenth-Century Revolution in Government."<sup>1</sup> In retrospect, and even at that time, it was clear that something of a revolution was then under way in British administrative historiography. By the

<sup>1</sup> O. O. G. M. MacDonagh, "The Nineteenth-Century Revolution in Government: A Re-appraisal," *Historical Journal*, 1 (1958): 56-67. See also his "Delegated Legislation and Administrative Discretions in the 1850s," *Victorian Studies*, 2 (1958): 44-60. Professor MacDonagh's argument is amplified and illustrated in his book, *A Pattern of Government Growth: The Passenger Acts and Their Enforcement, 1800-1860* (London, 1961).

mid-1950s the making of the postwar "welfare state" had quickened a tremendous impetus among British social and economic historians to study and describe the long and winding administrative and legislative paths that had led in the direction of the new and, to preserve the metaphor, "revolutionary" social services. The transformation of British society during and after the war had not ushered in quite the millennium of which social planners had dreamed; it had been, after all, what W. H. Beveridge had called an "English revolution"—silent, or relatively so, and full of compromise. But the processes by which it had begun in the nineteenth century were seen as historically vital. Historians turned to the lessons of administrative history with a new sense of discovery.<sup>2</sup>

With hindsight, this development was highly important to the study of administrative history itself. Traditionally administrative history in Britain had been more or less a subdivision of constitutional history, usually concerned, following Tout and Maitland, with the elaboration of constitutional law or with the legislative chronology by which new departments emerged and old ones expanded or changed.<sup>3</sup> There was, in addition, a genre of memoirs, of "amateur" history in the best sense, including reminiscences of old Whitehall hands, which recounted for younger and amazed generations of hard-worked civil servants those gentler days when the administrative class, like the fountains in Trafalgar Square, regularly played from ten to four.

Although social historians had examined the great extension of legislation and administration in the nineteenth century in the context of economic and social developments, most administrative historians of Victorian Britain looked for guidance to Professor A. V. Dicey's classic *Lectures on the Rela-*

<sup>2</sup> See R. M. Titmuss, *Essays on the Welfare State* (London, 1958); Asa Briggs, "The Welfare State in Historical Perspective," *Archives Europeenes de Sociologie*, 2 (1961): 223; Dorothy Wedderburn, "Facts and Theories of the Welfare State," *The Socialist Register*, 1965 (London, 1965), 127-46; T. H. Marshall, "Citizenship and Social Class," in *Sociology at the Crossroads and Other Essays* (London, 1963), 67-127; John Saville, "The Welfare State: An Historical Appraisal," *New Reasoner*, 3 (1957-58): 3-25; Dorothy Thompson, "The Welfare State: Discussion," *New Reasoner*, 4 (1958-59): 125-30; see also David Roberts, *Victorian Origins of the British Welfare State* (New Haven, 1960); Maurice Bruce, *The Coming of the Welfare State* (London, 1961); Calvin Woodard, "Reality and Social Reform: The Transition from Laissez-faire to the Welfare State," *Yale Law Journal*, 72 (1962): 286-328.

<sup>3</sup> See *The Collected Papers of Thomas Frederick Tout*, ed. F. M. Powicke (Manchester, 1932), and F. W. Maitland, *The Constitutional History of England* (new ed.; Cambridge, 1961). See also the early and the "new" Whitehall series, well-known examples of which include Sir Edward Troup, *The Home Office* (London, 1925); Sir Arthur Newsholme, *The Ministry of Health* (London, 1925); Sir Thomas Heath, *The Treasury* (London, 1927); and Lord Bridges, *The Treasury* (London, 1969). The study of administrative history in Britain owes, in addition, an especial debt to the editors of the quarterly journal, *Public Administration*. See also Robert Moses, *The Civil Service of Great Britain* (New York, 1914); Sir Adair More, "Officials and Policy," *Public Administration*, 5 (1927): 461-70; Emmeline W. Cohen, *The Growth of the British Civil Service* (London, 1941); H. R. C. Greaves, *The Civil Service in the Changing State* (London, 1947); K. B. Smellie, *A Hundred Years of English Government* (London, 1950); and M. W. Thomas, "The Origins of Administrative Centralisation," *Current Legal Problems*, 3 (1950): 214-35.

tion between Law and Public Opinion in England during the Nineteenth Century, first delivered at Harvard in 1898 and published in England in 1905. Dicey tried to outline constitutional developments as mediating critical shifts in law, political theory, and public opinion. His emphasis, however, was placed chiefly on the legislative process rather than on specifically administrative forms, and his intention was to describe trends or movements in ideas, with their corresponding political and administrative characteristics. He outlined in broad terms a highly generalized description of the transition he perceived within British administration from a period in which assumptions were apparently based on laissez faire individualism to a period apparently dominated by "collectivism." Each period, in his view, was distinguished by a particular climate of opinion, derived from an influential thinker or set of ideas. Thus, the period from about 1800 to 1830 he described as one of "Old Toryism" and "legislative quiescence," in which Paley's natural philosophy and Blackstone's legal philosophy combined to endorse and maintain traditional social and political institutions. In Dicey's scheme this period gave way to a period of "individualism" influenced by Bentham and his followers and characterized by legislation designed to extend "personal liberty," political rights, and civil freedoms. This was the "heyday of *laissez-faire*" and of Utilitarianism, when a small intellectual elite within the civil service acquired the status, in James Stephen's phrase, of "statesmen in disguise." The third phase, between 1865 and 1900, saw, in Dicey's terms, a final transition between individualism and collectivism, in which the civil service expanded, the range of state responsibilities were extended, and faith was increasingly placed in the benefits to be derived from state intervention.<sup>4</sup>

For nearly half a century Dicey's interpretation and its assumptions dominated administrative history and the history of social policy. Indeed Dicey's interpretation had an elegant simplicity to recommend it, and for many it neatly summarized received opinion. By the late 1950s, however, new work began to reveal certain flaws in its argument.<sup>5</sup> Dicey clearly recognized the importance of economic and social interests in the making of policy. But his emphasis upon the "Rule of Law"<sup>6</sup> and the work of the Benthamites in influencing legislative opinion led to the neglect of other

<sup>4</sup> A. V. Dicey, *Lectures on the Relation between Law and Public Opinion in England during the Nineteenth Century* (London, 1905), see especially page 22. The influence of Dicey's example, cast against the context of social policy, was brilliantly demonstrated in the series of lectures delivered at the London School of Economics in 1957-58 and published in Morris Ginsberg, ed., *Law and Opinion in England in the 20th Century* (London, 1959). See also G. Kitson Clark, "'Statesmen in Disguise': Reflections on the History of the Neutrality of the Civil Service," *Historical Journal*, 2 (1959): 19-39.

<sup>5</sup> See especially Roger Prouty, *The Transformation of the Board of Trade, 1830-55: A Study of Administrative Reorganisation in the Heyday of Laissez-faire* (London, 1957), and R. J. Lambert, *Sir John Simon, 1816-1904, and English Social Administration* (London, 1963).

<sup>6</sup> See Dicey's introduction to the second (1914) edition of *Lectures on the Relation between Law and Public Opinion*, and see also W. Ivor Jennings, "In Praise of Dicey, 1885-1935," *Public Administration*, 13 (1935): 123-34.

factors activating government in particular areas. Eventually Dicey's explanations were seen to beg crucial questions about the changing form and function of state activity, to ignore the influence of technological innovation and new forms of expertise, to oversimplify the tremendous transformation occurring within the civil service, and to leave unexplained the manner by which departmental and political interests actually contributed to the making of policy.

The first sustained critique of Dicey's work appeared in 1948 in an article by J. B. Brebner. Brebner demonstrated difficulties on several levels: for example, Dicey had incorrectly believed Benthamism to be synonymous with the principles of laissez faire and therefore opposed to the concept of state intervention. Professor Lionel Robbins has since reminded us that if one's reading of Bentham is confined to certain chapters of his works, it is indeed possible to credit Bentham with an extremely negative view of the state. But Henry Fawcett long ago argued that it was not Bentham but the free-trade movement, the Anti-Corn Law agitation, and the writings of Harriet Martineau that fixed laissez faire in the British mind. Furthermore Élie Halévy recognized that Bentham's philosophy embodied both a rationalist philosophy of state intervention in restraint of disorder and civil justice and a postulate of individualism founded on the belief that all individuals in society have an approximately equal capacity for happiness. Halévy added that "Benthamism was the work of a jurist who was by accident an economist."<sup>7</sup>

Brebner saw further that there is a basic fallacy in believing Benthamism to be a monolithic, coherent structure. In the Benthamite legal system the expediency of any act of government must be tested by its consequences for the greatest happiness of the greatest number. In this system, as Robbins explained,

the invisible hand which guides men to promote ends which were no part of their original intention is not the hand of some god or natural agency independent of human effort, it is the hand of the law-giver, the hand which withdraws from the sphere of the pursuit of self-interest those possibilities which do not harmonise with the public good.<sup>8</sup>

<sup>7</sup> J. B. Brebner, "Laissez-faire and State Intervention in Nineteenth-Century Britain," *Journal of Economic History*, 8 (1948): supp., pp. 59-73. Dicey believed that "though laissez-faire is not an essential part of Utilitarianism it was practically the most vital part of Bentham's legislative doctrine and in England gave to the movement for the reform of the law both its power and its character." *Lectures on the Relation between Law and Public Opinion*, 147. See, too, Lionel Robbins, *The Theory of Economic Policy in English Classical Political Economy* (London, 1952), 39. Henry Fawcett, who believed that a government with powers of intervention was like a physician with a dangerous drug, and who deprecated state provision to those who saw it as a "right," nevertheless explicitly stressed that there was "nothing whatever in the principles of economic science to lead to the establishment of any general conclusion with regard to the advantages or disadvantages of State interference." "The General Aspects of State Intervention," in Henry Fawcett and M. G. Fawcett, *Essays and Lectures on Social and Political Subjects* (London, 1872), 33. Finally, see Élie Halévy, *The Growth of Philosophic Radicalism* (London, 1952), 488.

<sup>8</sup> Robbins, *Theory of Economic Policy*, 56.

Accordingly, an acceptance of state intervention in certain circumstances was as important to Benthamism as the doctrine of *laissez faire*; both elements were inextricably connected by the empirical requirements of a pragmatic utility.

Because Dicey largely ignored this interventionist element, it became possible, even easy, to find instances of state activity between the 1830s and the 1860s where the notion of a governing principle of *laissez faire* apparently did not apply.<sup>9</sup> We also know now that many classical economists did not espouse the principle of *laissez faire* as necessarily or invariably the best instrument of social policy.<sup>10</sup> In his introductory lecture at University College, London, J. E. Cairnes suggested that *laissez faire* was

at best a mere handy rule of practice, useful perhaps as a reminder to statesmen on which side the presumption lies in questions of industrial legislation, but totally destitute of all scientific authority. . . . It must never for a moment be allowed to stand in the way of the candid consideration of any promising proposal of social or industrial reform.<sup>11</sup>

In view of the accumulating evidence of government growth from 1830 onwards, Dicey's conception of the "heyday of *laissez-faire*" has not unnaturally lost some of its hold on the work of administrative historians. Nevertheless there were in the late 1950s a very few who chose to ask questions that probed the relationships among political philosophy, pragmatic political demands, new technological requirements, and internal administrative development in the context of the growth of Victorian government. Oliver MacDonagh was one of these few. His article in the *Historical Journal* in 1958 had an immediate effect. In substance MacDonagh set out to discover

what men thought, and what men felt, contemporary practices should be . . . ; what external or overt events directed the current of affairs decisively, or made men fully conscious of the tendencies of their time; what the underlying social and economic pressures and the medical, engineering, and mechanical potentialities consisted in; and what was actually taking place within executive government itself.

<sup>9</sup> For an illuminating discussion of *laissez faire* as myth and metaphor, see H. Scott Gordon, "The Ideology of *Laissez-Faire*," in A. W. Coats, ed., *The Classical Economists and Economic Policy* (London, 1971), 180-205.

<sup>10</sup> Thus J. R. McCulloch: "The Principle of *laissez-faire* may be safely trusted to in some things but in many more it is wholly inapplicable; and to appeal to it on all occasions savours more of the policy of a parrot than of a statesman or a philosopher." *Treatise on the Succession to Property Vacant by Death* (London, 1849), 156. Later in the century Professor J. E. Cairnes wrote that he was "unable to find in the maxims of abstract justice any key to the practical problems of the distribution of wealth," but that this did not justify, *ipso facto*, the arguments of *laissez faire*. "The recognition of private property and freedom of individual industry" must, he thought, "be judged by practical utility." *Some Leading Principles of Political Economy* (London, 1874), 320.

<sup>11</sup> Cairnes, "Political Economy and *Laissez-faire*," in his *Essays in Political Economy* (London, 1873), 244, 251.

He suggested first, that government did indeed grow in the 1850s and 1860s, but that we must look deeply into government departments to see clearly how this occurred; second, that Benthamites had little influence on most policy in this period and certainly less than historians had previously assumed; and third, that a conceptual model could be devised to describe the process of administrative growth that took place in the middle quarters of the century.<sup>12</sup>

"MacDonagh's model," as it was subsequently called, attempted to relate the political, economic, and social circumstances of the early decades of the century to the reforms in government emerging after the Reform Bill of 1832, the Northcote-Trevelyan report, and the Crimean War.<sup>13</sup> His work aroused some explosive opposition from those who saw the claims of ideas, opinion, and especially Benthamism being written out of history,<sup>14</sup> and the forces of an internal logic of legislation and administration rising in their place. This debate continues, but few can doubt that in his attempt to reach beyond traditional constitutional history MacDonagh precipitated much useful discussion about the explanatory validity of "models" and "phases" in historical writing<sup>15</sup> and about the analytical value of legislative and administrative evidence. This discussion occurred at about the time when many young British historians were beginning to experience the heady wines of sociological and economic analysis, borne by the new Bacchae of the social sciences. Even when the historiographical debate in MacDonagh's wake seemed to spend more time upon the semantics of description than upon matters of evidence or interpretation, the subject commanded lively

<sup>12</sup> MacDonagh, "Nineteenth-Century Revolution in Government," 54, *passim*. The importance of these questions was stressed in S. E. Finer, *The Life and Times of Sir Edwin Chadwick* (London, 1952), and has since been repeatedly argued by G. Kitson Clark, *The Making of Victorian England* (London, 1962); Lambert, *Simon*; and W. L. Burn, *The Age of Equipoise* (London, 1964).

<sup>13</sup> MacDonagh's model was based on five stages of administrative change and, though not intended to correspond in detail to particular cases, was based largely on his earlier work on the regulation of emigrant traffic and was intended to provide a suggestive way of viewing other examples of government intervention in this period. The first stage described the exposure of a social evil and the passage of initial, often permissive legislation to remedy it. When such legislation was found inadequate or ineffective, special officers were appointed to enforce it, and a second stage ensued. A third stage appeared when the special officers themselves began to respond to the needs of their posts by pressing for further legislation and for a superintending central body. In the fourth stage a dynamic conception of administration replaced a static one, as administrators ceased to regard their problems as soluble simply by the addition of more legislation and additional staff. This gave way to a fifth stage, when the officers took upon themselves a wider ambit of administrative discretion and the administration itself became merged with the body of government.

<sup>14</sup> Among the earliest criticisms of MacDonagh's model was that by Henry Parris, "The Nineteenth-Century Revolution in Government: A Reappraisal Reappraised," *Historical Journal*, 3 (1960): 17-37. See also Jennifer Hart, "Nineteenth-Century Social Reform: A Tory Interpretation of History," *Past and Present*, no. 31 (1965): 39-61. For evidence of the extent in which this "Benthamite vs. anti-Benthamite controversy" had developed by the mid-sixties, see Robert Gutchen's review of Lambert's *Simon* in *Victorian Studies*, 8 (1964): 82-83.

<sup>15</sup> An excellent summary of the debate and its limitations appeared in Valerie Cromwell, "Interpretations of Nineteenth-Century Administration: An Analysis," *Victorian Studies*, 9 (1966): 245-55.

attention. In the university context alone, administrative history appeared to come out of a deep sleep and at last seemed something one could literally discuss with one's students, rather than a dusty, quasi-theological canon that was dutifully learned by rote for the routine catechism of finals.<sup>16</sup>

But this was just the beginning. Over the next decade MacDonagh's article helped to focus attention on what he called "self-generating bureaucratic growth" and stimulated many historians to consider seriously what the expression "administrative growth" really means. A new world of metaphor opened before us. In what sense, for example, is administration properly described as an "organic" phenomenon, historically capable of analysis in terms of "behavior," "motivation," and the influence of competing pressures? Alternatively, to what extent is administration, in Lord Haldane's classic expression, best considered in terms of the "machinery of government"—a complicated set of mechanical instruments constructed by constitutional clockmakers working to principles of nearly Newtonian universality, with logical internal requirements for satisfactory performance and with a delicate balance easily upset by sharp changes in political temperature and pressure? And where in one's administrative historiography does one include the influence of political philosophies or of religious, social, or economic pressures? And what of the civil servant and the politician, and the none-too-clear relationship between them? Where must the administrative historian place his emphasis?

Such were some, at least, of the questions that had emerged by the mid-1960s. It was then already clear that the range of detailed studies was still too narrow to generate even what R. K. Merton might call "second-order generalizations." We had excellent work by a small group of scholars including G. S. R. Kitson Clark, Jennifer Hart, Henry Parris, R. J. Lambert, and S. E. Finer, but the range of central—let alone local—government activities amenable to study was enormous. In any case, it was by no means clear that the mere accumulation of new examples would lead inductively to a new or in some sense objectively more accurate picture of government growth. Indeed, the more detailed studies that emerged, the more one began to feel that MacDonagh's model, far from being a finished blueprint (which he, of course, would not have claimed) was little more than a builder's sketch. By the late 1960s the graduate students in Britain and abroad were given the task of articulating this new historiography. The study of different government departments and of different aspects of policy began to reveal the rich variety and density of the subject. Students were set to unraveling the intricate connections between law and lawmaking opinion, between

<sup>16</sup> One is tempted to draw a parallel with the impact of another "revolutionary" study—Thomas S. Kuhn, *The Structure of Scientific Revolutions* (New York, 1962)—upon the intellectual assumptions of a different field. It is perhaps not too much to suggest that MacDonagh's work, like Kuhn's, helped precipitate a "paradigm shift" in theoretical areas of problem choice and interpretation.

social change and civil-service reform, between expertise and administration, and between departmental policies and party politics.

Broadly speaking the books under review reflect this new industry. On the one hand, there has been a substantial monographic literature arising (some would say too quickly) from Ph.D. theses;<sup>17</sup> subsequently we have had a number of extended biographies, articles, and books, often themselves building upon graduate theses and reflecting additional years of hard work at the rock face of records in Chancery Lane. More recently there have appeared general textbooks that summarize recent work and render it more accessible to historians and students of public administration. Finally, thanks to the efforts of Dr. Gillian Sutherland, a colloquium, organized under the auspices of *Past and Present* and attracting sixty people, has resulted in a series of extremely valuable essays.<sup>18</sup> This evidence—all most welcome—of serious interest in the subject provides a convenient opportunity for an interim stocktaking of the work in hand.

INTO THE CATEGORY of recent monographs come the works of Dr. Ray Jones and Dr. W. S. Lubenow. Dr. Jones's study of the Foreign Office, the first historical volume in the LSE Research Monograph series, follows developments in that department through the eyes of the staff, from the administrative reforms of 1848 to the "Great Reforms" of 1906. Despite its obvious political importance, the Foreign Office has held few charms for administrative historians. It has the reputation of being intellectually isolated and mildly reactionary, stuffy and crotchety; even its accommodation—in "dark offices and labyrinthine passages, four houses at least tumbled into one, with floors at uneven levels and wearying corkscrew stairs that men cursed as they climbed"—gives the impression of inertia, meanness, and indifference to public criticism. It is not surprising to learn that the Foreign Office was the last department to which administrative innovations were applied. To its credit, its officers could claim particular success in standing up to the Treasury—a novel virtue, one might think, important to preserve at all costs—but this was hardly to their credit if the costs were too great. Probably they were. Dr. Jones well describes successive attempts to develop principles of administrative integration and devolution and concludes with an informed discussion of the Foreign and Diplomatic Services.

A difficulty arises, however, when one closes the book and asks oneself what one has learned from Dr. Jones about the *business* of the Foreign Office. What effect, for example, did administrative change have on depart-

<sup>17</sup> It is a revealing sign of the growing professionalism of administrative history that five of the seven books under review owe their origins to graduate theses.

<sup>18</sup> See Gillian Sutherland, "Reform of the English Civil Service, 1780-1914: A Project for a Colloquium," *Past and Present*, no. 42 (1969): 163-65; Gillian Sutherland, "Recent Trends in Administrative History," *Victorian Studies*, 13 (1970): 408-11.



mental policy and objectives? This leads to a larger question: can administrative history at its best be confined solely to structure and organization and formal rules of competition? What of the old risk of isolating form from content, structure from substance? What of considerations of morale, administrative leadership, and bureaucratic innovation? What of the political attitudes of civil servants themselves? One recalls the anti-German (and to a lesser extent, anti-French) bias of the higher permanent staff and Sir Warren Fisher's famous attempt to place an anti-German in the post of permanent secretary at the Foreign Office in 1939. Given that the duty of the first division (now the administrative class) in the last analysis was (and is) to advise its ministers, such questions are not trivial. Dr. Jones's study shows an excellent grasp of technical matters, but it should be read in conjunction with Dr. Zara Steiner's *The Foreign Office and Foreign Policy, 1898-1914* (1969), with which it will inevitably be compared. One must read both together to come away with the sense that the Foreign Office was, after all, staffed by living beings and that, however removed from the limelight of national or international political events, the substance of their views was critical to the efficient execution of business and perhaps to the direction of policy.

Dr. Lubenow's book draws upon a broader canvass. Looking above the tempest of day-to-day administrative issues, Dr. Lubenow discusses what he believes to be influential political attitudes toward government growth in Britain between 1833 and 1848. He has made a thoughtful and in many ways courageous attempt to grasp the elusive explanations of intellectual influence that Dicey failed to cement and to propose a view of governmental growth that incorporates different models of political behavior. Dr. Lubenow's attitude toward the "great debate"—"resurrecting old and outworn controversies for antiquarian purposes"—is perhaps unfair, given that this assumption is by no means universally shared. A more significant problem in this work is its use of a strange meta-language of political analysis, making it sometimes difficult for the reader to perceive beneath its surreal imagery the plain, detailed landscape of British history. For example, Dr. Lubenow's discussion of what he describes as the "attitudinal pattern of Victorian Britain" is weakened by the absence of an attempt to prove the prior assumption—that there is such a thing as an "attitudinal pattern," that Victorian Britain (however defined) possessed it, and that in some way it had a mysterious power no less disturbing than Banquo's ghost. Dr. Lubenow also tends to reify the notions of "thought" and "opinion" and to consider them as variables somehow independent of the people who actually held them. The difficulties inherent in making tangible the elusive elements of *Zeitgeist*—including that most difficult of all legacies from Dicey, the "spirit" of lawmaking opinion—are not resolved; they are merely replaced by newer sociological concepts of equal turbidity.

A similar difficulty emerges when we try to use what Dr. Lubenow de-

scribes as two possible models of Victorian political behavior: the so-called organic view of politics and the so-called incrementalist view. Dr. Lubenow appears to argue that the incrementalist view provides a *via media* between the interpretation of government growth as either a "carefully prepared blueprint for political change" or a "consequence of a comprehensive and exhaustive policy analysis." But as neither alternative interpretation appears very probable, and as no one to my knowledge has ever proposed either, the problem (and thus the solution) seems false. One is inclined to credit Dr. Lubenow's thesis that Victorian government growth did not lead ineluctably to a welfare state and that there was much less opposition along theoretical lines than Dicey might have supposed; but neither thesis is strengthened much by shaky references to a notion of incrementalist growth.

In his study Dr. Lubenow relies heavily upon blue books and parliamentary debates to support his conclusion that Victorian attitudes toward state intervention were conditioned by the "structure and assumptions of traditional political forms" rather than by "socialist attempts to create a welfare state." One might question on methodological grounds whether the study of traditional political sources could really lead to any other conclusion. One is led to suppose, and not only from his bibliography, that Dr. Lubenow has not made use of unpublished departmental files or correspondence. But surely the real difficulty is the way in which the argument is set out: we do not come away from this book much wiser about the nature or the significance of intellectual beliefs or public demands in the formation of policy.

Within the category of more extensive studies fall two excellent treatises on the Treasury and Treasury control, by Dr. Henry Roseveare and Dr. Maurice Wright. As James Stansfeld, financial secretary in Gladstone's first government, once said, the Treasury is a "useful department to be in; it gives you the clue to most others." Dr. Roseveare's work, already widely praised, gives us several clues to the writing of good administrative history. His book is an extremely successful example of an approach that blends the study of administration with the study of policy and casts both in a fresh light. Dr. Roseveare accepts the premise that a history of the Treasury, properly conceived, "could hardly fall short of being . . . a social, an economic, and a political history of Britain." In his hands the Treasury, as an institution, comes alive—born of the specific needs of a medieval monarchy; its growth dictated, first by royal convenience, then parliamentary influence; its stature sometimes eclipsed, sometimes strengthened by its political leaders and its permanent heads; its functions slowly, even passively, changing in war and peace, from an eighteenth-century ideal of a "balanced constitution" to the Keynesian and post-Keynesian assumptions of a "managed economy."

Dr. Roseveare describes in rich detail the growing functions of the Treasury—the drafting of legislation, the administration of revenue, the overall

control of expenditure, and ultimately the management of the civil service. At the same time he sees the weaknesses—personal, administrative, and political—that by the mid-nineteenth century rendered the Treasury impotent to place more than a temporary brake on the growing expenditure of departmental government. Ironically it was largely this crippling impotence, reflected in its apparent preoccupation with the parsimony of “candle-ends” accountancy, that shaped the Treasury’s public image. Yet it was chiefly the sense of innate moral superiority that comes with a balanced budget, a sense of property and propriety, and an ability to shift fiscal needs from political expedience that gave the Treasury its special strength. Moreover, fostered from the late seventeenth century by such men as the eponymous Sir George Downing (an early gift of Harvard College to British financial history), the Treasury established a hallmark in administrative order and elegance that set precedents for all Whitehall. Yet from time to time, and vividly by 1900, the weight of its moral pride in “Lingensism” and in monetary orthodoxy as well as the sheer intellectual aloofness of its senior clerks left the heavily armored Treasury moored outside the mainstream of radical innovation in the range and effectiveness of civil government. To political and social reformers the Treasury, and particularly the bulwarks of Treasury control, stood as an obstacle instead of a defense. With the new century and the sharp transformations of two world wars came new generations of civil servants and a new recognition that responsibility for public expenditure must be a “managing” responsibility. The Treasury’s twin functions of finance and management were by then inseparable, but they had to pull in harness in response to changing political perceptions of public demands. Dr. Roseveare, with a becoming modesty that would profit the Treasury itself, traces its growing responsibility through its several reorganizations, its dealings with the Bank of England and the National Economic Development Council, up to the dark, ambiguous days of George Brown (now Lord George-Brown) and the ill-fated Department of Economic Affairs.

It is only perhaps in his concluding pages, devoted to the Fulton Commission’s report, that Dr. Roseveare rises with a defending vengeance to argue the Treasury’s side against the alleged tendency to promise “change for change’s sake,” which he suggests will inevitably follow Fulton. Dr. Roseveare concludes, however, that “time’s argument in all this will prove more interesting than any historian’s.” He is still right. Today, five years after Fulton, the Treasury seems to have weathered the storms of criticism. One cannot leave the vast Edwardian monument in Great George Street without a grudging admiration for its sheer tenacity.

If Dr. Roseveare has woven a broad tapestry, Dr. Wright provides much necessary needlework. His purpose is to suggest that the role of chief villain in the story of nineteenth-century administration is not by rights the Treasury’s. Restricting himself to the years between the Northcote-Trevelyan

reforms and the Playfair Commission, he probes deeply into the questions of organization, control of establishments, conditions of service, and Treasury control. His accomplishment is solid, giving vital details concerning the "department of departments" in cross-sections of crystalline clarity. No student of Victorian administrative history can afford to neglect the sheer substance of establishment debates; the endless difficulties of salary, office hours, and discipline; the interminable negotiations with different departments and different political chiefs over the implementation of open competition; and the conduct of Treasury committees of inquiry.

In Dr. Wright's opinion the traditional view of the Treasury as financial watchdog has been greatly oversimplified. He argues convincingly that if the Treasury behaved in a niggardly way in its formal dealings, this was merely what Parliament, indeed the public, asked of it. Even in this the Treasury was not wholly successful, as pleas for improved salaries or increased departmental provision were often granted, if only after much emotional bloodshed. Retrenchment proved a good slogan but an elusive reality. Likewise, we are told, the Treasury was not completely hidebound by its rules and precedents; indeed it frequently found expediency more attractive than principle. Ministers (for example, A. S. Ayrton) who persisted in injurious cheeseparing were sometimes sacked.

Dr. Wright suggests that the Treasury was not really hardhearted; perhaps it was, at worst, the villain with the heart of gold. Besides, other Treasury activity provided a flexible and cheap device for conducting government business. This was certainly true of the system of Treasury committees. Moreover, the Treasury's attempts after 1854 to combine economy and efficiency, to rationalize the service, and to eradicate "abuse," jobbery, and favoritism were in principle laudable. "Beneficence combined with flexibility" might have been a Treasury code, and the avoidance of dispute in the interests of conciliation was to be its distinguishing characteristic. It must be said, however, to anyone looking at the Treasury from outside, particularly from the so-called second-class departments (including the Board of Trade, the Local Government Board, the Board of Agriculture, and the Science and Art Department), in which so much of the country's new work of government was taking place, the Treasury's mandarinism seemed remarkably imperious. Dr. Wright provides an exemplary defense, but surely the case cannot be said to be complete without hearing the views of the plaintiffs.

This one-sidedness is perhaps the one weakness underlying Dr. Wright's otherwise solid edifice. It does not, however, diminish his achievement to suggest that the book loses by omitting vital mention of the eternal and detailed cut and thrust between the Treasury and the departments and the sense of conflict that demoralized so much of Victorian public service. Administrative historians know too well how negotiating often degenerated into haggling, where "conciliation" was a euphemism for weary acquiescence,

where "beneficence" was a charade for polite bludgeoning, and where assertions of "administrative flexibility" could be mere rationalizations of defeat.

These issues, fundamental to the business of administrative history, remain when one turns to Dr. Fry's book. This vast product of an enlarged Ph.D. thesis begins with a "general argument" of three hundred words, a section that, for many, may replace the reading of the rest of the volume. Dr. Fry's work is not limited to historical analysis, even though his book will doubtless form a useful contribution to the historiography of the subject. His chief mission is to use historical arguments to support certain related contentions: that the regulatory state of the nineteenth century has now been succeeded by a welfare state; that modern government in the welfare state rightly places a premium on management, specialized knowledge, and research; that the acceptability of the so-called all-rounder in the administrative class is now much diminished; that in the interests of better government the administrative class and the executive class should be merged into one; and that greater mobility among staff between all sections of government should be encouraged.

Dr. Fry devotes only his first chapter to the history of the civil service before 1900 and spends the remainder of his time discussing the background and shortcomings of civil-service recruitment and training and the neglect shown the "specialists." In so doing he makes much thorough use of secondary literature and published government reports. But as his main concern is to recommend change in the organization of government as it affects the service today, he does not give great space to specific issues of legislative or administrative control or to personal leadership within the existing system. Indeed there is much about his book that will dissatisfy historians: his level of generality, his assumption of a high order of homogeneity in a system that thrives (in spite of the Treasury) upon its diversity, and his neglect of the "fine structure" of internal governmental debate that can only come with the detailed analysis of specific departments or decisions or acts of Parliament.

Yet Dr. Fry's discussion does raise several important questions. To what extent, for example, was the typical mid-Victorian civil servant really an all-rounder? Which departments in the last half of the nineteenth century could really function without a good measure of hardheaded specialist advice of a legal, technical, or financial nature? How important were the notorious watertight barriers between departments and within individual departments in contriving a series of pragmatic specializations? How deleterious were these barriers to the work of individual departments? How much more innovation came about through the use of patronage and specialist appointments, both permitted to a significant extent throughout Whitehall until well after 1870?

That Dr. Fry is on the side of the angels of the moment is evident from his obvious preference for the specialist over the all-rounder, from his

disagreement with the position taken by Lord Bridges, and from his views on the Fulton report, to the criticism of which he devotes a huge post-script of nearly sixty pages. But mere assertion will not exorcise the ghosts of traditionalism from the administrative class, nor is it a foregone conclusion that the British civil service, for all its manifold deficiencies, will be renewed to grace by the straightforward acceptance of Dr. Fry's prescriptions.

As a textbook welcome to both students and administrators, Dr. Henry Parris's recent *Constitutional Bureaucracy* is a particularly valuable contribution to the literature. Dr. Parris's early participation in the "nineteenth-century revolution" debate is well known, and his study of *Government and the Railways in Nineteenth-Century Britain* (1965) has enjoyed a wide readership. His present book, clearly the result of almost a decade of scholarship (his own, no less than others) devotes most of its space to the discussion of specific issues in the rise of a "constitutional bureaucracy" as a counterpart to constitutional monarchy. These issues, including patronage, ministerial power and responsibility, and the "grey eminence" myth, are set against the relationship among legislation and administration, Treasury control, and the relevance of public administration to our conception of public accountability and "representative bureaucracy." Dr. Parris's book is a careful distillation, concisely written. His dissection of Dicey, Halévy, and MacDonagh in chapter nine is extremely serviceable. One only wishes he could have devoted more space in this chapter to reviewing the ramifications of the old debate, particularly the relationship between law and opinion, the character of this relationship in a given period, the nature of laissez faire, the rationale of legislation in health, transport, industrial regulation, and education, and the important differences existing within "old" and "new" departments of state. If Dr. Parris's conclusions stand out in measured similarity to his views of 1965, they do leave the debate much more accessible. One is led to see that the old nettle of collectivism was rather a red herring—an *idée fixe* in Dicey's interpretation of social history. In fact, state intervention and laissez faire were not necessarily incompatible; on the contrary, they were contingently related, and in practice individuals could reasonably espouse both.<sup>19</sup> All depended on the circumstances of the case.

We turn finally to the recent work edited by Dr. Gillian Sutherland. It is a pleasure to welcome to the growing canon of new scholarship these ten essays that reflect an extremely animated sense of historical enterprise. Important generalizations that emerge from the essays again revolve round the questions that have informed the whole discussion—the role of political ideas in government growth (ideas versus pragmatism), civil-service reform and the strong personalities of different departments, the role of "zealots"

<sup>19</sup> See Gordon, "Ideology of *Laissez-Faire*."

and experts as innovators and as agents of legislative change, and the interpretative view of politics and the state that all this implies.

To the first question, both Professor Samuel Finer (whose paper alone did not originate at the *Past and Present* colloquium) and Dr. Alan Ryan address themselves precisely. Professor Finer's immensely provocative use of scientific imagery to describe the process by which the political ideas of Bentham, aided by men of influence, were projected into society—a three-fold process, as he calls it, of "irradiation, suscitation and permeation"—has the immediate appeal of elegant simplicity. Dr. Ryan's study of J. S. Mill's views on the concepts of sound knowledge and good government provides an extremely useful introduction to the context of administration in India and Britain and to the transfer of administrative experiments from one of Britain's largest "social laboratories."<sup>20</sup> Ryan, like Finer, argues for the importance of political theory in the causation of legislative change, but, unlike Finer, he does not insist upon basing his analysis on a single like-minded group of Utilitarians, possessed of a system of homogeneous beliefs. Instead, in his view legislative change often followed the charge of an "intellectual cavalry" riding a generalized set of intellectual assumptions that could help bring about administrative change by helping to instill moral changes in the perception of administrative objectives. While their conclusions may be debated, both Finer and Ryan will become required reading. So far it is clear that Benthamism and the particular impact of Utilitarian radicalism up to 1850 will continue to be the point at issue. It will be interesting to see how the argument will develop when intellectual undercurrents of lawmaking opinion in the last half of the nineteenth century—what Melvin Richter calls the "politics of conscience"<sup>21</sup>—come under the microscope.

With the essays of Jennifer Hart and A. D. Donajgradski on the Northcote-Trevelyan report we think ourselves on familiar ground until we are gently led to question conventional assumptions about the effective nature of patronage (including its class connotations) and the real purpose of financial reform. From Mrs. Hart's point of view civil-service reform was fundamentally an internal development following with inexorable logic from the combined desires of Trevelyan and Gladstone for efficiency and economy and for a "purer and more strenuous ethic" in public life. Although writing from a different point of view Donajgradski, in his case study of the Home Office in its reactions to the Northcote-Trevelyan report, concludes with Mrs. Hart that the real contest was between patronage and efficiency and between the supposed negative values of the one and the virtues of the other. Donajgradski goes rather further toward destroying the mystique of the Northcote-Trevelyan report by disclosing the im-

<sup>20</sup> As W. L. Burn once tantalizingly described Ireland, "Free Trade in Land: An Aspect of the Irish Question," *Transactions of the Royal Historical Society*, 4th ser., 31 (1949): 68.

<sup>21</sup> See Melvin Richter, *The Politics of Conscience: T. H. Green and His Age* (London, 1964).

balance of its extremely biased arguments. We have, of course, long been released from any reason to believe in blue books as impartial collections of value-free observations and balanced reportage, but it is good to be reminded not only of the value-laden character of the Northcote-Trevelyan report but also of its consequences. In the light of Donajgrodski's conclusion that the report "failed to describe the old system accurately . . . defined non-existent problems and proposed new clerical roles of doubtful utility," our view of it will never be the same again.

The remaining essays by Dr. Sutherland and Dr. Richard Johnson on education, by Dr. Snelling and Dr. T. J. Barron on the Colonial Office, by Mrs. Valerie Cromwell and Dr. Zara Steiner on the Foreign Office, and by Dr. Roger Davidson on the Board of Trade all demonstrate careful interest both in the internal dynamics of departments in relation to social and political demands and in the notion of departmental "personalities." In a reflective introduction Dr. Sutherland concludes that state action, and consequent government growth, was virtually always seen, even by those whom Henry Parris calls zealots, in a relatively negative light, in the sense that the state was urged to act only when attempts at local, voluntary, and individual action had failed. In a comparative European context there were few ideological collectivists in British government in the period from 1870 to 1914, and the starting point for state intervention was clearly remote from the centralized assumptions of France and Germany. It could be valuable to compare Britain with France and Germany in this period to discover how external influences of humanitarianism or science could combine with internal departmental and parliamentary concern for efficiency and economy, or with the more straightforward concerns of politicians to be re-elected, in order to produce a dramatic "interventionist" effect on a representative political system that prided itself on its individualism.

THESE SEVEN BOOKS give us a sense of the current preoccupations and expectations of administrative history. The nature of the research program is becoming clearer, and several outstanding tasks await the historian. First, it is generally agreed that studies of government departments, as such, cannot wholly succeed unless they grasp both the details of office and the objectives for which those offices were created—that is, policies and machinery must be assessed together if we are really to make sense of either, and both must be seen, as Valerie Cromwell has reminded us, against a larger background of political maneuver.<sup>22</sup>

It follows that the notion of administrative history as beginning where the "interesting" history of political turmoil ends can now be safely buried.

<sup>22</sup> Valerie Cromwell, "Interpretations of Nineteenth-Century Administration."



As Royston Lambert once observed, historians have repeatedly made the mistake of assuming that once a piece of legislation was on the statute books the precedent was created and the rest was mere routine. We now know that it is profoundly wrong to confuse the passage of an act with the actual execution of policy. Jennifer Hart makes the same point in a different way when she reminds us that it was a mistake to see in the Northcote-Trevelyan report pressures that existed only after the report appeared.

It is also certain that interest in administration will encourage new work on both the history and the mechanics of legislation. In this direction one of Professor MacDonagh's most valuable contributions was to describe not merely legislation but directed sequences of legislation, called forth in what may become quite explicable patterns, moving with the aid of reformers' opinion to specific enabling legislation, then with administrative opinion to tighter or more general enabling legislation and the appointment of inspectors, then to compulsory legislation, consolidation, devolution to local authorities, or even repeal. Future work should consider different kinds of general and special legislation and the relationship between certain legislative forms and political objectives. Some acts, we know, were intended to re-establish a status quo that the influence of changing circumstances had perversely distorted. They consisted, to paraphrase Henry Buckle, "not in doing something new, but in undoing something old." Such legislation attempted to "restore things to the natural channel from which the ignorance of preceding legislation has driven them." This attitude, for example, governed the assumption of the *Times* in 1869 that

people may, in a general way, be trusted to govern themselves and manage their own affairs, and that a State can do little more than render some formal and official assistance, chiefly in the removal of obstruction. There are evils that impede the current of affairs and stop the circulation. The State can do little more than clear the thoroughfare and bid all to "move on."<sup>23</sup>

In contrast other acts illustrated new legislative assumptions, some of which embodied the explicit extensions of government responsibilities for technical reasons or for reasons of social and economic justice. In the one case the presence of the state was felt desirable where private initiative had not sufficed; in the second the state was asked to extend standards of hygiene and education into areas where previous action had no effect.

Such fresh administrative and legislative studies will unavoidably rest on a more thorough knowledge of departmental history. As Dr. Roseveare's work demonstrates, the study of administrative history need not be obsessed with the artifacts of government—the "seemingly inert materials" of writs, seals, and minute books. These inert materials can be brought

<sup>23</sup> Henry Buckle, *History of Civilisation in Ireland* (London, 1872), 1: 375; *Times* (London), Aug. 10, 1869.

to life when the faceless image of administration is given flesh and color. We know now that, whatever the formal position may have been, certain civil servants and their associates could play an important role in "innovational bureaucracy." It remains for future research to show us how innovations could be encouraged, or resisted, and how their success bore upon the goals of the department.

In this context much remains to be learned about the reception of expertise. Much has been written about the eternal contest between those two famous ideal types—the generalist (or all-rounder) and the specialist (or "professional")—the one educated in law or classics and inclined to keep things running smoothly, the other keen with enthusiasm for righting wrongs and possessing the knowledge with which to do so. This conflict did exist, as the histories of Edwin Chadwick, John Simon, and Angus Smith and the vivid stories of scientists in the War Office and the Admiralty amply recall. But historians must not be deceived into thinking that such a polarization always occurred or that there was, *pace* Dr. Fry, always a clear division of opinion about the merits of either caste. The picture is much more complicated. For example, there are distinct generational phenomena to be taken into account. Within the civil service, patterns of administrative behavior changed decisively between 1870 and the turn of the century, and the growth and differentiation of the service sometimes militated for and sometimes against the rapid acceptance of new ideas. Again, to take another example, one finds that the "heroic age" of a particular species of expert could be quickly succeeded by a period of apparent quiescence and consolidation.<sup>24</sup> Experts, as Michel Crozier once observed, may well only remain heroic as long as they are on the leading edge of policy.<sup>25</sup> Once their advice is taken and assimilated into the system, many become merely another part of the system, defending their own group or professional interests. This seems to have taken place among the lawyers, the doctors, the engineers, and, at least since the Second World War, the scientists. Who is to say that it is not now also occurring to our most modern zealots, the economists and the planners?

It will be interesting, therefore, to explore the conditions of the period from 1870 to 1914 during which generalists and experts in most departments had begun to reach a *modus vivendi*. Possibly one will see, as Richard Johnson argues, the "zealous expert" giving way to the "administrative expert." One has increasing evidence that by the middle of the nineteenth century, specialist technical knowledge (and even research) was becoming accepted as a prior condition of action, and that, conversely, departments

<sup>24</sup> See Roy M. MacLeod, "The Alkali Acts Administration, 1863-1884: The Emergence of the Civil Scientist," *Victorian Studies*, 9 (1965): 85-112; "Social Administration and the 'Floating Population': The Canal Boat Acts, 1877-1899," *Past and Present*, no. 35 (1966): 101-32; and "Government and Resource Conservation: The Salmon Act Administration, 1861-1886," *Journal of British Studies*, 7 (1968): 114-50.

<sup>25</sup> Michel Crozier, *The Bureaucratic Phenomenon*, tr. by the author (London, 1964), 165.

realized (and advised their ministers) that legislation could proceed no faster than the science of a particular subject permitted. By the turn of the century there were signs that scientific and specialist expertise, within limits, was becoming part of the accepted orthodoxy. This, in turn, began to raise new problems of parity for the experts and even greater problems of ensuring a steady flow of the best experts from the new universities into Whitehall. But what were the limitations placed on government policy by the state of knowledge? And what became of the zealots?

Such questions remain to worry us. And no less important are the questions still posed by the rubric of "Treasury control." Whether and how the Treasury in fact "controlled" expenditure is now better known, thanks to the work of Dr. Wright and Dr. Roseveare. We know that the application and principles of control were by no means uniform from department to department, that they were often no more than delaying devices, and that they could be overruled by personal intervention at ministerial level. One important goal of future research must be to determine how, in practice, the Treasury reacted to requests of different kinds, what role "personalities" and expertise played in these decisions, and whether the watertight divisions of the Treasury militated against any concept of a broad view of public spending. We especially need a book that will build upon the excellent work of Roseveare and Wright by taking specific issues, in the hands of specific Treasury clerks, through the corridors of specific spending departments, together with detailed vertical studies of individual departments and individual issues of policy and administration. At the same time we need works that will, for specific historical periods, take wider, interdepartmental views across Whitehall and raise issues in a political and economic perspective.

Within this contextual framework we might also give continuing attention to two related issues—the circumstantial characteristics of government activity and the relationship between individual officials (whether generalist or specialist), their political masters, and the public they are meant to serve. A. P. Thornton has recently observed that "ideas in politics, as elsewhere, are forced to fight a grinding battle with circumstance,"<sup>26</sup> and surely no student of the British civil service would deny that the climate in a department has a profound influence on the ways in which policies are executed. Moreover, within the firmament of departments each sphere of government has its own distinctive Pythagorean harmony. Complicated questions of morale, access to the "top," prospects for promotion, intra-departmental tensions and rivalries, and a sense of hierarchy and status are fundamental to an understanding of a department's overall attitude toward its work. It is insufficient to assume that single, universal, and straightforward definitions exist in practice for such concepts as patronage,

<sup>26</sup> A. P. Thornton, *The Imperial Idea and Its Enemies* (London, 1959), ix.

competition, accountability, and responsibility. We know, for example, that patronage was not an unmitigated evil but a flexible, pragmatic tool that could be turned to good effect, whether in the appointment of specialist advisers, or, as Richard Johnson puts it, at the "oldest and highest" strata of administration.

Finally, it is unequivocally clear that no study of the effect of ideas in administration can wholly succeed without considering the actual relationship between permanent officials, their own beliefs, and their political leaders. By the late nineteenth century, permanent secretaries were notoriously self-possessed and wielded considerable power. That they could, and still can, aid or attenuate the policies of even the most strong-willed politician is today common knowledge. But how was this done, and with what effect? This has a timely interest, particularly outside academic circles. Can there be, for example, substance in the belief that in extreme cases the permanent civil servant can thwart the political programs of Number 10 Downing Street, as Mrs. Marcia Williams has recently asserted? On the other hand, has the civil servant become more vulnerable to public criticism through the development of the select-committee system or through such innovations as the Central Policy Review Staff? As Lewis Gunn has written, the notion of "Ministerial responsibility rarely involves the acceptance of culpability by the Minister and . . . even Ministerial answerability is sometimes so diluted as to offer little protection for the anonymity of the official."<sup>27</sup>

During the last century the role of the civil servant has become increasingly visible. At the same time public willingness to accept without question his use of power has been significantly weakened. The character of governmental accountability, through Parliament to the people, is now one of the pressing issues before the British public. Since the Fulton report this issue has been extended to the civil service, whose senior members are now recognized virtually as "statesmen undisguised." This appreciation has given administrative history a new relevance, far removed from the old-fashioned notion of constitutional chronology. Perhaps we are now beginning to perceive that administrative history is not merely about administrative development and structure per se but about the objectives and priorities of public interest and government policy. Indeed can one now see administrative history as a fundamental discipline, helping to interpret structures in the light of achievements, wedding social policy and social administration, and providing a framework for analyzing the social and political basis of power and responsibility in our society.

<sup>27</sup> Marcia Williams, *Inside No. 10* (London, 1972), 344-57; Lewis Gunn, "Politicians and Officials: Who is Answerable?" *Political Quarterly*, 43 (1972): 253-60.

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## De Gaulle as President: First Triumphs and Last Memoirs

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A Review Article by JOHN C. CAIRNS

ANDRÉ MALRAUX. *Les chênes qu'on abat* . . . [Paris:] Gallimard. 1971. Pp. 235. 21 fr.

CHARLES DE GAULLE. *Mémoires d'espoir*. Volume 1, *Le renouveau, 1958-1962*; volume 2, *L'effort, 1962* . . . [Paris:] Plon. 1970; 1971. Pp. 314; 223.

CHARLES DE GAULLE. *Memoirs of Hope: Renewal and Endeavor*. Translated by TERENCE KILMARTIN. New York: Simon and Schuster. 1971. Pp. 392. \$10.00.

PHILIPPE ALEXANDRE. *The Duel: De Gaulle and Pompidou*. Translated by ELAINE P. HALPERIN. Boston: Houghton Mifflin Company. 1972. Pp. xvii, 360. \$7.95.

MAURICE COUVE DE MURVILLE. *Une politique étrangère, 1958-1969*. [Paris:] Plon. 1971. Pp. 499.

"BETWEEN OURSELVES, Malraux, when you get right down to it, is it worth the trouble?" the General asked his visitor one winter day in the final year of his life. "Why write?"<sup>1</sup> Was he serious? Did he merely wish to savor again the delicious historical ruminations of his "brilliant friend, the devotee of lofty destinies,"<sup>2</sup> the oracle of the Gaullist mystique, who, like some world-weary Alexander of the intellect, seemed to have accomplished the meeting of East and West, and who never failed to locate de Gaulle and his epic in the rhythmic progression of great men and creative moments in the history of the world? Did he wish again to hear a discourse on de Gaulle and Caesar as men of action and of letters, on the implications for a hesitant writer at Colombey of the *Mémorial de Sainte Hélène*? If it was not merely a rare entertainment in a quiet country house

<sup>1</sup> Malraux, *Les chênes*, 59; the translation, *Fallen Oaks, Conversation with de Gaulle* (London, 1972), is not without rough and awkward renditions—a very unsatisfactory substitute.

<sup>2</sup> De Gaulle, *Mémoires d'espoir*, 1: 285; translated as *Memoirs of Hope: Renewal and Endeavor* (New York, 1971).

lying in the snow-covered fields of Champagne, was it a reinforcement of purpose that he anticipated, pitched at the universal level?

De Gaulle left office in April 1969; the first volume of his second installment of memoirs was in his editor's hands by July 1970.<sup>3</sup> There could not have been much hesitation. He had remarked of the earlier war memoirs that, unlike Churchill who had "just strung things together," he wished to leave "a literary work."<sup>4</sup> It is fair to say that he did it again, although his oft-repeated query whether God would spare him to complete his task was answered negatively. Whether there is a falling off from the standard of the first memoirs is a matter of opinion. The framework for action was, all in all, less dramatic, and the documentation here is thinner. But in many respects it is the mixture same as before, an intensely subjective reading of a four-year period much written about and still only sketchily known. The two volumes, of which the second, *L'effort, 1962 . . .*, is no more than a fragment appended to the first, *Le renouveau, 1958-1962*, take de Gaulle from the moment of his return to power in 1958 to the 1962 alteration of the constitution by popular referendum. This he was unable to achieve until he had secured his position by ending the war for Algerian independence, destroying the army's political temptations, and overwhelming the opposition of the political parties. It was then that they finally understood that for de Gaulle Algeria was no more than the opening act in the work of reconstructing the state and society. In a sense, then, the manuscript breaks off at the pinnacle of his achievement, what he thought of as the triumph of "hope." "That is also why," he told Malraux, "I'm far from preparing the second volume (let's not mention the third!) with the same feeling."<sup>5</sup>

Time had both altered him and left him much the same. "He has, of course, aged a lot," Harold Macmillan noted in June 1958. "He has grown rather fat; his eyes are bad and he wears thick spectacles. . . . His manner is calm, affable and rather paternal. But underneath this new exterior, I should judge that he is as obstinate as ever."<sup>6</sup> The pose now adopted in the memoirs is again that of the beleaguered hero, opposed by the jealous pygmies of sclerotic institutions (rather than the giants of the vanished war era), supported by the people whenever they can break through to make their will known. Of the celebrated *traversée du désert*, 1946-58, with all its frustrations, interventions, embittered silences, and philippic predictions of the wrath to come, little is said here. The Fourth Republic is written off in fourteen pages as a kind

<sup>3</sup> Jacques Chapus, *Mourir à Colombey* (Paris, 1971), 224.

<sup>4</sup> J.-R. Tournoux, *La Tragédie du général* (Paris, 1967), 233.

<sup>5</sup> Malraux, *Les chénes*, 192-93.

<sup>6</sup> Harold Macmillan, diary, June 29-30, 1958, in Macmillan, *Memoirs* (London, 1966-72), 4: 448.

of mad comedy in which powerless French heads of state presided over a nonstop political ballet, with only its industrial progress, owing to de Gaulle's own initiatives before the 1946 resignation, worth commending. The unfortunate *le Rassemblement du Peuple Français* experience is dismissed summarily as a victim of the malevolence of the parties, the government, the unions, business, and the press. Thus the crisis of May 1958 is reached, for obvious reasons, at a gallop. Jean Monnet once remarked of de Gaulle that "he has an odd technique. He always creates problems in order to solve them."<sup>7</sup> This may not be entirely fair, but the memorialist unwittingly suggests that there is some truth in the proposition.

Grave problems there certainly were. Whether he expected to return to power remains an enigma now as before.<sup>8</sup> His position is that he had nothing whatever to do with the emerging crisis in Algiers and Paris during the spring of 1958, though he realized at once that he must take hold as "l'instrument désigné" in the face of the collapse of "what people through force of habit still called the government." Superbly he recounts how he summoned to his house the prefect of the Haute-Marne and ordered him to tell Prime Minister Pierre Pflimlin at once that "the public interest demanded that he see me." No explanation is offered for the May 27 statement he issued, saying he had begun the regular procedure to establish a government (Pflimlin had not given up; the president, René Coty, had charged him with no such mission). His account of the meeting, May 28, with the *présidents* of the two chambers overlooks his refusal to present himself to the National Assembly. He does not recall telling Guy Mollet, during the May 30 conversion at Colombey, that because of poor health he no longer sought power.<sup>9</sup> There is no mention of his having been kept informed of events in Algiers during those days, or of General Salân sending General Dulac to apprise de Gaulle of the Algiers plans, though it is alleged that de Gaulle criticized the dispositions and told Dulac to do what was necessary if parliament and the government continued blocking the way to his return.<sup>10</sup> This recollection of the opening round, when Coty and the government yielded, de Gaulle compromised, and the parachute troops failed to

<sup>7</sup> Jean Monnet, Dec. 15, 1959, quoted in C. L. Sulzberger, *The Last of the Giants* (New York, 1970), 625.

<sup>8</sup> When General Paul Ely asked him in June 1958, "At what moment, General, did you come to feel that your return to power was certain?", meaning at what moment between May 13 and the investiture by parliament, de Gaulle turned away abruptly, saying "Always!" Paul Ely, *Mémoires* (Paris, 1964-69), 2: 342. But he told Macmillan in March 1960 that "he never expected to return to power—at least not in recent years." Macmillan, *Memoirs*, 5: 183.

<sup>9</sup> De Gaulle, *Mémoires*, 1: 21-25, 28, 29-32; cf. André le Troquer, *La Parole est à André Le Troquer* (Paris, 1962), 181-99.

<sup>10</sup> Jacques Soustelle, *L'Espérance trahie* (Paris, 1962), 45-46; Soustelle, *Vingt-huit ans de gaullisme* (Paris, 1968), 145-47.

appear, is something less than a full statement. What is missing is less some terrible admission of complicity in a plot than a frank explanation of his own situation within the overall crisis of the state. The scenario is spare. Prizes and punishments are handed out: commendations to Pflimlin or Coty, reprimands to Pierre Mendès-France or Jacques Duclos. Even the shade of Edouard Herriot is not spared a school-masterish rebuke for a foolish attempt to read de Gaulle a lecture on the Resistance (Herriot's record in that epic was somewhat muted) twelve years earlier, and the General recalls "the rather blunt and ironical reply it deserved" and he apparently delivered, with a slight suggestion of relish.<sup>11</sup>

After the blameless return (in response to "the contract which the France of past, present and future imposed on me eighteen years before") came the resolute determination to end the war through a grant of independence to Algeria. Various he says that a wiser Fourth Republic doubtless could, or perhaps might, have enabled an autonomous Algeria to evolve peacefully into an association with France,<sup>12</sup> but he is almost silent on the record of the Provisional Government before that (and on the inhuman reprisals inflicted following the May 8, 1945, independence demonstrations and killing of some twenty-nine Europeans in Kabylia). His private conversations during the *traversée du désert* suggest that he had come to see independence as both desirable and inevitable. Speaking of Morocco, he once remarked to General Catroux: "If I were the government of France, I should not let independence be wrested from me, I should bestow it." His problem was how to create the requisite conditions for Algeria. He admits calculatedly "flinging" to the crowd in the Algiers forum the notorious ambiguity, "Je vous ai compris," with apparent spontaneity, one of the first of many tactical feints. But he reveals little. He claims to have measured General Salan's "slippery and enigmatic" personality (he owed his return in some measure to this man) from the first and to have determined to be rid of him soon, though this was not clear at the time.<sup>13</sup> He recalls a town clerk in some Kabylia village taking him aside to expose the fraudulent popular enthusiasm and warn that the people wanted independence, not France. He confesses that his prime minister, Michel Debré (with the new 1958 constitution in force de Gaulle had been elected president of the Republic), both acted with

<sup>11</sup> De Gaulle, *Mémoires*, 1: 33; cf. Tournoux, *Tragédie*, 196, 205.

<sup>12</sup> *Ibid.*, 34, 17-18, 49.

<sup>13</sup> Tournoux, *Tragédie*, 241-44; cf. de Gaulle, *Mémoires*, 1: 50, 57. Olivier Guichard, then de Gaulle's directeur du cabinet, said, however, that "he was not able to persuade de Gaulle on this mistrust [of Salan]. Among de Gaulle's faults is that he thinks he can regulate military affairs by handing out promotions and decorations. He is simply unaware of the fact that he, de Gaulle, never made the slightest imprint on the French army." Mar. 1, 1962, quoted in Sulzberger, *Last of the Giants*, 853.



complete loyalty and vented his hostility to self-determination.<sup>14</sup> He remembers commanders who pledged their fidelity to him and withdrew it in April 1961.

Of that revolt staged in Algiers by the four generals known for months to be plotting against him, he says he feared a paratroop descent on Paris; but he passes in silence over the famous panicky appeal to the population launched by Debré as a consequence of de Gaulle's silence and (before the ringing television and radio denunciation of the quadrumvirate which broke them) apparent paralysis that had unnerved the cabinet.<sup>15</sup> Faithful to his formula, he asserts both that he had "the clear-sighted support of the people" and that few voices supported him, whether he was trying to extricate France from Algeria or defend her garrison at Bizerte against President Habib Bourguiba's fell assault. Nearly all were arrayed against him, whether Dag Hammarskjöld, the OAS ("the scum of the military mob"<sup>16</sup>), unnamed foreigners,<sup>17</sup> or even his second prime minister, Georges Pompidou, who, with the minister of justice, Jean Foyer, intervened and maneuvered to prevent General Jouhaud's being executed—the drama of which encounter is missing here, though the remembered bitterness may be read between the lines.<sup>18</sup>

The loss of Algeria is presented as inevitable in the circumstances; its ultimate negotiation into independence was the result of an almost singlehanded turning of "a page of our history." Perhaps de Gaulle was never imperially minded: his military education had oriented him more obviously toward Europe than toward the colonial world. But it remains uncertain from these memoirs at what point he came to believe that the European empires were doomed. Though he invokes the Brazzaville Conference of January 1944, he gave no indication at that time. Somewhat curiously, he recalls Ferhat Abbas approaching him in those days to say that de Gaulle then, more than anyone else before or after, could bring about a democratic Algerian state federated with

<sup>14</sup> Debré, as Christian Fouchet put it, was "too visibly unhappy with the way things were working out," and Soustelle remarked on his "curious masochism." Fouchet, *Mémoires* (Paris, 1971), 1: 146; Soustelle, *L'Espérance trahie*, 97.

<sup>15</sup> De Gaulle, *Mémoires*, 1: 112; Alexandre, *The Duel*, 72-73.

<sup>16</sup> De Gaulle, *Mémoires*, 1: 128.

<sup>17</sup> With some justice: for instance, Ambassador James M. Gavin told Sulzberger on January 15, 1962, that "our only policy is to stick with de Gaulle until he achieves an Algerian settlement and then to drop him like a hot potato." See Sulzberger, *Last of the Giants*, 839. Georges Pompidou even claimed that there had been a "serious plot" to oust de Gaulle two years earlier. American money was said to have been linked with Alain de Sérigny and others. The inference was that the CIA was behind it. The scheme was said to have involved a government that would include Georges Bidault and the election of Antoine Pinay to the presidency. See the conversation with Sulzberger, Feb. 11, 1960, *Last of the Giants*, 641.

<sup>18</sup> De Gaulle, *Mémoires*, 1: 135; cf. Alexandre, *The Duel*, 86-92; Tournoux, *Tragédie*, 413-18. Significantly, de Gaulle does not name Fouchet as being in the opposition camp, though in fact he advised the president against carrying out the execution. No doubt his fidelity to the last earned him this silence. See Fouchet, *Mémoires*, 1: 172-79.

France. "But," he says, "given that in the middle of a war, in the frightful situation in which our country then found itself, there were more imperative and urgent matters than that one to be resolved, I had listened to Ferhat Abbas with as much reticence as interest."<sup>19</sup> The explanation is unconvincing; it is essential to the suggestion—it is no more than that—that had he been in power after January 1946 the process of imperial devolution in North Africa and Asia might have been different.<sup>20</sup> Things did not turn out that way; to him fell the ungrateful task of ending "the colonial epic" after others had produced only war, defeat, and civil strife.

Of the evolution of black Africa and Madagascar to independence he has relatively little to say. It caused small fuss. But the uncooperative Guineans are shown as being already enveloped in a totalitarian system by that first summer of 1958: even the women were dressed alike, singing and dancing on cue. "Young, vibrant and ambitious," Sékou Touré, for his insolence, was read a sharp lecture, and as de Gaulle departed he said to him, anticipating a vote to leave the empire, "Farewell, Guinea!" Those who remained with France, to achieve independence within interdependence over the following two years, are favorably presented, each leader, whether Houphouët-Boigny, Senghor, or M'Ba, receiving a brief but special word of commendation. And on the smooth transition with which this enormous change was effected a note of self-congratulation is sounded: "Perhaps the friendly relations which General de Gaulle enjoyed with their leaders had something to do with it."<sup>21</sup> This is said without a trace of awkwardness and with a good deal of truth. It is simply characteristic.

"Two issues fill his mind," Macmillan remarked that first summer, "Algeria and French Constitutional reform."<sup>22</sup> Algeria was the occasion of national breakdown; it had to be eliminated. The rearrangement of what would continue to be useful and profitable imperial relationships was secondary. The principal issues were at home, in Europe, and in the wider world. On the making of the constitution

<sup>19</sup> De Gaulle, *Mémoires*, 1: 15-16, 126-27.

<sup>20</sup> *Ibid.*, 127. But there is no evidence that he would have done differently than did the governments of the Fourth Republic in the wake of the activity of Admiral Thierry d'Argenlieu, an old Companion for whom de Gaulle continued to show deference and something like affection. See Tournoux, *Tragédie*, 48-49, 174-76. The General's subtle suggestion here is in line with, though more cautious than, the passage in his war memoirs where he declared his intention to have been to release Marshal Pétain from imprisonment on the Ile d'Yeu after two years. It might have been so, but the published avowal long after the marshal's death was unmatched by any activity on behalf of parole before the event. See Charles de Gaulle, *Mémoires de guerre* (Paris, 1954-59), 3: 250. Moreover, the president's attitude toward formal "rehabilitation" of the marshal after 1958 lent no great credence to his supposed intent in 1945. But such, perhaps, are the minor, if dubious, compensations that memorialists are tempted to extract from the bitterness of political exile.

<sup>21</sup> De Gaulle, *Mémoires*, 1: 59-60, 74.

<sup>22</sup> Macmillan, diary, June 29, 1958, in Macmillan, *Memoirs*, 4: 447.

de Gaulle has nothing new to say. The manner in which his government orchestrated the campaign for its adoption is not mentioned; nor is the way that the police were unleashed to discourage the more vociferous opponents (these pages tell nothing of the violences condoned by his regime, the assaults on the critical press, the savagery directed at demonstrators, and the atrocious and racist treatment of the wretched North Africans in France who were trapped by the dilemmas of the Algerian drama). He naturally does not recall the shabby treatment of President Coty, who was left to find his way home following the ceremony marking de Gaulle's assumption of the presidency,<sup>23</sup> though he provides space for Coty's encomium of his successor. In such matters reticence and understatement are not the weapons drawn from this literary arsenal. For his common sense and self-effacement, Coty would, it is true, continue to receive marks of distinction from the Elysée. For the politicians who had resisted, however, it was war then, and the campaign against them rumbles on through these volumes, a scarcely suppressed fury with those who wished no more from him than immediate salvation from the mess into which their incompetence had plunged the nation. Only "le peuple français"<sup>24</sup> had no such *arrière-pensées*. Thus from the early pages of the first volume the stage is set for the great confrontation with "the parties," as with every political assemblage, national or international, falling here under the stinging epithet of "areopagus." France had not "summoned" him to put her to sleep. His task was no less than to halt "the frightful decline which she had experienced for more than a hundred years."<sup>25</sup>

Whatever the 1958 constitution provided in the way of parliament, council of ministers, and prime minister, "it was toward de Gaulle that the French turned in every case. It was from him that they expected the solution to their problems." Hence his determination to institutionalize more directly the "contract" between himself and France (he said it was on behalf of his successor, who might not have his prestige against the parties, but his general conception of the post-Gaullist era was almost Dantesque). Whatever the legal texts might say, his authority seemed to him not open to question. These pages reveal an astonishing self-confidence, precisely justifying the most extreme interpretations of his self-assurance, without for a moment betraying the faintest suspicion of self-interest. On the discussion of his relations with the ministers it would be hard to think of lines better communicating

<sup>23</sup> Tournoux, *Tragédie*, 301-02.

<sup>24</sup> Used formally in this way, the term is an idealized abstraction seeming to have little connection with "the French," of whom the General's not so private opinion was often very low. "The French," he said to Malraux, "have no more national ambition. They don't want to do anything for France. I amused them with banners." Malraux, *Les chênes*, 23. By contrast, he said that "le peuple" represented what he meant by France. *Ibid.*, 115.

<sup>25</sup> De Gaulle, *Mémoires*, 1: 40.

the master-servant relationship in government, unsullied by anything calculating or base. "All in all, I kept my distance, but not in an 'ivory tower.'" <sup>26</sup> ("He fascinates the ministers," Antoine Pinay remarked after leaving the cabinet. "No one dares to speak." <sup>27</sup>) As for parliament, what he says is of a consummate cynicism. Having almost emasculated parliamentary institutions, he confesses that he had always "reversed" the oratorical talents of the tribune and that "the morose spirit of routine" into which the chambers had now fallen "filled me with melancholy," although he "consoled" himself with the reflection that the old "'games, poisons, and delights'" of the Republics had gone and taken with them all the trouble they had caused. Gaston Monnerville, *président* of the senate ("this clever Antillean"), the not so consoled and unyielding defender of the upper chamber, is written off as "dogmatic" on the subject of republican regimes. Against such dogmatics and all the jealous, self-seeking "féodalités" the presidency had to be protected by popular election. This he had known "for a long time," but he had not put it forward at Bayeux in 1946 or in 1958 because "I thought it preferable not to do everything at once." <sup>28</sup> In fact, such a proposal would have created an uproar in either year. The explanation is not flattering because false.

There follows his account of the struggle in the autumn of 1962, with every authority condemning his choice of constitutional amendment by referendum, to free the presidency from the old political notability. Here again the memorialist haughtily overrides every objection to his invocation of article 11 of the constitution (rather than article 89, which provides for amendment through parliamentary action), attacking this "armée du 'Non,'" the parties, the constitutional organs, the oldest of his political supporters, like Paul Reynaud, as then he dismissed their pleas, embassies, and public protests. Only one minister, Pierre Sudreau, failed the test and had to leave. <sup>29</sup> When the politicians carried the war to de Gaulle and censured his government he dissolved parliament. A crescendo of bitterness rises in this tale, which, even in the recollections from that remote country house seven years later, almost sears the page. If the General was so apparently calm and contemptuous of the assembly hubbub before dissolution as to attend military maneuvers, he does not entirely master his displeasure even when adversaries are dead. Hence the unjust epitaph for Reynaud, that he had "never ceased to place the

<sup>26</sup> *Ibid.*, 284, 287.

<sup>27</sup> Talk with Sulzberger, Dec. 22, 1961, *Last of the Giants*, 831; cf. the description of the Council of Ministers' meetings in Soustelle, *L'Espérance trahie*, 94.

<sup>28</sup> De Gaulle, *Mémoires*, 1: 290, 292; 2: 15-20.

<sup>29</sup> The minister of national education, Sudreau, was fearful of the precedent the president might set in so highhanded a constitutional revision. He expressed this to him in a personal interview after the event. De Gaulle said, "Oh come now, Sudreau, no one will ever have the cheek to do what I'm doing." Tournoux, *Tragédie*, 432.

life, the oratory and the primacy of the Palais Bourbon above everything else."<sup>30</sup> Between them, of course, lay the catastrophe of 1940, their reactions to it, and the bitter words Reynaud flung at him in 1962 from the assembly tribune. Nearly thirty years had passed since Colonel de Gaulle had waited on this man, helping him in the parliamentary campaign for military reform, seeking to serve the army, the country, and, no doubt, himself.<sup>31</sup> It was Reynaud who had launched the first brief phase of de Gaulle's political career. What had happened since had cancelled out their past and whatever debt the General once had owed.

Amusingly, in light of what he writes (it is scarcely news) of ministerial dependence on him, he cites cabinet support for his stand. Hence the ministers criticized the Conseil d'Etat for its condemnation and dissolution of the Military Court of Justice (established by decree the previous June in the aftermath of the regular High Military Tribunal finding Salan guilty, but with extenuating circumstances). Obedient servants they were, but the fight against press, parties, and unions, the "Cartel," as he calls them, was carried almost single-handedly by the infuriated president. The referendum to approve the alteration of articles 6 and 7 of the constitution was successful. He writes that "a very strong majority" approved the constitutional change, but, since he gives the figures, readers with simple arithmetical skill will readily see his exaggeration. In fact they were not very brilliant referendum results, and he neglects to record that he appeared for some days to consider the withdrawal he had promised in the event of such an outcome.<sup>32</sup> Nonetheless, the legislative elections were satisfactory. The new parliament had a strong Gaullist majority. His account of this great battle ends with another self-congratulation on having brought the people to "this marvelous transformation" of its public powers. Indeed, this was the pinnacle of his political authority. Ahead lay all the struggles for a second term in 1965, the ruinous events of May 1968, and the fatal referendum of April 1969.

Against the charge that only foreign policy and the high affairs of state much interested him, de Gaulle is quick to assert the contrary. He calculates having spent half his time on economic and social matters, giving a lengthy audit of his stewardship here. Assailing the multiple and contradictory "doctors" whose prescriptions for the national health varied so widely (Jacques Rueff was the favored physician, a poet of finance and economics), he elaborates his conception of participation. This was to be the great social nostrum of the time,

<sup>30</sup> De Gaulle, *Mémoires*, 2: 61.

<sup>31</sup> A sketchy account of their cooperation in the mid-1930s and a few deferential letters to the deputy from de Gaulle are in Paul Reynaud, *Mémoires* (Paris, 1960-63), vol. 2.

<sup>32</sup> De Gaulle, *Mémoires*, 2: 78-79, 89-90; Alexandre, *The Duel*, 95-104.

in domestic affairs, the most profound thought of the "elective monarchy." Here, too, the theme is his triumph over all the nay-sayers, the "féodalités" of business and labor, as he pressed forward to humanize the economic system of the modern age. "Against such swelling opposition I saw myself as the engineer, in an American film, who keeps on driving the train heedless of the alarm signal pulled by restless or ill-intentioned passengers." Undeniably, there was industrial growth, some financial stability, and agricultural change of a profound character, although he discovered daily, he says, "that economic affairs, like life itself, are a struggle in the course of which no victory is ever decisively won."<sup>33</sup> He does not dwell on the setback suffered at the hands of the Decazeville miners in 1962-63, whose defiance of the Elysée united clerics and Communists against him, nor on the strikes that constantly troubled the country. Among the last pages he wrote are those dealing with education, reform of the senate, and regional reorganization, cheerless issues that finally broke his hold on the nation.

In the discussion of foreign affairs the memoirs are alive with that peculiar combination of an old integral nationalist distortion of French and European history and a lofty awareness of the far regions of the globe. Here, as elsewhere, he presents himself as fated to slice through the Gordian knots. Here, too, it is the same general cast of characters: hostile governments, jealous of him, hoping if not working for his disappearance; friendly peoples, gaily manifesting their enthusiasm on his visits, and thus compelling the rulers to give heed. As in the first memoirs he is the soldier of French independence, struggling to free France from the English, the Americans, or the menacing confrontation of the superpowers. Every domestic questioning of his foreign policy is one more manifestation of "the established, promulgated doctrine" of national self-effacement. Only the people know what is good for them; or rather, only they, he, and Maurice Couve de Murville, who is pictured here as something like the sum of all virtue. He, it seems, had "le don" as few others in French diplomatic history had had.<sup>34</sup> (What that was is evident in Couve de Murville's own work, a book of record and of almost uncanny reticence.<sup>35</sup>) He was the apparently perfect executant, and for

<sup>33</sup> De Gaulle, *Mémoires*, 1: 156, 171.

<sup>34</sup> *Ibid.*, 180-81.

<sup>35</sup> Couve de Murville, *Une politique étrangère*. Harold Nicolson, who had employed him some thirty years before as a tutor to his two sons, found that the appointment "makes me laugh," possibly because one does not expect summer French tutors to become foreign ministers, or possibly because, as he wrote to his wife, "He is so dry and plain, like a biscuit." Harold Nicolson, *Diaries and Letters* (London, 1966-68), 3: 349, 368; cf. the portraits of Couve in Paul-Henri Spaak, *Combats inachevés* (Paris, 1969), 2: 365-66; and Soustelle, *L'Espérance trahie*, 97-98.

his discretion, fidelity, and relentless hard work he is singled out in these pages.<sup>36</sup>

In the liberation of Europe and the establishment of French primacy there, Germany was the key. De Gaulle recollects the manipulation of Konrad Adenauer without excessive modesty and imparts that the chancellor's distinguished record did not spare him, on their first meeting at Colombey, a blunt if not wholly accurate rehearsal of three wars launched against France since 1870. (Adenauer's lengthy account makes no mention of this opening gambit; nevertheless, each appears to have delivered an expansive discourse on the past that was doubtless more spontaneous and sparkling than the remembered version set down.)<sup>37</sup> The tone is one of considerable satisfaction ("I invited Adenauer whom I scarcely knew," he remarked to Malraux, "you get people who despise each other, because they don't know each other, to sit down to the same leg of lamb, and that transforms them into sheep"<sup>38</sup>). He naturally does not mention Adenauer's anger with him for the attempt, immediately following this September 1958 house party (it was exactly two days later), not so much to reconstruct Europe as to set up a three-power Atlantic directorate.<sup>39</sup> The memorialist's purpose, rather, is to lead up to the historic January 1963 treaty of friendship and cooperation, and, despite occasional difficulties with Bonn, the impression given is of great harmony. The troubled days lay ahead, after Adenauer had gone (when, for instance, de Gaulle would hold forth to Harold Wilson on the theme of a prospective new threat, since "Germans will always be Germans"<sup>40</sup>).

In some contrast is his discussion of the British, whose principal object in those early days was the destruction of the European Economic Community. As he remembers it, Prime Minister Macmillan immediately warned him that the Common Market would be "the Continental Blockade" in a new war, economic at first, but threatening to spread to "other fields."<sup>41</sup> Everything here builds toward the famous veto of January 1963, for by that time, of course, the issue was not dismantling

<sup>36</sup> Of the private critiques later reported ("this wrinkled unctuous fellow") there is naturally no suggestion here; see Frédéric Barreyre, *Les derniers mots du Général* (Paris, 1971), 70.

<sup>37</sup> Konrad Adenauer, *Erinnerungen* (Stuttgart, 1965-68), 3: 424-35.

<sup>38</sup> Malraux, *Les chênes*, 173; cf. Couve de Murville, *Une politique étrangère*, 37, 241. It is not absolutely certain that Adenauer was wholly delighted with the visit. Terence Prittie discovered German and British officials who believed that de Gaulle had quickened serious reservations in the chancellor at that time. Terence Prittie, *Konrad Adenauer 1876-1967* (London, 1972), 264-65.

<sup>39</sup> De Gaulle appears to have communicated a copy of his September 17, 1958, memorandum to Paul-Henri Spaak, secretary-general of NATO. Spaak then permitted the German and Italian ambassadors in Paris to see it. Macmillan, *Memoirs*, 4: 453-54; cf. Spaak, *Combats*, 2: 180-81, where he says that this copy later disappeared from his files. The episode is not, to say the least, too clear. Curiously, Adenauer himself does not refer to it in his recollections.

<sup>40</sup> Harold Wilson, *The Labour Government 1964-1970: A Personal Record* (London, 1971), 412.

<sup>41</sup> De Gaulle, *Mémoires*, 1: 199. It is of some interest that Macmillan's diary entry for this day in question, June 29, 1958, at least as published, makes no mention of any such dire warning.

the EEC but British entry into it. Patiently the General exposes the unreadiness of his neighbors to shed their dependency on America and to turn toward Europe. Nevertheless, he offers sympathetic accounts of the visits back and forth across the Channel, not least of the honors done him during the state visit of 1960, in particular the great ceremony in Westminster Hall. But he was above influence, public or private. In November 1961, while entertaining de Gaulle at his Sussex house, Macmillan noted:

Charming, affable, mellow as the General now is, his little pin head is still as small as ever. His views are inward, not outward looking. I fear he has decided to oppose us, yet, in a way, he wants us in Europe. . . . Sometimes, when I am with him, I feel I have overcome it. But he goes back to his distrust and dislike, like a dog to his vomit.<sup>42</sup>

De Gaulle's thoughts on these occasions seem likely never to be known for certain. But his analysis of English weakness is pitiless, and his conviction that the British were preparing to wreck the EEC from the inside was evidently unshakable. He foresaw the day when he would have either to veto the Brussels negotiations or to end French participation in the Common Market, since no one else in the Six would say "No!" to England.<sup>43</sup>

He throws no light on his September 1958 approach to Great Britain and the United States for a three-power Atlantic directorate; he confirms that he expected evasive replies and thus freedom to begin the withdrawal from all military cooperation with NATO.<sup>44</sup> Almost overnight he had ended the long French subordination to the Anglo-Saxon leadership, and although there are only the friendliest personal references to President Eisenhower here, the note of satisfaction with his own achievement is as clear as it is justified. With the Russians also, he maintains, his policy was both firm and independent, and he established with Chairman Khrushchev "a real man to man contact."<sup>45</sup> Certainly the Russian leaders' state visit to France in March 1960 provides one of the few mildly amusing moments in these largely high-minded reminiscences, all too regrettably stripped of their author's lethal wit. The particular moment came at Rambouillet, with Kosygin rowing de Gaulle and Khrushchev around a pond to the accompaniment of the General's slightly sarcastic badinage and the chairman's coarse

<sup>42</sup> Macmillan, diary, Nov. 29, 1961, in Macmillan, *Memoirs*, 5: 426, 428.

<sup>43</sup> De Gaulle, *Mémoires*, 1: 200.

<sup>44</sup> *Ibid.*, 1: 214-15, where he misdates the memorandum September 14; Couve de Murville, *Une politique étrangère*, 33-34, is even more laconic and dismissive of the approach. "The 1958 memorandum was only a diplomatic pressure tactic," de Gaulle is said to have remarked later on. "I was then looking for a way to get out of the Atlantic alliance and to regain the freedom of action that had been surrendered by the Fourth Republic. . . . Hence I asked for the moon. I was sure they would not give it to me." Quoted in Tournoux, *Tragédie*, 321.

<sup>45</sup> De Gaulle, *Mémoires*, 1: 238.



humor.<sup>46</sup> But beyond these familiar tales of three men in a boat and the bold defiance of the American hegemony in Europe he has little to tell.

At the abortive summit conference of May 1960 he is naturally the central figure, if not the hero, evidently suffering no nonsense from Khrushchev while counseling the shaken Eisenhower (who later recalled de Gaulle touching his elbow and saying, "Whatever happens *we are with you*"<sup>47</sup>). From the outset he had taken a strong line over Berlin, and here again he accuses both of his Western partners (at their earlier December 1959 Paris conference) of showing weakness, especially Macmillan, who is said to have been upset by the prospect of nuclear war for the sake of a German city. Thus he had given them a lecture on standing up to the Russians. How much more impressive it may be in these pages than it was in reality one can only guess, but there is certainly no English equivalent for the magnificent hauteur with which he recalls a sharp encounter with the rough-and-ready chairman: "M'enveloppant de glace, je fais comprendre à Kroutchev que la menace qu'il agite ne m'impressionne pas beaucoup."<sup>48</sup> It is one of the many miseries of the English language that this kind of thing cannot be said, even if one wished to say it. Hence Anglo-Saxon skepticism that it is worth saying in any language, and hence Macmillan's conclusion that de Gaulle's intransigence was a mere device to impress Adenauer ("who usually sucks up to de Gaulle"<sup>49</sup>), while counting on the British and the Americans to get him off the hook: "If de Gaulle thought there was any real danger of war, he would be in a panic."<sup>50</sup> It may well have been so, for, asked some years later by Harold Wilson whether he cheated at patience, the General replied that there were situations when this was justified.<sup>51</sup>

Confirmation of such suspicions will not be found here. The memoirs are full of set pieces and formal portraits, with few unaware touches. If there is an almost excessive elevation of tone (broken only by the occasional mordant thrust), they are not wooden, like the Eisenhower memoirs; they are not whimsical and chatty, like the lengthening Macmillan diaries and commentaries; they are undeniably his own,

<sup>46</sup> The sole comment on de Gaulle in *Khrushchev Remembers* (Boston, 1970), 507, is "and he's a sober-minded man."

<sup>47</sup> Dwight D. Eisenhower, *The White House Years* (New York, 1964-65), 2: 556.

<sup>48</sup> De Gaulle, *Mémoires*, 1: 241.

<sup>49</sup> Macmillan, diary, Oct. 22, 1959, in Macmillan, *Memoirs*, 5: 93.

<sup>50</sup> "He really admitted this to me. He said it was not perhaps anything but a rather cynical policy. Yet it was justified for we must at all costs prevent another German 'myth', such as had made Hitler's rise to power possible." Then de Gaulle went on to say that he would take this line even if Adenauer should accept the British view. Macmillan asked, "You would be plus royaliste que le roi?" De Gaulle replied, "Certainly. Then the Germany of the future will know that France was true." Macmillan, diary, Nov. 29, 1961, in Macmillan, *Memoirs*, 5: 426.

<sup>51</sup> Wilson, *Labour Government*, 406.

as the Khrushchev recollections may or may not be. But they present the General in a consciously imperial pose, gravely, if sometimes mechanically, commenting on the state of India or the city of Toronto ("industry was very active, construction was thriving, the university flourished"), discussing with Richard Nixon the difficulties of getting the press to take one at one's word, contemplating the vast hopes placed in President Kennedy until, "having taken each other's measure, we continued on our way, each of us bearing his burden and moving on toward his destiny!"<sup>52</sup> Clearly de Gaulle in retirement wanted to communicate only the historic personage, "the myth," as he used to say, that posterity should know, even to his work habits and his relationships with his family, but tidily and in a rigidly controlled manner. ("History," he said to Malraux, "may justify life, it does not resemble it.") And one can only admire the methodical manner in which this elderly man measured out his pages, assigned his paragraphs, assembled his brief character sketches, and rounded out his chosen subjects. As memoirs these chapters are both disciplined and impassioned with a cold and distant fury. On the one hand there is a compulsive underlining of uncertain triumphs, some of which were no more than the ephemeral brilliance of a particular *conjoncture*; and on the other, a bleak chronicling of a life without illusions: "I'm the character in Hemingway's *Old Man and the Sea*: I've brought back only a skeleton."<sup>53</sup> As is so often remarked, the key to this historic personage was written long ago, in the collection of essays titled *The Edge of the Sword*, with their definition of "the man of character" and their reflections on the nature of "prestige."<sup>54</sup> These last pages, though suddenly stopped short, continue the depiction of the exceptional leader adumbrated forty years before.

What is missing, of course, is Charles de Gaulle. "Now that he is old [sixty-nine] and mellowed," Macmillan observed in March 1960, "his charm is great. He speaks beautiful, rather old-fashioned French. He seems quite impersonal and disinterested." The memoirs consciously hide the man. "What did Caesar *believe*?" he said to Malraux. "Nothing he wrote tells us."<sup>55</sup>

<sup>52</sup> De Gaulle, *Mémoires*, 1: 255, 271.

<sup>53</sup> Malraux, *Les chênes*, 71, 79.

<sup>54</sup> *Le Fil de l'épée* (Paris, 1932), in a good translation, *The Edge of the Sword* (New York, 1960).

<sup>55</sup> Macmillan, diary, Mar. 13, 1960, in Macmillan, *Memoirs*, 5: 183; Malraux, *Les chênes*, 73. Doubtless many of those who knew him well will reveal him little by little, but the portrait will require a thousand tiny details if it is convincingly to replace the formal mask that the General left behind. For example, the solemnity of the Washington funeral in 1963 was quickly followed by a bantering exchange with John Kenneth Galbraith, who remarked that tall men, being more visible, were more to be trusted. De Gaulle shot back, "It is important that we be merciless with those who are too small." J. K. Galbraith, *Ambassador's Journal: A Personal Account of the Kennedy Years* (Boston, 1969), 598.

He said that writing was torture, that it was slow, that it was useless, but each time he returned to it.<sup>56</sup> Whether this was because he did, after all, think there might be some relationship between the idealized "peuple" à la Michelet with whom he lived out his "contract" with France, and the "cattle" and worse, as he branded the French in his fits of anger; or because he felt compelled to write his epic as the necessary extension of the action twice broken off and denied him; or because he was a man of letters with an esthetic need to complete his work of art (as once before, with the first memoirs, he had so evidently done<sup>57</sup>), it would be difficult to say. And his question to Malraux remains without answer.

<sup>56</sup> "He smiled quite sadly and said: 'Retire? Yes. Write another volume? No. Writing about the war; that was one thing. Now, that is another. Retire? Yes. But not another volume. Colombey? Yes. But not writing.'" Interview, Jan. 20, 1962; in Sulzberger, *Last of the Giants*, 843; on writing, see Tournoux, *Tragédie*, 233, and Malraux, *Les chênes*, 34; on the marketing of, and the writing of dedications in the memoirs, see Chapus, *Mourir à Colombey*, 223-31.

<sup>57</sup> De Gaulle, *Mémoires de guerre*, 3: 287-90.

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## Reviews of Books

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### GENERAL

MAURICE LOMBARD. *Monnaie et histoire d'Alexandre à Mahomet*. (École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés 26. Études d'économie médiévale 1.) Paris: Mouton. 1971. Pp. 233. 36 fr.

This is a strange book. Undocumented theses and annoying inaccuracies make it a worrisome volume. Yet the late Maurice Lombard possessed a brilliant mind, and so this work, rich in insights, expectedly furnishes pleasurable and instructive reading for the economic historian. On balance, then, the piety of Lombard's disciples in seeing his research into print deserves praise; I urge them to keep their promise to publish one or two more of his studies.

The premise of this volume is that there was an ancient and a medieval precedent for such momentous economic events as the flood of American bullion into sixteenth-century Europe. Lombard contends that the conquests of Philip and Alexander created a Eurasian monetary ecumene based on gold, stretching from West Africa to India. Economic resources hitherto locked within separate and hostile orbits, he would argue, were then released into the brilliance and openness of Hellenistic civilization. Lombard unfortunately provides few citations to support his views; moreover, the reader is left on his own to ponder multiple chicken-egg problems that arise in the dust of the author's fast tour of ancient economy.

Lombard then leaps to his medieval example. The Muslim conquests, he declares, resulted in a similar monetary world system based on gold, one that pumped life into a Western and an Eastern Europe creaking along on insufficient gold supplies. The argumentation for this pri-

macy of gold in the Islamic Empire is downright sloppy and some ten years out of date. Lombard all but ignores, for instance, the huge finds of Muslim silver coins in the Baltic region.

The remainder of the book contains valuable discussion of some other matters relating to Muslim economic activity from the eighth to the eleventh centuries. For example, Lombard's examination of Europe's exports to Islamic areas—slaves, furs, wood, and metals, including swords—should, if correct, reopen old debates concerning the influence of commerce in human bondage and war materials in the development of Western Europe's economy. Equally valuable is his treatment of the growth of Muslim towns—one of the great urban moments in history, Lombard states, in comparing it to the Hellenistic period. In addition, the author presents an informative description of the social repercussions deriving from the ascendancy of merchants in these Islamic cities. Landed families thereby declined in importance, he says, and a rural and urban *lumpenproletariat* suffered considerable misery. Noteworthy also are Lombard's sensible remarks concerning the early activity of Muslim guilds, a subject most Islamicists are foolishly avoiding.

The bibliography of sources in the first third of the volume will be useful to economic historians. This catalog lists Eastern and Western texts in which Lombard found materials relevant to the Muslim economy from the eighth to the eleventh centuries. Lombard's book has glaring faults, but his vision peered beyond stereotypes. The insights and the bibliography of sources make the volume worthy of its price.

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MIKLÓS JANKOVICH. *They Rode into Europe: The Fruitful Exchange in the Arts of Horsemanship between East and West*. Translated by ANTHONY DENT. New York: Charles Scribner's Sons. [1973]. Pp. 175. \$12.95.

*They Rode into Europe* will be of particular interest to readers concerned with such varied fields as the history of transportation, the dissemination of technology, sociological developments, and the evolution of military practices and institutions. This work is valuable because the author has written from the East European point of view, from the geographical region where Asian and European struggled for supremacy over more than a millennium. More important is the fact that Jankovich has been able to utilize sources that are usually unavailable to the Western scholar, unless he is able to use the Magyar and Slavic languages.

The scope of the work is vast. It begins with an account of the domestication of the wild horse and of attempts, often successful, to break onagers, camels, and even reindeer to harness and the saddle. The history of what the author not inappropriately calls "the age of the horse" is continued until it was supplanted by the steam locomotive and the internal combustion engine. This is done in 154 pages of text, plus an additional 12 pages of notes, and even the most casual reader will lament that the author has not gone into more detail on this or that aspect of horsemanship. These deficiencies are offset, to some extent, by illustrations on a lavish scale, in color and in black and white, and by numerous line drawings in the text.

The serious scholar, however, will find that *They Rode into Europe* is disappointing in a number of respects. The documentation is inadequate and many a conclusion is reached with no indication of the grounds on which it is based. The bibliography is notably lacking in Western sources; for example, Widukind and Luitprand of Cremona, authorities on the irruption of the Magyars into Germany and Italy, are omitted. Even more serious is the failure of Jankovich to consider the influence of Moorish horsemanship in Spain upon the cavalry tactics in early modern Europe. Lynn White's significant work on cross-cultural influences in a civilization based on horsepower is ignored, as well as the more recent studies of B. S. Bachrach. Finally, it should be noted

that this is a translation of a German version of a Hungarian original. The publishers nowhere state the qualifications of the author to write such a book, the faithfulness of the German translation to the Magyar text, or the grounds on which the translator was selected. By Western standards, this is not a scholarly publication.

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HANS HUTH. *Lacquer of the West: The History of a Craft and an Industry, 1550-1950*. [Chicago:] University of Chicago Press. 1971. Pp. x, 158, 364 plates. \$25.00.

In this much expanded version of his popular *Europäische Lackarbeiten* Hans Huth has provided a substantial picture of the European lacquer craftsman's work from the mid-sixteenth to the mid-twentieth centuries. Handsomely illustrated, both in black and white and in full color, the new book incorporates Huth's subsequent research findings, until now only available in scattered periodical sources. The study is concerned with Western objects "made of wood, papier-mâché, tin, leather and earthenware . . . decorated to produce the highly polished effect of lacquer."

The first wave of lacquer work reached Europe by way of the Middle East between 1550 and 1630. The technique was mastered by Venetian craftsmen and found its use in the decoration of boxes, chests, cabinets, musical instruments, and mirror and picture frames. The products of this period reflect Islamic sources, particularly Turkish bookbindings and weapon ornamentation, and show no indebtedness to the Far East.

Far Eastern lacquered objects began to appear in Europe toward the end of the sixteenth century but not in sufficient quantity to satisfy the demand. The idea of creating a reasonable Western substitute seems natural although there is scanty documentation, Huth believes, to suggest European production between 1630 and 1670. By 1680, however, "European-produced lacquer rivalled that imported from the Far East."

The basis for this new craft (and ultimately for a substantial industry) was laid in 1720

when Father Filippo Bonanni published the first "accurate and authoritative account" of Chinese lacquer, identifying its chemical base in Oriental resin. The impossibility of obtaining this in the West led Bonanni to suggest the use of substitute techniques based on varnish or European shellac. These adaptations were variously referred to as Japan work, *vernis de la chiné*, or *Indianish werk*. Oriental products and European imitations both were popularly called lacquers.

The second phase of lacquer work in Europe coincided with the eighteenth century and its interest in *chinoiseries*, representations of the Orient and things Oriental by Western artists and decorators. At first these representations tended to dignify and exalt the Orient. Later a withdrawal of Church sanctions led to an emphasis on the droll, the grotesque, and the comic aspects of the exotic. European flowers and *rocaille* displaced the vogue for *chinoiseries* at mid-century. With the late-century romantic revivals a new treatment of China as a fairyland of pleasant landscapes peopled with sentimental figures came into style, following the manner of Boucher and Pillement. In the late eighteenth century lacquered objects lost most of their foreign characteristics and began to follow closely upon European fashion shifts.

*Chinoiseries* were produced in every major country either by professional craftsmen and decorators or by aspiring dilettanti. Among those of the eighteenth century who we know by name are Gérard Dagley of Berlin, Martin Schnell of Dresden, Giles Grendey of London, members of the Martin family of Paris, and Thomas Johnson of Boston.

When it became economically unfeasible for self-employed lacquerers to continue their trade after the French Revolution two new factors aided the craft in becoming a popular industry in the nineteenth century: the production of tinware and papier-mâché. Huth traces the rise of the japanned tinware industry in Pontypool, Usk, Birmingham, and Bilston and the papier-mâché industry in Wolverhampton, Birmingham, and Scotland. Of unusual interest is Huth's research on Stobwasser of Brunswick and on the Russian enterprises of Demidoff, Korobov, Lutkins, and others. To some extent this material parallels Walter Holzhausen's 1959 study, *Lackkunst in Europa*. Both authors

give a nod to lacquerers practicing in the twentieth century.

Plates 148, 150, and 151 illustrate a "Clavichord, by H. A. Hass, Hamburg, 1732." The instrument is a harpsichord, however, and should be so labeled in the second edition of this work—otherwise so free of errors. One also hopes that Huth will expand, in the future, his evaluations of the 1630-80 evidence as well as his discussion and illustration of late eighteenth- and early nineteenth-century lacquered furniture, the latter a topic more fully managed by Hugh Honour in his 1961 volume, *Chinoiserie, The Vision of Cathay*.

Hans Huth's *Lacquer of the West* is the stunning contribution of a mature connoisseur and art historian who has never for a moment ceased to be in love with either the subjects or the objects he describes. The book is therefore an enviable model of humanistic scholarship.

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ANTHONY D. SMITH. *Theories of Nationalism*. (Torchbook Library Edition.) New York: Harper and Row. 1971. Pp. viii, 344. Cloth \$12.00, paper \$3.95.

BOYD C. SHAFER. *Faces of Nationalism: New Realities and Old Myths*. New York: Harcourt Brace Jovanovich. 1972. Pp. xv, 535. \$12.95.

These two books offer a comparison between the approaches of two different disciplines, sociology and history, to the study of a phenomenon of interest to both: nationalism. Both books reveal some of the strengths and weaknesses common in the practice of each discipline. The study by sociologist Anthony D. Smith examines the various theoretical models that have been advanced for the analysis of modern nationalism, indicates the problems of each, and offers a model of its own; Smith's hypothesis is analytical, highly abstract, and largely devoid of the kind of evidence historians demand in support of generalizations. The work of historian Boyd C. Shafer, on the other hand, traces the evolution of national sentiment from the eighteenth century to the present and offers some conclusions about nationalism in the present and future; the book is descriptive, filled with historical examples and quotations illustrating the different "faces" of

nationalist doctrine, but it lacks any genuine conceptual framework that would attempt to analyze the social forces that generate nationalism. Smith begins with the questionable assumption that nationalism must be studied as an ideological movement, that "the growth of nations and the rise of nationalism can be separated" (p. 6). Shafer begins with the equally questionable opposite assumption that "the nation makes patriots," that all the activities of national governments, from the centralizing bureaucracy of Louis XIV to the New Deal of Franklin Roosevelt, are responsible for the growth of nationalism. Attempting to avoid all value judgments, Smith adopts an attitude of "ethical ambivalence"; Shafer, while acknowledging his bias against extreme nationalism, readily offers his own values when he concludes that the nation has protected man against "the Hobbesian anarchy that ever lurks behind the veneer of human politics," that it has given man "the feeling that he is not alone, that he belonged to a group that cared" (p. 350). The historian interested in more discourse between history and sociology will be discouraged to discover how little these scholars seem to have learned from each other's disciplines.

Smith objects to the "idealist" analysis of Kedourie, which sees modern secular thought as the primary solvent of traditional social institutions and thus Kant as the fountainhead of modern nationalism. He finds that most of the views of nationalism as a response to the crisis of modernization (as espoused in various ways by Eisenstadt, Smelser, Lerner, and others) are tied too closely to the Western experience and thus are ethnocentric and deterministic. He disagrees too with the more complex treatment of the relationship between nationalism and modernization offered by Gellner because he believes that it exaggerates the link between nationalism and the aspirations of an uprooted proletariat, that it overstresses both the unanimity of the intelligentsia behind nationalism and the linguistic criterion for nationhood. Nevertheless, Smith's own analysis draws much from these other theories. He begins by drawing a distinction between what he calls "ethnocentric" and "polycentric" nationalism. The former is a movement against foreign rule; it regards outsiders as barbarians and seeks only to preserve its own culture; the latter sees the

world divided into separate nations, each with its own value and autonomy. The question he addresses is: how does the transition from "ethnocentric" to "polycentric" nationalism come about? The answer, he suggests, is to be found in the conflicting responses of the intelligentsia to the tensions produced by modernization. Modernization produces an essential conflict between the religious and cultural values of the traditional order and the rational and efficient claims of the "scientific state." Both systems exert legitimate claims for authority and the intelligentsia responds to this "dual legitimation" in three ways: the "traditionalist" rejects modernization entirely, the "assimilationist" embraces it completely and sets his hope on an enlightened cosmopolitanism, and the "reformist" tries to combine both traditional and modern society into one synthesis. Neither the reformist nor the assimilationist can realize his objectives completely, however, and the result is that the former tries to save the unique values of his culture, which leads him toward an ethnic definition of community, while the latter abandons his devotion to common humanity and substitutes a belief in the community of nations—"polycentric nationalism."

Shafer's argument is much less complicated. He summarizes it best himself: "This book has argued that the nation and nationalism developed because the governments (early the dynastic, later the democratic or the authoritarian) increasingly penetrated the lives of people, because people increasingly participated in the national affairs and identified themselves and their political and cultural interests with those of their respective nations, that out of the penetration, participation, and identification there came to be a kind of consensus within each nation, that once this was established nationalism fed upon itself and grew, and that the national states, once established, encouraged and imposed nationalism, educating and persuading the already loyal to be more patriotic by many means and coercing the reluctant by law and by force" (p. 345).

Shafer's volume contains a great deal of information, useful notes, and an extensive bibliography; his overall approach, however, offers little that is new, and it uncovers no promising new trails for future exploration.

Smith's approach is admittedly too schematic and too tidy; it lays too much stress on the role of the intelligentsia. Despite its obvious faults, however, the book does suggest some new paths historians should follow if we hope to learn more about the complex topography of nationalism.

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T. BENTLEY DUNCAN. *Atlantic Islands: Madeira, the Azores, and the Cape Verdes in Seventeenth-Century Commerce and Navigation*. (Studies in the History of Discoveries: The monograph series of the Society for the History of Discoveries.) Chicago: University of Chicago Press. 1972. Pp. xv, 291. \$10.00.

Islands possess a fascination *sui generis*. Centers of communication, they have a strategic, commercial, and navigational importance out of proportion to their size. The Azores, Madeiras, and Cape Verdes comprise twenty-nine islands. Each has its own personality, ethnic composition, climate, and topography and different degrees of habitation, cultivation, and prosperity. From the sixteenth century there was considerable interinsular trade in complementary raw materials. But the historical importance of these archipelagoes lay in their geographical position as pivots of international trade. The axes of colonial Atlantic commerce—London-Boston, Seville-Caribbean-Central America, Lisbon-West Africa-Brazil, West Africa-Caribbean-North America—took advantage of the islands as sources of manpower and supplies, havens for storm-tossed ships, refuges for smugglers, and commercial entrepôts for the supply or transshipment of raw materials and manufactured goods. Funchal and Horta in the seventeenth century boasted an international mercantile elite and a cosmopolitan outlook. The Azores were vital to the slave trade between West Africa and the Americas. Dr. Duncan demonstrates how outmoded the geometrical clichés of triangles and polygons are in describing the cat's cradle of Atlantic routes bearing greater or lesser densities of traffic.

The prosperity of the islands did not lie in the commercial acumen or initiative of their peoples. The destiny of the Atlantic islands was decided in Europe, the Americas, and, to a lesser degree, Africa. Reduced to a passive role, the

islands were the victims of commercial and political vicissitudes. Whereas English sallies into the Guianas and Lesser Antilles and the seizure of Jamaica opened up new markets for Madeira and the Azores, the import of cheap indigo from Spanish America dealt a mortal blow to Azorean pastel production, and the rise of a whisky industry in the United States deprived Madeira of a major market. The ebb and flow of economic fortune was no better illustrated than by Madeiran sugar production in the sixteenth and seventeenth centuries, until Brazil came to dominate the trade following the expulsion of the Dutch in 1654. Dr. Duncan has ably delineated the flux and reflux of prosperity and decline in the basic commodities of each island.

The author has performed a valuable service in emphasizing the vital role of the islands in the building of the Portuguese, English, and Spanish seaborne empires. Archives in Portugal, the Azores, and Madeira have been consulted; it is regrettable that Dr. Duncan was unable to consult Brazilian, Spanish, English, and North American manuscript sources. Portuguese colonial accounting methods are the despair of any modern scholar, but the presentation of the available figures on imports and exports from the islands is markedly unsophisticated and lacks analytical maturity. The book would have benefited from more careful editing: the author's descriptions based on personal experience often disrupt the historical narrative; statistics of shipping for the years 1875-98 and of coal and oil sales at Mindelo from 1927 to 1932 and from 1952 to 1957 have no relevance to the central theme. The cartography is of a high standard, and the descriptive map of trade between the Cape Verdes and Guinea is a model of its kind. *Atlantic Islands* whets the reader's appetite but will not stir his imagination. Asking more questions than it answers, this monograph will, it is hoped, stimulate further research into the role of these islands in the formation of the Atlantic community.

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L. A. SHUR. *K beregam Novogo Sveta: Iz neopublikovannykh zapiskov russkikh puteshestvennikov nachala XIX veka* [To the Shores of the New World: From Unpublished Reports of



Russian Travelers at the Beginning of the 19th Century]. (Akademiia Nauk SSSR, Institut Etnografii imeni N. N. Miklukho-Maklaia.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 285.

More than two centuries ago the imperial Russian double-headed eagle swooped down over North America and grasped Alaska in her talons. Alaska, the land of fur gold and Stone Age Aleuts, Eskimos, and Indians, fell prey to Russian fur hunters, merchants, and Cossack banditti who exploited the fur wealth and enslaved the nearly defenseless natives. In the early 1800s the Russian-American Company, a Russian fur trade monopoly in Alaska, occupied Spanish-claimed northern California at Fort Ross and Bodega Bay.

In *K beregam Novogo Sveta* Leonid A. Shur, a noted Soviet historian of Latin America, published for the first time three Russian travel diaries describing the history and culture of early nineteenth-century Brazil, Peru, Mexico, California, and Alaska. Fedor F. Matiushkin and Fedor P. Litke, young Russian naval officers, kept the first two diaries while traveling around the world from 1817 to 1819 on the sloop *Kamchatka*. Their comments on the Russian settlements at Fort Ross and Bodega Bay are particularly valuable, since before 1820 few firsthand descriptions of these outposts exist.

Baron Ferdinand P. Vranghel, famous arctic explorer and general manager of the Russian American colonies, wrote the third diary while journeying from Sitka to St. Petersburg through Mexico in 1835 and 1836. His diary reveals his unsuccessful attempts to convince Mexico to establish trade relations with Russian America. Unfortunately, Shur failed to note or make use of Vranghel's diary of his Mexican trip which is found in the United States National Archives' *Records of the Russian-American Company, 1802-1867: Correspondence of the Governors General, Communications Received* (volume 10, folios 189-98). This omission is inexcusable, especially since microfilm copies of these documents are in the Soviet Union.

Well illustrated with rare nineteenth-century drawings, Shur's publication is a priceless contribution to a thorough understanding of Russian activities in the New World.

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DAVID W. COHEN and JACK P. GREENE, editors. *Neither Slave nor Free: The Freedman of African Descent in the Slave Societies of the New World*. (The Johns Hopkins Symposia in Comparative History, number 3.) Baltimore: Johns Hopkins University Press. 1972. Pp. xi, 344. \$13.50.

This book is a distinctive contribution to the enticing but treacherous domain of comparative history. It succeeds because it is written by qualified scholars who address a delimited, manageable subject—the role of the free colored population in New World slave societies. Six of the papers were given at a conference in 1970 and then revised. Three more were written, and one reprinted from elsewhere, to fill gaps noted in the discussions. This procedure helped to eliminate the randomness and incommensurability that plague many such collections. The task was to canvass current knowledge and pinpoint areas of needed research regarding two topics: first, the experience of the free colored as a measure of the character of slavery and race relations; second, the functional roles of this group in the evolution of the respective societies.

Eschewing the usual piety and rhetoric of anthologists, the editors have synthesized their contributors' comparative data to highlight contrasts and general trends. Tables display the percentage of freedmen in each society with respect to total, free, and colored populations for the period from 1764 to 1840, demonstrating the low proportion for the United States South and the British West Indies as against Ibero-America. In every society, manumission of white males' black and mulatto wives or concubines and their offspring yielded large increments to the free colored group, as did self-purchase, especially by males, in times of economic growth. Furthermore, each society experienced periods of restriction on manumission, when natural replacement became the main source of free-colored growth. The editors tentatively conclude that freedmen tended to acquiesce in the slave system and to avoid forming a common front with slaves, although frequently they raised political issues damaging to the slavocracy.

Availability of basic research importantly determines space allocation and strategies of presentation. In a mere twenty pages Eugene

Genovese sketches a trajectory with local variations for the United States South, while Jerome Handler and Arnold Sio take twice as long to construct a story, largely from sources, for tiny Barbados. (Total population and territorial size, however, can be misleading indicators; readers may be surprised to learn from Franklin Knight that from the late eighteenth century to 1860 the free colored population of Cuba was numerically about equal to that of the United States South.) In a smoothly written essay Frederick Bowser generalizes as best he can for three centuries of regionally diverse Spanish American colonial history, frequently drawing on his own Peruvian research for illustration. John Russell-Wood tackles the similarly complex assignment of colonial Brazil by focusing on three major nuclei of colored population: the Bahian sugar zone, the mining zone, and the city of Salvador.

Some of the book's most arresting hypotheses arise from single papers rather than from the ensemble. A notable instance is Hermannus Hoetink's essay on the Netherlands Caribbean, which shows that a free-colored elite developed in Surinam, with its severe slave system, but not in Curaçao, with its mild, paternalistic system. Other articles treat the French Antilles (Léo Elisabeth), Saint Domingue (Gwendolyn Hall), Jamaica (Douglas Hall), and nineteenth-century Brazil (Herbert Klein). One hopes for a sequel by equally knowledgeable scholars that would consider the adjustment of freedmen in post-emancipation societies.

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PETER CALVOCORESSI and GUY WINT. *Total War: The Story of World War II*. New York: Pantheon Books. 1972. Pp. xiii, 959. \$15.00.

Twenty-eight years after the most destructive war in human history the memories of those who fought in it have begun to fade; new generations have grown up for whom the war is but a blurred panorama of distant battles and unfamiliar names. To bring the causes and events of the war back into focus the authors decided "to write a book showing why it happened and how it went" (p. xi). In spite of the considerable difficulties involved in such an undertaking Messrs. Calvocoressi and Wint have succeeded in presenting a lucid and well-written

account of the diplomatic, political, social, economic, and military aspects of the war. They have also managed to include enough background material to make even the most complicated developments comprehensible to the average reader.

To cope with the enormous amount of material the authors have divided their story into two parts. Mr. Calvocoressi writes on the war in Europe, on the Atlantic, in Africa, and in the Middle East, while Mr. Wint, and after his death, his wife, covers the war in Asia and the Pacific. It is a sensible arrangement and the minor overlapping that occurs does not distract from the even flow of the narrative. The major problems of writing a one-volume history of a period as complicated and confusing as the Second World War are selection and organization of the material; the authors should be commended on both counts.

The origins of the war are a confused and tangled web, and Calvocoressi stresses correctly that there were several wars that only merged into one global conflict after Pearl Harbor. Hitler's policies played a crucial role in unleashing the war in Europe, and in explaining Hitler's policies Calvocoressi presents a good summary of modern German history and the rise of nazism. Inevitably there are some misconceptions: Bismarck did not use racial prejudice for political purposes, and Hitler's plans for Germany's expansion included considerably larger territories than those lost after World War I. For the rest, Calvocoressi's account of developments between the wars is balanced, concise, and eminently sound. In his view, "Hitler's political gamble came off not because France and Great Britain were militarily incapable but because they were strategically inept" (pp. 94-95).

The initial stages of the war—the Polish and Norwegian campaigns and the Russo-Finnish war—are well summarized, but Hitler's contribution to Manstein's plan for the invasion of France is not adequately discussed; on the other hand, the Führer's ambivalent attitude toward Britain and the German army's unexpected halt before Dunkirk are clearly explained. The Battle of Britain, the campaigns in the Balkans and North Africa, and Hitler's preparation of "Barbarossa," the invasion of Russia, are well described. Calvocoressi does not

believe that Germany's Balkan campaign had any appreciable effect on "Barbarossa" (only if the British defense of Crete had lasted longer would it have made any difference), but he fails to explain Stalin's persistent refusal to heed numerous warnings of the invasion.

Germany's New Order, territorial adjustments, population movements, and the extermination of Jews and other minorities are well covered, as is the relationship between Britain and the various European governments in exile. Of the neutrals, only Switzerland's role in the war is mentioned; Sweden, Spain, and Portugal are omitted. The European resistance movements, including the German opposition to Hitler and the revolutionary movements in southeastern Europe, are carefully explained. The United States-British-Russian alliance and the issue of the second front are put into proper perspective. One of the best chapters deals with British domestic problems during the war. That the British kept on fighting in spite of blatant inequalities at home and seemingly insurmountable odds abroad has puzzled many observers. "The British did not rebel against inequality and injustice," Calvo-coressi writes, "because on the whole they did not believe in equality or expect justice. . . . Freedom and incorruption were the secular religion of Great Britain . . . and *in hoc signo* the British engaged the enemy as a united people" (p. 406).

In judging the Allied bombing campaign and its effects on Germany's war economy Calvo-coressi concludes that Germany's Achilles' heel was her fuel oil production and her transportation system, while her greatest economic weakness was the inefficiency of her administrative machinery. But bombing did not win the war. By the time it became fully effective—in the fall of 1943—the Russians had already inflicted heavy defeats on the German army.

The section on the war in the Far East and the Pacific is shorter, though equally well written and organized. There are excellent background chapters on China, India, and Japan, and the major military and naval campaigns are adequately dealt with and lucidly described. There are concluding chapters for the European and the Asiatic sections but there is none for the entire war. The maps and pictures are carefully selected and extremely useful.

The major drawback of this otherwise excel-

lent account is the lack of footnote references. There are a number of statements for which citations to proper authorities would have been helpful. At the same time, the necessity to summarize complicated developments leaves the reader occasionally at a loss regarding additional details. Here, too, proper footnotes would have been worth the additional effort and space. Beyond that, the present volume is, without a doubt, the best general account of the Second World War.

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JAMES BARROS, editor. *United Nations: Past, Present, and Future*. New York: Free Press. 1972. Pp. 279. \$8.95.

ANDREW W. CORDIER and WILDER FOOTE, selected and edited with a commentary by. *Public Papers of the Secretaries-General of the United Nations*. Volume 2, *Dag Hammarskjöld, 1953-1956*. New York: Columbia University Press. 1972. Pp. xiv, 716. \$22.50.

SHIRLEY HAZZARD. *Defeat of an Ideal: A Study of the Self-Destruction of the United Nations*. Boston: Atlantic-Little, Brown. 1973. Pp. xv, 286. \$8.50.

The three books under review zig zag across the various levels and perspectives from which world politics may be approached. The essays edited by Barros focus on the institutions and activities of the United Nations; the Hammarskjöld papers cover a wide range of subjects as viewed by the second secretary-general of the United Nations; Mrs. Hazzard's monograph assesses the organization from the standpoint of the "second" United Nations—that is, from its most populous and widespread branch, the Secretariat. Together these books give us a picture of the personality, bureaucratic, nation-state, and system-wide factors at work in international organization since World War II.

Professor Barros has brought together six original essays that treat the past, present, and future of three institutions—the Security Council, the General Assembly, and the secretary-general—and the role of the UN in decolonization and in the development of international law, as well as its economic, social, and technological activities. Of uniformly high quality, these essays provide an ideal introduction to the subject for graduate students or for special-

ists on world affairs who wish to brush up on recent thinking and developments in international organization. The collection is sufficiently detailed that it could stand also as a reference book. Each essay contains a compact history of relevant events prior to 1945, a careful analysis of major trends since that time, and a brief outline of future alternatives. The essays on decolonization and social-technological activities are particularly interesting. Taken as a whole, however, the collection does not contain sufficiently new data or fresh interpretation to deflect the specialist from reading *International Organization* and other scholarly journals.

Leon Gordenker's essay in Barros on the secretaries-general reminds us of the alternating roles played by such men since the first days of the League of Nations and the International Labor Organization, ranging from overt activism to behind-the-scenes facilitating. All three books agree that Dag Hammarskjöld managed to combine both activism and quiet diplomacy to make his office an important and independent force in world politics. The public papers of Dag Hammarskjöld, therefore, are important sources not only for understanding the UN Secretariat but—even more so—for comprehending international organization in this period (1953–56 are the years covered in this volume). Most of these papers are scattered and hard to obtain, making this collection a must acquisition for university libraries. Cordier and Foote continue the high standards that they set in editing the public papers of Trygve Lie, not only in their selection of materials but in their competent introduction to the entire book and their historical notes giving the context of the selections, which include key reports, statements, addresses, verbatim transcripts, diplomatic communications, and press material of public record.

The most spirited of the three books reviewed here is Shirley Hazzard's *Defeat of an Ideal*, which makes a valuable antidote to the pious tone of the Hammarskjöld papers. Whereas Cordier and Foote served in positions close to the secretaries-general, Mrs. Hazzard worked for over a decade in less exalted niches within the Secretariat. She therefore saw the "other" UN from the ground up, rather than from the top floor. She provides, at a minimum, a heuristic corrective to the official line. Was Hammarskjöld

firm in resisting McCarthyite pressures from Washington to check out and perhaps force out certain U.S. nationals from the Secretariat? Did he spend considerable effort in molding the Secretariat into an effective bastion from which he could mount struggles to establish peace at Suez, in the Congo, and elsewhere? Was he a self-effacing man, catapulted into power but always retaining his own humility and sympathizing with the rank-and-file workers of his home base at Turtle Bay? To these and other questions we gather sharply differing conclusions if we read Hammarskjöld's public statements (or the Cordier-Foote commentaries) and Mrs. Hazzard's analysis.

Mrs. Hazzard holds that the paralysis of the UN—even its aging bureaucracy—may be traced to the failure of Lie and Hammarskjöld to stand up to the United States and other powers that sought to influence the selection of Secretariat personnel. She probably exaggerates the novelty of her charges (which she bases not only upon her own experiences in the Secretariat but also on the revelations of the McCarran and other congressional hearings), for the press conferences recorded in both the Lie and the Hammarskjöld papers show that they and working journalists were quite conversant with the political pressures exerted by the United States and other countries. Whether the secretaries-general practically killed the UN by submitting to FBI security checks for U.S. nationals is debatable. Pressures of this kind were known in the League period and continue to be exerted in various ways by the countries that present lists of candidates for Secretariat employment. A long half-life might in any case be preferable to a sudden demise. The perfidy of the secretaries-general—even if it could be established—would hardly be tantamount to the "self-destruction" of the United Nations, as implied in Hazzard's subtitle. Nor is it clear how "public opinion"—her *deus ex machina*—could ever be mobilized to rejuvenate the UN.

Mrs. Hazzard agrees that Hammarskjöld was the greatest of the four secretaries-general the UN has known. His public papers will occupy three volumes of the series edited by Cordier and Foote, compared with one for Trygve Lie, who occupied the position for a similar period. We need both the public position and critiques such as Mrs. Hazzard's to fathom the depth and

grasp the breadth of the man. We need not only the public image but also the recollections of those who knew him personally and who had to work with him. His own notes, *Markings* (1964), must also be evaluated. Were they the private or—as Mrs. Hazzard suggests—also the public Hammarskjöld? Here is material for a Bergman film, with the difference that the protagonist is clearly a giant among statesmen. Even his public papers display a deep knowledge of poetry in English, French, and German as well as Swedish. They reveal the genial mind invited to address the Museum of Modern Art, finding meaningful analogies between the problems of the “modern” artist and the struggles of the international civil servant. Hammarskjöld’s pride, to be sure, may have been his most serious fault. Like the mystics he revered (clear even from his self-analysis on an Edward R. Murrow program), he talked openly about the dangers of *hubris*, though he often acted as though he were possessed by it; withal he wore his hair shirt close to the skin.

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#### ANCIENT

MASON HAMMOND, assisted by LESTER J. BARTSON. *The City in the Ancient World*. (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xiv, 617. \$20.00.

Although historians have traditionally focused their attention on cities as creators and transmitters of civilization, urban history has only recently come into its own as a specialized field of study. It is therefore of interest that this book appears in a series devoted to the nature of cities and to the character of urban life throughout human experience. The book is also intended to fill a gap, since no general survey exclusively devoted to ancient urbanism exists in the modern scholarly literature.

Professor Hammond defines a city as an organized community exploiting its resources so as to create the economic base for political expansion beyond its immediately supporting hinterland. Concerned only with tracing the rise and spread of cities in areas having contact with the Middle and Near East and the Medi-

terranean basin, he takes as his basic chronological limits the emergence of the first cities in Sumer about 3200 B.C. and the end of classical antiquity, dated to the mid-sixth century A.D. The book naturally falls into three roughly equal parts dealing with the Near East and the Greek and Roman worlds.

He believes that the rise of Sumerian cities was at least in part a response to the challenges of organization posed by the creation of large-scale irrigation projects. Thereafter, urbanism developed widely in the Near East, normally as a result of local economic and political conditions but never without contact with older cities. Direct Sumerian influence, it seems, was extremely important for what became an abortive development of cities in the Indus basin; in Egypt urbanism was a rather late by-product of the political unification of the Nile valley. The author quite rightly insists that the widespread disruptions of civilized life in the Near East at the end of the Bronze Age did not, generally speaking, destroy its urban base.

Such was apparently not the case in the Aegean basin. The author tentatively concludes that there the Minoan-Mycenaean tradition was completely broken and that nascent Greek urbanism emerging in the early Iron Age was an independent Hellenic phenomenon made possible by a return to settled times. Greek life in cities, however, was soon influenced by the older urbanism of Asia Minor and the Levant. He regards as the most original Hellenic contribution to urbanism the invention of the city-state with its characteristic organs of government, magistrates, council, and assembly, and with sovereignty residing with the citizenry, no matter how narrowly defined.

Professor Hammond believes that the urban transformation of Rome depended on initial Etruscan domination but more importantly on Rome’s “historic role” in controlling central Italy. Seen in this light, Rome does not emerge as a true city until shortly before 300 B.C. Although affected by mature Hellenic institutions Roman urbanism was characteristically more primitive, since it never abandoned a preurban organization of the citizenry into tribes and kinship groups. The author emphasizes the most original Roman contribution to urban development as being the practice of dual citizenship; an ever-widening circle of her sub-

jects received Roman status while continuing active service as citizens of largely self-governing municipalities.

Such local autonomy was gradually limited, however, and then all but destroyed during the great crisis afflicting the principate during the third century A.D. Nevertheless, cities in general weathered the crisis and served the Late Empire as centers of civilization, although shorn of any real claim to self-government. In Western Europe urban life continued beyond Roman times until the eighth century, but municipal spirit did not survive to stimulate the general revival of urbanism that began around the year 1000. The author believes that there was greater continuity of the late ancient urban tradition in the Byzantine East and even to some degree in the various Moslem successor states that gradually absorbed it.

Despite a certain tendency to repetition this is a first-rate work of scholarship. Broadly conceived, it has the sweep of a handbook; maps, chronological surveys, an annotated bibliography of 160 (*sic*) pages, and indexes admirably support the text. Although specialists will find much to think about in this volume it will be most useful for readers who approach ancient urbanism with only a rudimentary knowledge of ancient history, since the general historical framework is sufficiently articulated to explain ancient cities to the beginning student.

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CHRISTIAN FROIDEFOND. *Le mirage égyptien dans la littérature grecque d'Homère à Aristote*. (Publications universitaires des lettres et sciences humaines d'Aix-en-Provence.) [Aix-en-Provence:] Ophrys. 1971. Pp. 403.

J. R. HARRIS, editor. *The Legacy of Egypt*. 2d ed.; New York: Oxford University Press. 1971. Pp. x, 510, 24 plates. \$10.00.

Greek literature has much to say about Egypt, albeit, not about the civilization studied by modern scholars but about a wondrous Egypt, the oldest of nations and the mother of civilization. From Homer to the end of antiquity writers increasingly elaborated the idealized portrait of Egypt as the country where wise priests preserved the wisdom of the ages in the mysterious hieroglyphs, priests at whose feet the

philosophers of Greece had sat and studied. Although a study of this aspect of Greek thought has long been a desideratum, complete treatment of the theme would be a herculean task as there is scarcely a Greek author whose works would not be to some degree relevant. Froidefond has made a beginning, however, with this thorough study of the references to Egypt in Greek literature from Homer to Aristotle.

*Le mirage égyptien* is in many ways a notable work. Froidefond's command of the extensive ancient and modern literature relevant to his topic is impressive. Illuminating observations abound, and a number of difficult problems have been significantly clarified by him. Notable in this regard is his demonstration that Homer's various references to Egypt are all compatible with conditions in the eighth and seventh centuries B.C. His vigorous defense of the originality of Herodotus's account of Egypt against those who would ascribe most of the best things in it to Hecataeus of Miletus is equally welcome. And yet, despite its virtues, this is an unsatisfying book. The problem lies deeper than the devotion of much valuable space to the discussion of obsolete theories. Put simply, Froidefond's approach to his subject is literary, not historical. He is not concerned to provide a reasoned account of the development of the Greek image of Egypt, but to determine how eight Greek writers—Homer, Aeschylus, Herodotus, Euripides, Aristophanes, Isocrates, Plato, and Aristotle—reacted to two stimuli: their often limited knowledge of Egypt and the intellectual currents of their own times. To achieve this goal Froidefond allots to each author a separate chapter in which a brief analysis of the state of Greco-Egyptian relations in his time is followed by an exhaustive analysis of the references to Egypt in his works. No general theme, however, unifies the book. Instead, the reader must thread his way through discussions of detail—valuable discussions, to be sure—guided only by vague references to the *mirage* and the significance of this or that aspect of the various authors' works to its development. Scholars interested in the authors treated or in the cultural relations between Egypt and Greece in the period before Alexander will profit from consulting Froidefond's study, but it is not the history of the Greek image of Egypt that its title seems to promise.

*The Legacy of Egypt* replaces in the well-known Oxford series the volume of the same title edited by S. R. K. Glanville. This is not, however, a revision of the earlier work, now over thirty years old but, except for C. R. Roberts's chapter on Greek papyri, a completely new collection of essays by some of the leading figures of contemporary Egyptology. A number of the pieces are of outstanding interest, notably Erik Iversen's study of "The Canonical Tradition" and its relation to archaic Greek sculpture and P. L. Shinnie's eminently sane assessment of Egypt's "Legacy to Africa." The primary significance of the new collection, however, is the picture it affords of the current state of Egyptology. Comparison with the first *Legacy of Egypt* is encouraging. Every chapter reveals an appreciable increase in the data at our disposal and, more importantly, an increasing understanding of that data in Egyptian terms. Space allows for only one example, but it is a revealing one. In seeking to account for the precocious knowledge of some aspects of human anatomy possessed by Egyptian doctors Warren Dawson, in the Glanville volume (pp. 187-88), pointed to the possibilities afforded by the practice of mummification for the study of the human anatomy. In the new *Legacy of Egypt*, however, J. R. Harris (p. 125) cites the evidence showing that Egyptian anatomical ideas were, in fact, derived primarily from animals. Harris is to be thanked for putting together this fine collection of essays, which should serve this generation of students as a convenient and reliable summary of our knowledge of Egyptian civilization as well as the old *Legacy of Egypt* did the last generation of students.

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T. B. L. WEBSTER. *Potter and Patron in Classical Athens*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1972. Pp. xvi, 312. \$17.50.

Professor Webster's admirable industry of scholarship is focused here on some pleasant topics in the field of Greek vase painting. Starting from the famous lecture given thirty years ago by the late Sir John Beazley, "Potter and Painter in Ancient Athens," Webster explores the links among potters, painters, and custo-

mers, and tries to account for the fluctuations in popularity of particular scenes. This endeavor requires enormous knowledge, not only of surviving vases, but of history and prosopography, musical, dramatic, and athletic contests, and cults and myth. There are few other scholars with the detailed knowledge to undertake such a broad survey.

Attractive ideas appear at many points. Webster suggests that the gilded youthful aristocracy of Athens, especially before the Persian Wars, commissioned vases with special scenes and inscriptions. An expensive symposion might be marked by orders for matching winebowls and drinking cups, with the names of the guests inscribed; after memories of the party faded, the vases might be sold secondhand in the western markets of Etruria. Athletes and concert artists might have sold their prize vases, and married couples their wedding gifts. There is no ancient body of literature on the topic of recouping personal finances by selling gifts and prizes, but the idea is more practical than one that involves cultured Etruscans commissioning vases inscribed with labels or *kalos* names of known Athenians.

The book has more to offer—it is a general survey of the fluctuation of popular scenes from about 550 to 400 B.C., with massive statistical tables and bibliographical references. Unfortunately this scheme makes the book very hard to read. Large clumps of pages are solid numbers from Beazley's *Attic Black-figure Vase-painters* (ABV) or *Attic Red-figure Vase-painters* (ARV), reorganized by themes—what one might term a cultural index to Beazley. Of course Professor Webster has clarified distinctions in Beazley's own very efficient indexes, separating lion hunts from hare hunts or women washing from women spinning, and between lists are stimulating suggestions—for individual flute performances as sparks for sudden outbreaks of certain mythical images or how poetic quotations got selected for the scrolls in school scenes. Yet this is not a book to read so much as a reference source for valuable and interesting cultural history, strongly documented. As we look forward to a generation of gleanings in Beazley fields, we are reminded that one of Beazley's attributes was his mastery of English prose.

EMILY VERMEULE  
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## MEDIEVAL

ELEANOR DUCKETT. *Medieval Portraits from East and West*. Ann Arbor: University of Michigan Press. 1972. Pp. 270. \$10.00.

Miss Duckett, professor emeritus of classics at Smith College, has written another fine book, neither difficult nor especially profound, "simple pages" for the student or general reader. As in her *Carolingian Portraits* (1962) the structure is biographical, but now Miss Duckett begins with late antiquity, the barbarian invasions, and the dynasty of Theodosius the Great. Theodosius himself appears first, then in successive chapters persons of his house or its time, in each case paired man and woman: St. John Chrysostom, for example, with the Empress Eudoxia who brought him low; Synesius of Cyrene with his teacher the pagan philosopher Hypatia; Theodosius the Younger with his formidable sister Pulcheria. Then Miss Duckett moves, somewhat incongruously, to the ninth-century West, to Bernard of Septimania and his wife Dhuoda, and at last, inevitably, to Abelard and Héloïse, whose story does bear retelling in Miss Duckett's pretty style.

Some of the figures she includes do not lend themselves so well to this manner of treatment. The Emperor Honorius hardly figures in Miss Duckett's account of his reign, a traditional narrative concentrating on more important persons. Nor can Bernard and Dhuoda be disengaged in flesh and blood from the woeful history of Charlemagne's successors. Biography loses its advantage over less personal techniques when characters remain ciphers. Miss Duckett seems, moreover, to be less at home in late antiquity than in later periods. Her bibliography, admittedly "selected," omits recent works on precisely the figures she treats: major books of V. A. Sirago and S. I. Oost, for example, on Galla Placidia. At times greater care with the details of law and institutions might have added life and color. On July 4, 414, the reader finds, Pulcheria was "proclaimed not only Augusta . . . but also Regent for her brother." This is not in the sources. The Roman monarchy did not admit a formal regency, much less that of a female. Deprived of constitutional status the Theodosian women ruled the Empire by dominating their men.

In a work of this sort, however, these are minor blemishes. To the reader's profit Miss Duckett has worked extensively with the

sources and often allows her people to speak in their own voices. Chrysostom fulminates here against the whole race of women, while Synesius declares a disciple's admiration for Hypatia: "You, my mother, my sister, the teacher of my mind." The passages have been chosen judiciously and gracefully translated, adding to the charm of an attractive book.

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JOHN W. BALDWIN and RICHARD A. GOLDTHWAITE, edited with an introduction by. *Universities in Politics: Case Studies from the Late Middle Ages and Early Modern Period*. (The Johns Hopkins Symposia in Comparative History.) Baltimore: Johns Hopkins Press. 1972. Pp. 137. \$8.50.

Mindful of the interest in university history generated by their own university's brief involvement in politics in 1970, the department of history of Johns Hopkins University initiated the project that resulted in the present collection of "historical case studies of the involvement of universities in politics" during the Middle Ages and the early modern period.

Unfortunately, some shortcomings in the work are noticeable. The introduction repeats unwarranted generalizations, such as that "the eleventh and twelfth centuries witnessed an awakening of medieval society after a long epoch of chaos and stagnation" (p. 3); that "in response to the national aspirations of large populations, territorial monarchies appeared, first in England and then in France, in the twelfth and thirteenth centuries" (p. 11); and that "exclusive admission to membership" was a privilege guarded by the universities "with hypersensitivity" (p. 8). Apparently, my own *Scholarly Privileges in the Middle Ages*, published by the Mediaeval Academy of America in 1962, was not consulted on the subject of university privileges. Disregard of the above work is also evident in the essay by J. K. Hyde on "Commune, University, and Society in Early Medieval Bologna," where much the same ground is covered in different form, to be sure, as that of chapters 1 and 2 of *Scholarly Privileges*. Moreover, Jacques Verger, in the essay entitled "The University of Paris at the End of the Hundred Years' War," although he actually encompasses the years from the early fifteenth century onward, deals with matter



presented in greater detail in *Scholarly Privileges* (pp. 182–226), but with a different emphasis. The account by M. Verger seems slanted against the university. This is shown for example in his assertion that the university, “after presenting the defense of Rouen as an issue of national solidarity . . . reconciled itself to the surrender of that city without any qualms” (p. 52), a statement that the records do not justify.

The third essay, by Howard Kaminsky on “The University of Prague in the Hussite Revolution: The Role of the Masters,” is, on the other hand, an interesting and solidly constructed account of the political role of the university masters that “contrasts interestingly with the religious and non-national orientation of the University of Paris at this time” (p. 85).

The final essay, that of Christopher Hill on “The Radical Critics of Oxford and Cambridge in the 1650s,” provides an illuminating study, based on contemporary writings, of the objections to the universities made by both those within and outside the university. Among the objectives that have a familiar ring, enunciated, according to Professor Hill, by “many junior M.A.’s of Oxford—of the same age as today’s undergraduates,” were “more university democracy and reform of the curriculum” (p. 131). On the other hand, the seventeenth-century radicals outside the university wished primarily to separate the universities from their function of training students for the ministry.

These essays confirm the impression that university scholars were drawn willy-nilly into the political arena and that their writings provide an excellent source of information for the ideas and forces operative in their time. If the above collection serves to direct attention to these all too frequently neglected records, it will have served a very useful purpose.

PEARL KIBRE

*City University of New York*

PETER CLEMOES *et al.*, editors. *Anglo-Saxon England*. Volume 1. New York: Cambridge University Press. 1972. Pp. xii, 332. \$19.50.

In the preface to volume 1, Peter Clemoes states the purpose of *Anglo-Saxon England*: “This new periodical . . . expresses the growing sense of community among scholars working in the various branches of Anglo-Saxon studies in many parts of the world. It reflects their realiza-

tion that the different disciplines . . . aid each other and are but aspects of a common interest. . . . This is the only regular publication devoted solely to Anglo-Saxon studies and to fostering cooperation between them all. . . . By bringing different specializations into direct communication we hope to promote fresh areas of knowledge and to invigorate growth in new directions.”

The articles fulfill the interdisciplinary purpose expressed above. In the first, “The Pre-Viking Age Church in East Anglia,” Dorothy Whitelock demonstrates her remarkable command of all possible sources in reconstructing an establishment completely wiped out by the Vikings. Not only do no pre-Viking sources survive from East Anglia, but the break in tradition was so complete that post-Viking sources are practically worthless. Whitelock must therefore put together a bit from Bede here, a sentence in a saint’s life there, a notation in a Continental manuscript, and so on. Yet her conclusion “that the church in East Anglia was not behind that in most of the other kingdoms, and that it was influential outside its own borders” is convincing. An appendix gives the episcopal succession for East Anglia from the beginning until the Viking invasions. Mary Anne O’Donovan also deals with episcopal dates in her “Interim Revision for the Province of Canterbury, 850–950, Part I.” Tables for each see are followed by notes, some of them quite extensive, on many of the names in the tables.

Janet M. Bately’s “Relationship Between Geographical Information in the Old English Orosius and Latin Texts Other than the Orosius,” taken with her study of classical sources in *England Before the Conquest* (1971), reveals a remarkable number of additions to the basic source. As Bately points out, at least some of these additions may rest on an annotated Orosius or a commentary. If they do not, they indicate the existence of an extremely painstaking scholar in a better library than one would expect in England around 900.

In “The Origin of Standard Old English and Aethelwold’s School at Winchester” Helmut Gneuss suggests that this standard was deliberately developed under Aethelwold’s direction. The thesis rests largely on the vocabulary of certain Winchester texts and, as Gneuss admits,

must stand the test of linguistic studies yet to be made; but he makes a good preliminary case.

Michael Lapidge edits three poems from the Cambridge University Library Manuscript KK. 5.34, 71<sup>r</sup>-80<sup>r</sup>. He assigns these poems to Winchester in the mid-tenth century on the basis of similarities to dated poems of known authorship. These poems are further evidence of some cultural continuity through the First Viking Wars, for their style is that of the southern English eighth century, just as English characters preserve the prose style of that school in the tenth century.

Kemp Malone's "Beowulf the Headstrong," which deals with Beowulf's ignoring both Hygelac's advice not to fight Grendel and his retainers' advice not to fight the dragon, is a salutary antidote to the view that Beowulf brings ruin on the Geats through "avarice" for the dragon's gold. The ruin would have occurred "whether Beowulf died of a dragon's bite a little earlier or of old age a little later."

Literary criticism is represented by four articles: David Hamilton's "The Diet and Digestion of Allegory in *Andreas*," John F. Vickrey's "Exodus and the Treasure of Pharaoh," Daniel G. Calder's "The Vision of Paradise: A Symbolic Reading of the Old English *Phoenix*," and Paul Szarmack's "Three Versions of the Jonah Story." The first three, as their titles suggest, are examples of what might be called the exegetical school of criticism. The fourth is a study of narrative technique in Anglo-Saxon prose. Following these articles are Herbert Dean Meritt's ingenious and on the whole convincing conjectures on twelve difficult Old English words.

There are two paleographical articles. In the first, "The Manuscript of the Leiden Riddle," M. B. Parkes concludes after a most meticulous description of the manuscript that the riddle was copied after the rest of the text, probably at Fleury in the tenth century. "Northumbria and the Book of Kells" by T. J. Brown, with an appendix by C. D. Verey, illustrates the application of three specialties (paleography, archeology, and textual criticism) to a single problem. At one time the tradition of the great insular gospels—Durrow, Durham, Lindisfarne, Echternach, and Kells—was thought to be rooted in Ireland; but Brown places Durrow

in Northumbria and Kells in a center under Northumbrian influence, so that the flow of influence is reversed. The evidence is voluminous and rather complicated, and a good deal of ink may be used in the future on this subject; but I find Brown's thesis convincing in its main outline.

In "The Icelandic Saga of Edward the Confessor" Christine Fell presents a very neat study in intermediate sources. The saga uses material from the *vitae* by Osbert of Clare and Ailred of Rievaulx, but by way of an English service book. Material from Geoffrey of Monmouth's *Gesta Regum* probably came via the *Speculum Historiale* of Vincent of Beauvais, and a miracle involving SS. John and Edward very likely came from a collection of John's miracles. On the basis of the use of the *Speculum*, Fell dates the saga in the fourteenth century.

The two last articles deal with architecture, but in very different ways. In "Structural Criticism" H. M. Taylor suggests rules for a more systematic study of Anglo-Saxon buildings and appends one list of buildings for which there is some documentation and another of buildings that may show construction at two or more dates. In "The Anglo-Saxon House" P. V. Addyman sums up present knowledge of Anglo-Saxon domestic architecture. Until recently the picture was one of unrelieved squalor, since almost nothing but sunken huts had been discovered. At last, however, parallels to Continental "long houses" are coming to light, as well as two royal halls. Probably the sunken huts were often ancillary structures such as workrooms or storehouses. Where huts appear in Roman *insulae*, as at Canterbury, Roman structures may have served as dwellings. Yet excavation over rather large areas sometimes reveals only huts. Perhaps the first settlers had to be content with huts until they could build something better. Whatever their purpose, these huts explain the Germanic preference for gravelly, well-drained sites. Even in a workroom, a foot or two of water on the floor is inconvenient.

The bibliography with which the volume ends is to be continued annually. *Anglo-Saxon England* is off to a good start. Long may it continue.

J. D. A. OGILVY  
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D. E. GREENWAY, editor. *Charters of the Honour of Mowbray, 1107-1191*. (Records of Social and Economic History, new series, volume 1.) New York: Oxford University Press for the British Academy. 1972. Pp. lxxxiv, 307. \$29.50.

A point Sir Frank Stenton made in *The First Century of English Feudalism* (1932) was that charters are a prime source for Anglo-Norman social history. Only a fraction of the vast amount of available manuscript material has been edited, even counting royal charters, so our view of English feudal society still remains very incomplete. The Greenway volume is a welcome addition of new information.

With a *firma* over £365 in 1130—a conservative indication of its worth—the Mowbray honor was of major importance and deserves the attention of social historians. The lords are also personally interesting, not seignorial abstractions. Nigel d'Aubigny, for whom Henry I created the honor, was one of the king's "new men" whose baronial status was achieved by his entry into royal service. His son Roger's career shows how the political stances of barons in Stephen's and Henry II's reigns might be influenced by the manner in which estates they claimed had been acquired or lost at Henry I's hands.

There is more to the charters than "meadow and moor." Two charters (nos. 116, 375) reveal how a tenant might best his lord over the reciprocal obligations involved in tenure. Roger de Mowbray granted a carucate by charter to Rainald de Mildeby for one-fifteenth of the service of a knight until he, Roger, performed some service that he owed Rainald. Rainald achieved this advantage because previously he had obtained a charter from Roger defining what was due him. Evidently upon Roger's satisfaction of Rainald, the tenant returned both the carucate and the charter to Roger. Administrative history is very well served. Some executive role in the household is indicated for the chaplain, Guy. There is a variety of material on the responsibility of the steward for general administration.

In the introduction it is curious that no over-all estimate of the Mowbray honor's value is attempted. A more serious query, however, can be directed at the editor's claim that the clerical staff of the Mowbray household developed into a chancery during the second half

of the twelfth century. There is no discussion of scribal script or of the size of the *acta* and little concerning formulas and drafting techniques upon which to base such an assertion. The illustrations of the two hands of household scribes found in the charters are of little use to paleographers. One can hardly say that a chaplain was a salaried professional simply because there is no evidence of how he was compensated.

Scholars will appreciate the carefully edited texts and useful indexes. Minor criticisms aside, Miss Greenway's edition of the Mowbray charters launches the British Academy's new series, "Records of Social and Economic History," with distinction.

ROBERT B. PATTERSON  
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ELSA DE HAAS and G. D. G. HALL, edited for the Selden Society by. *Early Registers of Writs*. (Publications of the Selden Society, volume 87.) London: Bernard Quaritch. 1970. Pp. cxli, 334, 334, 335-430. £7.00.

Writs were the official letters of medieval England sent by persons in authority to give notifications or to transmit their commands. The king and his officers were the greatest writers of writs, and because in the late twelfth and thirteenth centuries the royal administration became a bureaucracy handling large masses of routine business, the men who did the king's work designed standard forms of writs to take care of recurring cases. Most of the standard forms were for writs that directed the machinery of justice: orders to summon defendants, commissions to judges, instructions for bailing prisoners, and others in increasing profusion. Those who worked with the law began to keep books of forms, "registers of writs." The earliest that survive, from the 1220s, contain fifty-odd forms. Through the centuries that followed the growth of the legal system produced in 1531 the modern printed *Registrum* with about 2,500 entries.

No two manuscript registers are exactly alike. In the volume under review the editors offer a selection of five registers to represent the first hundred years of the development as we know it: two registers of the 1220s, one of the 1250s, another of the 1260s, and a final example from the years 1318-20. Each text

is taken from a single manuscript, but where the manuscripts make bad Latin or bad sense, as they very often do, the editors have emended them, sometimes merely correcting a verb form, sometimes reconstructing an entire writ that was garbled beyond comprehension by the medieval copyist. The editors' learning and judgment never seem to fail; writ after writ shakes off its errors and stands forth in a clear and convincing text. The English translations that accompany the Latin are made to the same standard, an unusually fine blend of accuracy and felicity. Elaborate pains have been taken to make the material accessible; in addition to the alphabetical indexes there is a concordance of the five registers and an analytical index which is itself a work of scholarship.

After a short general introduction by Miss de Haas, Mr. Hall's longer commentary presents his careful thoughts upon the historical problems that gather about the registers. He addresses the old problem of whether, behind the endless variety of the manuscripts, there lay an authoritative official register kept in the Chancery. He believes that there must have been such a thing; and in the early thirteenth century, he finds, it was a file of parchments, the medieval equivalent of a loose-leaf book. Again, he considers the origins of the classic distinction between original writs and judicial writs and concludes that the main lines of the distinction are visible even in the first years for which we have detailed knowledge of the writ system, around 1200. A good many other topics are taken up, invariably with the same fine balance of learning, imagination, and caution that informs this whole work.

DONALD W. SUTHERLAND  
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MICHAEL PRESTWICH. *War, Politics and Finance under Edward I*. Totowa, N.J.: Rowman and Littlefield. 1972. Pp. 317. \$16.00.

Dr. Prestwich has presented us with a first-rate study of the interrelationships of war, finance, and politics under Edward I. He has thoroughly mastered his sources, especially those of the Wardrobe which provided the king with a clerical establishment for the conduct of war, and thus he is able to show us how Edward

waged war and just how expensive his wars were. These parts of the study are the most informative. But Prestwich also makes very useful observations on the political costs of Edward's wars, and the social and economic costs are considered, if only rather briefly at the end. It is natural in the work of a young scholar with an old subject that there should be a note of revisionism in this study, but it is scholarly and responsible. There emerges, for example, a convincing picture of Edward I as a domineering, rather slippery personality who brooked no opposition and reckoned no costs till he was driven to.

After an introductory narrative of the reign the study begins with an illuminating survey of the armed forces that Edward used. The cavalry included paid household troops, feudal magnates with their contractual retainues, and others, numbering some 3,000 in at least one campaign. The infantry were recruited in large numbers; 25,700 can be shown for one campaign, probably the largest English army before 1642. Victualing arrangements for these large armies were superior to those used thereafter until at least Elizabeth's reign or later. The navy was used in the victualing enterprise as well as in the coastal defense of England. The responsible officials, chiefly of the Wardrobe, were thoroughly competent, if also notoriously corrupt.

The costs of all these troops, their supplies, and fortifications was dear. For the period 1294-98 alone Edward's military expenses are estimated at £750,000. Since the normal Crown revenue averaged about £18,000 a year the king had to rely upon taxation. Over the whole reign the taxes on the movables of the laity are estimated at £500,000 and taxes on the revenues of the clergy at £300,000. This was heavy taxation but not nearly enough money to meet Edward's needs. The customs, export duties paid by merchants, were a major new source, but many other financial expedients had to be resorted to. Even so Edward was obliged to seek credit in huge amounts, since he seems never to have exerted control over expenditure; he did what he wanted to do and left it up to his ministers to pay for his orders as best they could.

"By the end of Edward I's reign the crown finances were in a chaotic state" (p. 222). This

financial chaos, in Prestwich's view, is the setting for the political conflicts of the later years of the reign, and the troubles of Edward II are seen as largely the result of his father's reckless extravagance. Some evidence is also given to suggest a breakdown of law and order in the later years of Edward I, which carried on into the reign of his son. The bad economic effects of Edward's policies are listed: the use of Italian merchants precluded the emergence of English financiers; the incidence of taxation and the export of money adversely affected English trade; purveyance and recruitment of soldiers, and most of all, taxation must have weighed heavily on economic production, especially agriculture. Although Prestwich does not draw any conclusion about the relationship between the consequences of Edward I's wars and the "depression" of the early fourteenth century described by Professor Postan, he does indeed suggest a connection that deserves further exploration.

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RICHARD W. KAEUPER. *Bankers to the Crown: The Riccardi of Lucca and Edward I*. Princeton: Princeton University Press. 1973. Pp. xviii, 279. \$12.50.

The Riccardi of Lucca were the first of a number of Italian commercial houses that, in the late thirteenth and the first half of the fourteenth centuries, served as bankers to the English Crown. In this book, originally a doctoral dissertation written at Princeton University, Richard W. Kaeuper examines what he appropriately calls the "Riccardi system" of royal finance, which was operative in England for some twenty-two years, until the firm lost the favor of King Edward I in 1294. The study is based chiefly upon a rigorous analysis of documents today preserved in the Public Record Office. Kaeuper first considers the structure and personnel of the Riccardi bank and the nature of their interests in moneylending and the wool trade. He then examines the changing character of the king's fiscal needs in the middle and late thirteenth century. Growing reliance on a paid army and bureaucracy and the construction of Welsh castles drove up the costs of war and government

beyond what the king's ordinary revenues could support. Edward energetically developed new sources of income by imposing taxes upon wool export and upon the movables of his subjects. These new fiscal recourses, while highly remunerative in the long run, still could not supply him with sufficient liquid funds in periods of crisis. Edward's reliance upon the Riccardi solved this problem of liquidity. From their own funds or from those borrowed from other Italian bankers the Riccardi advanced him liquid capital in large amounts. They were in turn given the right to collect or administer tolls and other sources of royal revenue. Kaeuper congratulates Edward on the success of the system, which, in the author's opinion, provided an essential fiscal support to the other major accomplishments of the reign. Although the Riccardi were disgraced in England in 1294 (for reasons still not entirely clear) and failed not long afterward, Edward and his successors continued to seek the aid of Italian bankers. The system proved its value and endured for another fifty years.

The strengths and occasional weaknesses of this study primarily reflect the character of the sources that support it. The chapters on the structure and personnel of the Riccardi bank are somewhat inconclusive and colorless; the large and still poorly known archives of Lucca itself would have to be consulted to enrich the picture. On the other hand, the description of the "Riccardi system" and its services for Edward I is excellent. The book offers, from a new perspective, many insights into English royal finances and Italian international banking in the thirteenth century.

DAVID HERLIHY  
*Harvard University*

JOEL T. ROSENTHAL. *The Purchase of Paradise: Gift Giving and the Aristocracy, 1307-1485*. (Studies in Social History.) Toronto: University of Toronto Press. 1972. Pp. xiv, 169. \$8.50.

Following the current interest in finding statistical answers to sociological and anthropological questions, Joel Rosenthal offers us a social study of the English aristocracy in the late Middle Ages. By focusing on a specific voluntary activity of giving gifts to the Church, he illuminates the general social behavior of

an important medieval class. Such a study can be best attempted in England because of the well-known advantages offered by the wealth of documentation. Full series of royal records enable him to isolate an important sample of the high aristocracy, the parliamentary baronage, so-called because of their individual summons to parliament by the king. Furthermore, such royal records enable him to study exhaustively their landed gifts to the Church because of the peculiar English mortmain legislation. (To alienate land to the Church one needed a special license from the king.) Using these sources the author has compiled solid and interesting statistics. But England also affords a good number of surviving wills pertinent to the subject. Here the author's endeavor lacks methodological clarity. We are not told how many wills are used, what their validity is as statistical samples, nor how their results can be combined with the other evidence. From these materials his study becomes more impressionistic, resembling the older investigations on which he attempts to improve.

Since valid generalizations are the chief justification of the statistical approach, what are the author's major conclusions? In a preliminary way he states that the family was the functional unit of aristocratic society and that nobles were strongly influenced by local ties. (Since the first was actually the working premise of the study, it cannot be confirmed until alternate premises have been explored.) But neither conclusion will change our long-cherished views of medieval aristocracy. More significant are Rosenthal's negative conclusions: that aristocratic benevolence waned toward the end of the Middle Ages, that the English aristocracy lacked class cohesion in this realm of behavior almost to the point of extreme individualism and idiosyncrasy, that they consciously renounced efforts of social manipulation in their giving, even in refusing to reinforce class distinction, and, most important, that they made no discernible effort to use their benefactions for political purposes. (Even the king refused to use his powers to control aristocratic benefactions.) Since this final conclusion is most surprising and stands at variance with what little we know about kings and nobility elsewhere, such as in France, the author has challenged future scholars to

pursue these studies to explain their discrepancies.

JOHN W. BALDWIN  
Johns Hopkins University

JOHN BELLAMY. *Crime and Public Order in England in the Later Middle Ages*. (Studies in Social History.) Toronto: University of Toronto Press, 1973. Pp. 229. \$10.00.

A century ago L. O. Pike wrote *A History of Crime in England*, but nothing further has appeared on the subject in the Middle Ages until now. In this book on crime in the fourteenth and fifteenth centuries Professor Bellamy uses the many legal records published since Pike's day and others yet unpublished. The seven chapters dealing with misdeeds and misdoers, criminal bands, enforcement of the law, accusation and trials, prisons, punishments, and pardons portray in great detail English medieval crime.

Although Professor Bellamy does not make the comparison, it is remarkable how similar crime was then to that of more modern times. Except for the reaping of greater profits in the Great Train Robbery and in the efficiently organized crime of the Mafia, the techniques seem hardly to have changed. Then, as now, crime flourished upon the connivance of those in government. The members of the royal household and highly placed officials not only abetted and profited from crime but occasionally participated in it. The dishonesty of such lesser officials as sheriffs, constables, gaolers, and even justices was notorious. In an age of retinue and maintenance it was practically impossible to bring the overmighty subject to justice. His indentured bands, often no more than criminal gangs, plied their occupation with impunity. No form of crime was unknown. It ranged from the more violent and serious homicide, murder, robbery, larceny, kidnapping, and rape to bribery, corrupt practices, confidence games, pimps, madams, and their hard-working prostitutes. How often canons, monks, friars, and priests were involved! And as more people became literate and could read some Latin the scandal of benefit of clergy grew. Fifteenth-century crime so impressed Chief Justice Sir John Fortescue that he ingeniously attributed it to "a rather praiseworthy determination not to be

overawed by the law, an independence of spirit which gave and took hard knocks in good part" (p. 31). Then, as now, those who enforced the law had to cooperate with those who broke it in order to bring some culprits to justice. Informing almost became institutionalized. According to Bellamy, "the approver became the agent of the king in preserving public order" (p. 130).

To compile exact data from medieval records is difficult, but Bellamy makes a good case for his assertion that crime and disorder rose alarmingly in the fourteenth and fifteenth centuries. Why? Because much depended upon the presence and the ability of the kings; there were some like Edward III and Henry V who were too often absent from the realm for long periods and those like Edward II, Richard II, and Henry VI who were ineffective. Then, too, the indenture system often blocked the functioning of the common law and the system of courts, while feuding among the great lords and civil war in the fifteenth century both contributed to disorder and obstructed those whose duty it was to apprehend criminals and to make the courts work. Bellamy believes that neither before nor since these centuries "has the issue of public order bulked so large in English history" (p. 1) and attributes it basically to the fundamental social and economic changes in a time of maladjustment and transition from a feudal and manorial society to one that increasingly was not.

While most of the conclusions are backed by sufficient evidence a few seem indefensible or require additional clarification. Contrary to Maitland, Bellamy has argued that most of those indicated did not object to jury trial because they were aware of local sympathy and of the high incidence of acquittals. An interesting point, but it requires further study. Bellamy notes and attributes the sharp rise in pardons granted by the kings to offenders to some understanding that such a practice could save many offenders from being irrevocably alienated from society and could prevent others being blighted by a single crime, but such understanding seems too enlightened for the age. Certainly the main reason for pardons was the royal need for money. Bellamy concludes that punishments for crime became less cruel and barbarous, but he makes no attempt to deter-

mine whether this was a reason for the increase in crime. This is a problem still debated by those in criminology and law enforcement. As for the high incidence of crime, especially in the fifteenth century, one could wish that Bellamy had related this phenomenon to the feeling of many scholars that the civil war and political disorder of the fifteenth century little influenced the daily life of the common man. Bellamy's evidence points to the contrary.

This book is important for students of English medieval legal, institutional, political, and social history who would perhaps welcome only more analysis.

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ARTHUR J. ZUCKERMAN. *A Jewish Principedom in Feudal France, 768-900*. (Columbia University Studies in Jewish History, Culture and Institutions, number 2. Edited under the auspices of the Center for Israel and Jewish Studies, Columbia University.) New York: Columbia University Press. 1972. Pp. xvi, 490. \$20.00.

With a working knowledge of at least a half-dozen languages Professor Zuckerman has demonstrated a prodigious capacity for digging into an immense variety of sources that are rarely mined in a unified context. This labor is not only evidenced in twenty-eight pages of "Select Bibliography" but by hundreds of learned footnotes. Thus it is disappointing that our author has discovered pyrites and cast a grotesque in fool's gold. It is especially unfortunate that many precious pits and pieces of reliable evidence are so thoroughly confounded with the worthless that only the specialist in Carolingian history, after a careful rereading of the book, will be able to form a reasonable picture of the situation.

With a highly selective use of *chansons* and other literary materials from the twelfth century and later Zuckerman sets out to prove that a vast Jewish principality embracing much of southern Gaul and northeastern Spain was established and supported by the Carolingians in concert with the Caliph of Baghdad. Zuckerman maintains that a Jewish king was imported from the East and married to a Carolingian princess to secure this complex arrangement. Count William of Gellone and his large-nosed offspring are identified by Zuckerman as the

Jewish royal family descended from King David.

It is not possible to ascertain why Zuckerman abandoned the canons of historical method, or why he chose to accept as superior the evidential value of fictional literary sources far removed in time from the events under consideration and to relegate documentary evidence of a more contemporary nature to inferior status. This lapse is particularly puzzling because in a recent article on Agobard of Lyons Zuckerman demonstrates a firm command of everything that makes an excellent historian. It is tempting to speculate that Zuckerman's vigorous though implicit reaction against the "lacrymose interpretation" of Jewish history that dominates the textbooks and his disdain for "majority historians" who have neglected and degraded Jewish history overstimulated his imagination. More fundamentally, however, Zuckerman seems to have become the victim of two genres of source material from which it is very difficult to obtain the kind of hard historical evidence that he sought. Efforts to secure sound data about the course of events from *chansons* and from *responsa* frequently lead to the pyramiding of conjectures. Zuckerman indulges in this dangerous practice and then rejects more compelling evidence in light of his previous conjectures.

Our knowledge of Jewish history and of the early Middle Ages in general would have been better served by a plausible, straightforward, well-organized, and clearly written reconstruction of the history of the Jews in southern France and the Spanish March. Despite all of its shortcomings, however, Zuckerman's book is fascinating, a kind of footnoted romance that can be both enjoyable and profitable to the properly cautioned reader.

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LAURO MARTINES, editor. *Violence and Civil Disorder in Italian Cities, 1200-1500*. (UCLA Center for Medieval and Renaissance Studies. Contributions: 5.) Berkeley and Los Angeles: University of California Press. 1972. Pp. viii, 353. \$12.00.

The papers collected in this volume were first read at a conference in Los Angeles in May 1969, a time when violence both on campus and

in Southeast Asia was of deep personal and national concern to all but two of the contributors (Professors Larner and Hyde, who had come from England). "Like many of us," Professor Brentano begins, "I have recently suffered peculiar changes in my perception of violence." And it is worth saying at the outset that this personal concern extended the scope of the treatment of the subject, primarily by including the violence of punishment and control and by connecting the potential violence in a society, especially as evidenced by crime, with the organized violence of faction, revolt, and war, without maiming the enduring value of the papers through too heavy a dose of subjectivity. Moreover, "the history of violence is also a history of the values and institutions that define it," writes Professor Martines, who organized the conference, in his introduction, and though written after the event, this axiom underlay the papers as they were composed and gives the collection a pertinence to any student of northern and central Italy from the thirteenth to the fifteenth centuries.

His introduction, a model of dispassionate appraisal—for it does not avoid criticism of the methodology employed in some of the contributions—is followed by an opening after-dinner speech by myself, which was printed as an act of editorial courtesy but neither calls for comment nor is, I hope, indicative of any bias that might make the reader think twice about the following, necessarily bald account of the ten papers that make up the body of the collection.

In "Order and Disorder in Romagna, 1450-1500," Professor Larner tilts gently against the conference's emphasis on violence in cities; "here violence was almost pre-urban in character: it came from the world of the peasant and the aristocrat, it was the fruit of underdevelopment rather than overcomplexity." And in his review of conditions in Rimini, Cesena, Forlì, Imola, and Faenza, he is careful not to distinguish too sharply between the values and institutions of the towns and their *contadi*. He is particularly illuminating on the effects of clientage on a *signore's* freedom of action, the consequences of broken as opposed to continuous rule, and the need to balance protoclass manifestations of violence against the emotional temper of the region and the age. Professor



Ilardi, in "The Assassination of Galeazzo Maria Sforza and the Reaction of Italian Diplomacy," describes the diplomatic repercussions of the murder of a head of state in vivid if orthodox terms. It is the only paper that is chiefly concerned with the consequences, rather than with the motives and circumstances of an act of violence.

From the *Libro de' Giustiziati* in the Biblioteca Comunale Ariostea in Ferrara, Professor Gundersheimer analyzes crimes punished with death in "Crime and Punishment in Ferrara, 1440-1500." He sees no crime waves, save those associated with political revolts, no dramatic incidences of violent crimes, an increase in hanging at the expense of beheading and burning, and a "relative decline in the criminality of the Ferrarese people around the turn of the century." He recognizes that his source does not give a complete picture; his conclusions contain no surprises, but the rarity of such an analysis, coupled with the caution and neatness of his demonstration, give this paper a modest claim to real distinction. The deftness with which Professor Herlihy humanizes the statistics deducible primarily from the Florentine *catasto* of 1427 in order to show the criminal violence latent in a situation where men outnumbered women, married late, and were considerably more numerous than their wiser elders, will come as no surprise. In "Some Psychological and Social Roots of Violence in the Tuscan Cities" he finds that the predispositions to violence suggested by contemporary moralists fit the demographic evidence, but he leaves himself provokingly little space to deal with any form of organized group violence. Professor Brucker's "The Florentine *Popolo Minuto* and its Political Role, 1340-1450" achieves, with a remarkable blend of evocation and precision, three objectives: determining the effect of the *popolo minuto* on political-constitutional crises, describing the life-style of the poor, and comparing their criminality with that of the rich.

The sources most relied on by Professor Chojnacki in "Crime, Punishment and the Trecento Venetian State" (we are now moving backward in time, according to the editor's arrangement of the articles) are those dealing with the *signori di notte* and the criminal courts. With statistically buttressed dauntlessness he challenges the "magic wand" theory of

the *Serrata*, tackles the problems posed by the high number of transients and foreigners in Venice, and sees the eventual answer to aristocratic and lower-class violence in terms not too much at odds with the "myth" of evenhanded justice. Like Ilardi, Professor Bowsky, in "The Anatomy of Rebellion in Fourteenth-Century Siena: from Commune to Signory?" concentrates on a single moment of violence, the abortive revolt of 1318, but his intention, pursued with both strength and delicacy, is "to attain a more nuanced and accurate knowledge of the role and nature of conspiracy and rebellion in Italian society." Professor Hyde's "Contemporary Views on Faction and Civil Strife in Thirteenth and Fourteenth-Century Italy" moves from the explanations offered for violence (paralleling the initial approach of Herlihy) to examples of revolts recorded by chroniclers, proceeding by way of excellent definitions of terms like "commune" and "Ghibellini" that help give the volume as a whole a value beyond the sum of the crimes and crises it is chiefly concerned with.

Professor Brentano's "Violence, Disorder and Order in Thirteenth Century Rome" (we are still moving backward in time to an increased incidence of violence in all its forms) takes us as far south as we get and into the most elastic of governmental organizations. In part because of the fragmentary nature of the evidence, but also, I suspect, from choice, the treatment is (in the concrete sense of the word) phantasmagoric, and it does not eschew the occasional guess. But the humanity of its tone is impressive and its conclusion, that "the violence of thirteenth-century Rome may have been socially functional, or sociologically functional, but it must have been humanly destructive, even for the survivors," is convincing. Professor Martines ends the volume with a crisp account of the economic, demographic, political, and social causes of "Political Violence in the Thirteenth Century," which has, apart from its own summary value, the merit of encouraging the reader to rethink the problems explored in the work as a whole. The publishers have associated themselves with the spirit of the enterprise by supporting order (the footnotes are at the bottom of the page) and committing a crime (there is no index).

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JOHN BELL HENNEMAN. *Royal Taxation in Fourteenth Century France: The Development of War Financing, 1322-1356*. Princeton: Princeton University Press. 1971. Pp. xvi, 388. \$15.00.

MARTIN WOLFE. *The Fiscal System of Renaissance France*. (Yale Series in Economic History.) New Haven: Yale University Press. 1972. Pp. x, 385. \$17.50.

These two volumes join the unusual number of books that have appeared in the last decade on French fiscal history of the medieval period and the *ancien régime*. Both deal with similar problems but differ strikingly in their purpose and treatment. Professor Henneman presents a most detailed and empirical study of war financing across a forty-year period. He has explored the municipal and departmental archives of much of France as well as the central depositories at Paris and seems to have uncovered every pertinent unpublished document. Professor Wolfe has written a much broader but still precise account of French fiscal resources and procedures across a two-hundred-year period, largely basing his work on a careful reading of published materials with occasional references to archival sources.

American scholars have a conditioned interest in the fiscal history of early European states for the connections that usually exist between the monarch's need of taxes and the rise of representative assemblies. The first of these volumes is filled with material on assemblies used by the first Valois kings: town, bailiwick, and provincial and regional assemblies. They played a less important role under the last Valois, who learned to tax without them. At both ends of the dynasty, unyielding local particularism was the ultimate obstacle to the creation of an Estates-General that could give or withhold consent to taxation on behalf of the nation. In the early years this particularism persisted in spite of the external threat posed by the Hundred Years' War. In the end, it flourished because of the centrifugal force of civil war. And in between, it seems to have been endemic with or without war.

War provided the *necessitas* that enabled the last Capetians and early Valois kings to ask for subsidies from their subjects. With the beginning of the Hundred Years' War in 1337, Philip VI and John II sought tax support annually for the next two decades. In studying this continuous negotiating, Mr. Henneman sees the

emergence of significant patterns of behavior. The north of France, Languedoc, was more easily persuaded to grant taxes, presumably because it saw the threat of invasion more clearly. It preferred to grant a sales tax while Languedoc usually chose direct taxation through the hearth tax. The south always offered greater resistance to taxation, even when the enemy was upon it, and the king resorted to commissioners, who, while seeking the requested subsidy, also investigated and fined for usury, money violations, and *franc-fief* acquisitions, and used other fiscal devices. Consent to taxation was usually sought in local and bailiwick assemblies, but after 1346 the Estates of Languedoc played an important role that signified the growing sense of separateness in the south. The evidence for these patterns is clear enough; we are sometimes left wondering why they developed—why the north, for example, preferred a sales tax and the south a hearth tax.

Unlike Mr. Wolfe, who can bring his fiscal ship into Sully's safe harbor, Mr. Henneman must leave his foundering on the rocks of the 1356 defeat and capture of the king. But he offers one last and significant thesis: as auspicious as the Languedoc Estates of 1355 was, its proposals, not only to consent to taxes but to collect them, ended in failure. Rather, it was the Poitiers disaster that introduced a new era in French fiscal history. Whereas taxation had heretofore centered on the war subsidy, the exigencies of John II's ransom would make the *aides*, *gabelles*, and *tailles* the basic royal taxes for centuries to come.

Mr. Wolfe's contrast of the sixteenth century with the fourteenth leads him to assert that the former, especially the age of Francis I, differed from the latter in three important respects: the appearance of a strong centralized treasury apparatus (the *Épargne*), the origins and growth of venality, and the power of the king to tax without the consent of the people. This last condition originated in the 1430s, in another age of crisis, when a series of grants of *aides* and *tailles* by several meetings of Estates led Charles VII to continue collecting these taxes every year without further consent. This tax "absolutism" of Charles VII led to the fiscal "absolutism" of Francis I, a phrase that I felt, several years ago, described Philip the Fair's reign. This development in Charles VII's reign makes it impossible to think of sixteenth-century

French tax history as merely a continuation of the late Middle Ages. The period from Charles VII through Henry IV can be considered as a single historical unit in French fiscal history.

In addition to this general view, Mr. Wolfe proposes a special thesis when he studies the role of taxation in prolonging the Wars of Religion. The system so carefully constructed by Francis I and Henry II made France wealthy in royal revenues, all flowing upward from the local level. With the outbreak of the religious struggle, this revenue system became one of the prizes sought by the warring groups. In the 1570s the Protestants controlled the royal tax machine of Languedoc and sought consent to taxation from Huguenot assemblies. But this was pure order compared to what followed in the late 1580s and early 1590s, when fiscal disobedience was rampant and France seemed on the verge of utter disintegration. *Grandeess, gouverneurs*, and other lords controlled counties, cities, and regions, and their autonomy, existence, and fiscal rape depended on the continuation of hostilities.

Finally, in reclaiming the machinery of Francis I and Henry II, Sully inadvertently bound future generations, even into the eighteenth century, to the institutions and attitudes of the Renaissance system, "whereby a moribund autocracy tightened its suffocating grip on a great country."

Both historians have included valuable appendixes. Mr. Henneman provides a careful assessment of coinage alterations for a clearer view of the comparative value of tax grants. The last third of Mr. Wolfe's book contains eleven short essays on technical aspects. A twelfth essay on sixteenth-century monetary units would have been helpful. These two studies of Valois taxation belong with the best that has been written on French fiscal history.

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G. L. KURBATOV. *Osnovnye problemy vnutrennego razvitiia vizantiiskogo goroda v IV-VII vv. (Konets antichnogo goroda v Vizantii)* [Basic Problems of the Internal Development of Byzantine Cities in the 4th-7th Centuries (The End of the Ancient Cities in Byzantium)]. (Leningradskii Ordena Lenina i Ordena Trudovogo Krasnogo Znameni Gosudarstvennyi Universitet imeni A. A. Zhdanova.) [Leningrad:]

Izdatel'stvo Leningradskogo Universiteta. 1971. Pp. 219.

Economic and social history of the late Roman and Byzantine Empires has traditionally attracted the interest of Russian Byzantinists. Modern Soviet historiography has continued and developed this trend, frequently making exaggerated use of Marxist categories and methodology. These strictly ideological exaggerations are fortunately rescinding in the work of most serious Soviet historians today.

G. L. Kurbatov has been well known since the mid-fifties as the leading Soviet specialist on the history of the Byzantine city. He is the author of a monograph on Antioch in the sixth century (Leningrad, 1962), which appeared almost simultaneously with a major American study on the same subject (Glanville Downey, *A History of Antioch in Syria from Seleucus to the Arab Conquest* [1962]), and of several important articles on the transformation of the old Greco-Roman city into a medieval "feudal" town. One must note, at this point, that the concept of "feudalism" is used by most Soviet historians in a much wider and less technical sense than is the case in Western historiography.

In his new book, which deals with the same general issue, G. L. Kurbatov studies the relationship between the economic life of city and countryside in the early Byzantine period, the decadence of many small cities, the emergence of large city-centers, the evolution of social structures in the urban population, and the disintegration of the old municipal institutions. The *Codes* of Theodosius and Justinian provide the author with his major primary sources of information, which are used with great intelligence and critical sense, together with such other evidence as may be available in Libanius, Synesius, Chrysostom, and the Byzantine historians.

Kurbatov's thesis on the radical disintegration of slavery, which was the basis of urban economy in the ancient "city," and its replacement by an economy of free enterprise was criticized in 1964 by his colleague, M. Ya. Syuzumov, but is fully maintained in his new book. The new development, in the author's opinion, is connected with such facts as the emergence of small industries, the social and cultural changes provoked by Christianization, and the new role of the Church, as well as with the disappearance

of the old "city's" independence in the framework of centralized imperial structures.

It is unfortunate that very few historians of late antiquity and medievalists have access to Russian publications. They would certainly profit from reading Kurbatov's study. Unlike that of many contemporary Soviet publications, especially on art, Kurbatov's book is anything but luxuriously presented. It is a paperback and, unfortunately, lacks an index. It is, however, a mine of factual information and a piece of very serious scholarship.

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ALKEMENE STAUROIDOU-ZAPHRADA. *Hē Sunantēsē Sumeōn kai Nikolaou Mustikou (Augoustos 913) sta Plaisia tou Buzantinoboulgarikou Antagonismou* [The Meeting of Symeon and Nicholas Mystic (August 913) in the Context of the Byzantino-Bulgarian Struggle]. (Byzantine Texts and Studies, number 3.) Thessaloniki: Center of Byzantine Studies. 1972. Pp. 130.

This book concentrates on an important episode in the relations between Symeon, no doubt the most ambitious medieval Bulgarian king, and Byzantium. Symeon, who had previously defeated the Byzantine armies, again threatened Constantinople, now guided by a regency threatened by internal dissension. But he finally agreed to negotiation, and for this reason was allowed to enter the Byzantine capital in August 913.

A problem associated with these negotiations is the question whether they included the coronation of Symeon as emperor, to what extent was there positive agreement on this point, and how, if at all, was the act of coronation carried out. The sources are not quite clear and modern authorities disagree in their interpretation of them. This is the problem that Dr. Stauridou has attempted to elucidate.

One may say at once that in method Dr. Stauridou's book is an admirable composition, worthy of the high scholarly standards of her mentor. But her conclusions—that in these negotiations there was no question of any coronation and that what the patriarch did when he met Symeon was to give him his blessings—are open to doubt. Granted that the sources are by no means clear; nevertheless they do hint at some kind of coronation, a coro-

nation that under the circumstances could only be performed in some indirect fashion. In Byzantium, although Dr. Stauridou does not agree on this point, coronation by the patriarch was indispensable, for it completed the process whereby one was fully vested with the imperial authority. In this instance coronation meant that Symeon was recognized as emperor—emperor, of course, of the Bulgars—but it meant also that the Byzantines recognized the existence of another empire. Herein lies the significance of the question studied by Dr. Stauridou.

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#### MODERN EUROPE

LAWRENCE P. BUCK and JONATHAN W. ZOPHY, editors. *The Social History of the Reformation*. Columbus: Ohio State University Press. 1972. Pp. xxiv, 397. \$12.50.

While Reformation studies have been languishing in Germany since the Second World War they have attracted a considerable number of young scholars in America. Harold J. Grimm, to whom his former students and American and German friends have dedicated this volume, is one of the historians who has stimulated this lively interest in Reformation history. As Grimm has been particularly interested in the social forces operative in the Reformation the seventeen essays of this volume deal with "the impact of the Reformation on some aspect of society in sixteenth-century Europe." The city of Nuremberg figures prominently in this book because Grimm has been especially interested in Nuremberg's role during the Reformation. The editors have arranged the essays in three categories: the municipal setting of the Reformation, the impact of the Reformation on society, and the organization of the Reformation. The essays are well researched, have good documentation, and deserve the attention of Reformation scholars.

In the first essay, on "The Control of Morals in Calvin's Geneva," Robert Kingdon argues that the stern austerity of Calvinism, particularly in sexual matters, resulted from the rigorous enforcement of morality by the consistory. Kingdon discusses the composition, election, and functions of the Genevan consistory in detail.

It is of interest to know that the vast amount of material emanating from this morals court has not yet been exploited.

In a skillful study of the beginnings of the Reformation in Nuremberg Gottfried Seebass shows that sociological categories such as popular movement or conciliar Reformation do not apply to Nuremberg. Humanist patrician circles were the first to accept Luther's doctrines which then also spread to the clergy and the ordinary burghers, with the evident approval of the town council. Between 1523 and 1533 the city government effected the decisive reforms, following the wishes of the mass of people. Charles E. Daniel has devoted an essay to one of Nuremberg's leading reformers, Wenzeslaus Linck. According to Daniel, Linck did not share the traditional negative view that work was the curse of mankind because of original sin. Work is good because it has therapeutic value for the individual and furthers the general welfare. Unlike Luther, Linck put his ideas on education in a theoretical framework, speaking of the child's mind as a *tabula rasa*. Philip Norton Bebb's essay is devoted to Dr. Christoph Scheurl, Nuremberg's well-known jurisconsult. Bebb offers a very instructive description of the three bodies of lawyers at Nuremberg, their functions, and the complicated court system. While the sketch of Scheurl's diplomatic missions in 1524 and 1525 tells much about Nuremberg's ambiguous religious policy it does not reveal much about Scheurl himself. (One note of caution: the free imperial cities were not sovereign. For example, in 1548 Charles V simply changed the constitutional setup of the free imperial cities in southwest Germany.) Jackson Spielvogel's essay on "Patricians in Dissension" discusses Willibald Pirckheimer's personal quarrels with other members of the Nuremberg town council. Spielvogel looks at these venomous fights from the point of view of the social historian, arguing that the town council made considerable efforts to patch up the quarrels among Nuremberg's ruling families. These feuds also prove that Nuremberg's famous humanist was indeed human.

Richard C. Cole's essay on "The Dynamics of Printing in the Sixteenth Century" argues that the appearance of thousands of books and pamphlets brought about a "change in the consciousness of society." The printed word

added a new dimension to Luther's revolution. As an example he points to the appearance of Eberlin von Günzburg's model of a new society and the enactment of laws on poor relief in the 1520s. Although a bit speculative the essay contains stimulating suggestions. The problem of a causal relation between the German Reformation and the Peasants' War of 1525 is the topic of Hans J. Hillerbrand's essay. Analyzing the social and economic problems discussed by Luther and his friends and followers, he concludes that some of their pamphlets did reveal a certain sympathy for the peasants. Hillerbrand then asks whether the articles advanced by the peasants during the uprisings of 1524 and 1525 revealed the influence of the reformers. Of 1,510 specific peasant articles analyzed only five per cent had to do with religion. Hillerbrand concludes that while the Reformation was one of several factors in the peasant uprising its impact was only very limited. I would like to add that the topic calls for analysis of more evidence than pamphlets and peasant articles. What, for example, was the religious situation of villages that rose up in revolt? What was the religious background of the peasant leaders? Above all, what role did the "Lutheran" ministers play in the uprising, men such as Lotzer, Strauss, or Hubmaier. Indeed, dozens of Lutheran-minded ministers were involved in the uprising. Obviously such a study might require archival work and concentration on a limited area.

In an essay also devoted to the peasant war Kyle C. Sessions analyzes a pamphlet written by Eberlin von Günzburg to the peasants of the Grafschaft Burgau in 1526 when new disturbances were feared. Sessions emphasizes the humanistic form and Lutheran theology of the pamphlet. He publishes its final, interesting section in which Eberlin relates his peace making efforts in the city of Erfurt during the uprising of 1525.

"The Two Social Strands in Italian Anabaptism, ca. 1525-1565," by G. H. Williams, introduces the reader to the rich work of Italian scholars. Following Aldo Stella, Williams argues that while the Italian-speaking Anabaptists of the south Tirol adhered to the theology, and particularly the Tridology and Christology, of the German and Swiss Anabaptists, the Valdesian-Anabaptist circles in Naples and later in Padua were influenced by Marranist and Philo-Judaic tendencies. According to Williams

it was due to Marranist influences that these Anabaptists eventually rejected the doctrines of the Trinity and completely humanized Jesus as the son of Joseph and a prophetic teacher—doctrines that were to lead to Unitarianism.

The intelligence and courage of women during the persecution of Protestants in England in the sixteenth century is the topic of Roland H. Bainton's essay, "John Foxe and the Ladies." Lengthy quotations from transcripts and letters taken from John Foxe's *Actes and Monuments* prove that these women argued very cleverly and defiantly during their interrogations and exhibited great fortitude during their imprisonment and execution. As always Bainton presents his subject in a persuasive form, but I find the tone of the essay a bit lachrymose.

Dannenfeldt's essay on "Wittenberg Botanists during the Sixteenth Century" offers a fresh picture of a little-known topic. It was partly due to Melancthon's open-mindedness and interest in science that Wittenberg played an important role in the early development of botany as a modern science. Numerous outstanding botanists either taught or studied at Wittenberg, among them the great Cordus, Belon, Cornarius, Lonitzer, Clusius, and Rauwolf. It was Cordus who initiated the use of botanical field trips. Dannenfeldt's learned little essay should encourage students to investigate the intellectual and scientific climate of other German universities in the sixteenth century. Lewis W. Spitz has contributed an essay on "Luther's Concern for His Students." Luther made great efforts to secure financial assistance and jobs for his students. Spitz also sheds new light on Luther's view of vocation and the Christian's place in society. Far from rigidly insisting that the Christian must stay in the position assigned to him, Luther thought that the choice of one's vocation depended on many factors, such as the needs of others, personal qualifications, preference, health, income, or living conditions. It was a happy idea to use Luther's correspondence to illustrate this point.

Gerhard Pfeiffer contributed an extraordinary essay entitled "Albrecht Dürer's 'Four Apostles': A Memorial Picture from the Reformation Era." On the basis of impressive literary evidence Pfeiffer maintains that the famous picture of the four apostles was painted after models, to be precise, after Melancthon, Joachim Camerarius, Hieronymus Paumgartner, and

Michael Roting, all of whom were connected with the founding of Nuremberg's secondary school in May 1526. As these four men had done so much for the founding of the new school in the spirit of the four authors of the New Testament they were held worthy to appear as the apostles' representatives. It is an audacious thesis, presented with much learning. Carl C. Christensen has written a short but very instructive paper on "The Significance of the Epitaph Monument in Early Lutheran Ecclesiastical Art (ca. 1540–1600)." He discusses the religious subject matter of the paintings and relief sculpture and inscriptions against the background of Lutheran doctrine. The secular themes on epitaphs deal with the personal characteristics and elevated social position of the deceased. I hope Christensen will extend his studies also to Catholic epitaphs of which there is such an abundance in Germany.

Irmgard Höss's article on "The Lutheran Church of the Reformation; Problems of Its Formation and Organization in the Middle and North German Territories" sheds new light on the first and decisive steps in the establishment of church government in Ernestine Saxony, which secured the dominant role of the secular authorities in the government of the Church. Contrary to Electoral Saxony and Brunswick-Wolfenbüttel, with their consistorial form of church government, Prussia retained the office of bishop until 1584. But in Prussia, too, it was the secular government, not the bishops, that exercised the dominant power in the Church. Hans Liermann's essay on "Protestant Endowment Law in the Franconian Church Ordinances of the Sixteenth Century" discusses the stipulations for four types of pre-Reformation endowments with which the Lutheran governments had to deal: endowments for the benefit of the soul and motive masses, which were considered Catholic and eventually merged with the common chest; ecclesiastical endowments, which were preserved; benefice endowments, which sometimes were taken over by the secular authorities and used to pay salaries to the pastors; and hospital endowments, which were left untouched. The author emphasizes the care taken by the Lutheran governments to preserve the essential purpose of the endowments. Gerhard Hirschmann's essay deals with "The Second Nürnberg Church Visitation" held in 1560 and 1561, a few years after the devasta-

tions of the Second Margrave's War. Altogether fifty-four parishes were inspected. Half of the transcripts of the visitations are still extant. Hirschmann discusses the preparations for the visitation, the background of the visitors, their itinerary, and the results. While the professional standing of forty-six of the fifty-nine pastors examined was considered good or satisfactory many parishioners did not do at all well in the examinations. Hirschmann's essay is a valuable case study showing how much valuable information is contained in visitation transcripts.

In summary, this colorful and wide-ranging collection of essays proves that the social history of the Reformation is still a new field. Once the rich German archives have been fully exploited it may be possible to achieve a synthesis of the many powerful forces and tendencies of one of the most fruitful periods of European history.

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FRANCES A. YATES. *The Rosicrucian Enlightenment*. Boston: Routledge and Kegan Paul. 1972. Pp. xv, 269. \$15.00.

*The Rosicrucian Enlightenment* is the fourth provocative book from the pen of Dr. Frances A. Yates, whose name is just as distinguished in her native England as it is in this country. While a good deal has been written on the subject by enthusiastic, but not too well qualified authors, she has made it the concern of serious scholarship. The topic is intricate and does not lend itself easily to empirical and strictly objective treatment. Despite profound erudition and unparalleled acquaintance with European culture between the Renaissance and the so-called scientific revolution, the author often contents herself with genial assumptions and mere hypotheses. The Rosicrucian manifestoes that kindled imagination and stirred reverberating echoes appeared anonymously, and in some instances their origin and authorship were deliberately disguised to frustrate detection and identification.

Dr. Yates seeks a new approach to the complex field in that she links the emergence of German Rosicrucianism with the invigorating atmosphere that pervaded at least some parts

of the Empire after the marriage, in 1613, of Elizabeth Stuart to Frederick V of the Palatinate. This glorious period, extending for several years, had a rather distressing continuation—the transfer of the royal couple to Prague, the hopeless involvement in the Bohemian affairs, the flight to the Netherlands, and eventually the loss of the Palatinate. "It is not only possible but probable that the Rosicrucian movement, by the time it emerged into print, was connected with the Elector of the Palatinate"—thus the author sums up her findings (p. 54).

Dr. Yates has obviously gone to great labor to prepare herself for the writing of her book, which is distinguished by its breadth and wealth of details. But no matter how superb her narrative is, a critical reader cannot follow her line without pausing and raising challenging questions. The sketch of life at Heidelberg is concise and effective, but no perceptible link has been shown to exist between the splendid residence on the Neckar and the places from which the Rosicrucian manifestoes most likely emanated. Prince Christian of Anhalt, the chief architect of Frederick's foreign policy, stood high above not only his sovereign but also most other Protestant princes, yet his interest in subtle intellectual problems has not been established. Prague is a vague term, used to designate both the imperial court under Rudolph II, the Protestant majority of the Czech Estates, refugees from other countries, men of genius and charlatans. These things call for a more precise definition.

By her remarkably intelligent and inspiring work Dr. Yates has reopened a vast number of problems pertaining both to the early stage of Rosicrucianism and to its reconstruction after a temporary eclipse. The historians of ideas will be grateful for her successful effort to take the subject out of the range of occultist studies and make it a legitimate sector of intellectual history. Her illuminating book is likely to provide material for debate of its specific points for some time to come.

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PAUL FRITZ and DAVID WILLIAMS, editors. *The Triumph of Culture: 18th Century Perspectives*. (Publications of the McMaster University As-

sociation for 18th-Century Studies, volume 2.) Toronto: A. M. Hakkert. 1972. Pp. 387. \$12.00. This volume is the second in the series issuing from the vigorous and fertile McMaster University Association for 18th-Century Studies, the first of which was *The Varied Pattern: Studies in the 18th Century* (1971), edited by Peter Hughes and David Williams. Both of these volumes emphasize the interdisciplinary approach and seek to explore the range and diversity of eighteenth-century culture. In the handsomely printed and well-edited book at hand the perspectives alluded to by the title are preponderantly historical ones, well worthy of the attention of the readers of a historical journal.

Social history is paid its due in several contributions. J. H. Plumb, in "The Public, Literature & the Arts in the 18th Century," discusses concretely the cultural revolution of the eighteenth century—"that process by which literature and the arts have ceased to be the pre-occupation of small specialised élites and have become available for the mass of society to enjoy." Complementing Plumb's article is one by Roy M. Wiles (to whom the whole volume is dedicated) that, with numerous specific illustrations, discusses "Provincial Culture in Early Georgian England." The essay by J. M. Beattie, "Towards a Study of Crime in 18th Century England: A Note on Indictments," is a sophisticated discussion of problems of methodology. Comparative social history is brought into play by George Rudé in his "Popular Protest in 18th Century Europe," a distillation of his important researches and one that discusses not only England and France but also the Pugachev revolt, the Lofthuus affair in Norway, and conditions in the Austrian dominions. Less persuasive, and close to being special pleading, is J. B. Owen's "Political Patronage in 18th Century England." The system, he contends, was no worse than what obtains in twentieth-century welfare states. "Bribery is now on a massive scale, and the muck is more evenly spread."

Economic history is represented by two articles on Spain. "The 18th Century Economic Analysis of the Decline of Spain," by C. Jago, emphasizes especially the ideas of Léon de Arroyal in the 1780s. Similarly, "Utility, Material Progress & Morality in 18th Century Spain," by W. J. Callahan, studies the conflict between

advocates of economic progress and clerical moralists. Finally, a very interesting and thoughtful essay combining local history with reflections upon the theory of history and the relationship of the historian to his subject matter is contributed by D. J. Russo in "The Deerfield Massacre of 1704 & Local Historical Writing in the United States."

The concept of culture is treated by the well-known art historian, Rémy G. Saisselin; the subtitle of his essay, "Tivoli Revisited or The Triumph of Culture," lends itself to be the title of the volume as a whole. Using such examples as Diderot's *Le Neveu de Rameau*, Mme de Staël's *Corinne*, and the David school of painting, Professor Saisselin concludes that "as concerns the eighteenth century one may consider culture [with its strongly historical, moral, civic, and intellectual bent] as the bourgeois invention *par excellence*." It is a complex and inviting argument, worthy of becoming a book.

The volume contains two articles on the history of ideas. A rigorous analysis by R. L. Walters of the various editions of Voltaire's *Eléments de la philosophie de Newton* ("Voltaire, Newton & the Reading Public") yields substantial and impressive results. The additions and successive revisions ended up by making the *Eléments* as much a metaphysical as a scientific work, which became an attack on Leibnitz and on atheism and expressed an increasing skepticism even toward science. L. Rosenfeld, in "Condillac's Influence on French Scientific Thought," with an admirably sinuous and supple style, summarizes Condillac's achievements in epistemology, psychology, and the theory of language.

The history of the theater is presented in an instructive essay by R. Morton entitled "'Blot and Insert Where You Please': The Fortunes of 18th Century Play Texts," which shows what companies of actors were likely to do to authors' completed texts. Two articles from the history of the arts are narrow in range but competent within their self-appointed limits. P. Walton writes of "The Educated Eye: Neo-Classical Drawing Masters & Their Methods"; H. Kalman discusses "The Architecture of Mercantilism: Commercial Buildings by George Dance the Younger [1741-1825]." Both essays are profusely illustrated.



Several of the articles in this volume reflect the current resurgence of interest in the theory and techniques of biography. This resurgence is partly in reaction to the more extreme exponents of the New Criticism who argued that biography was irrelevant to the understanding of a work of art. Partly it is because of the general acceptance of the concepts of identity and identity-crisis first revealed to us by the writings of Erik Erikson. Tracing a person's identity-crisis is intrinsically and inherently a biographical act, as illustrated in this volume by R. Van Dusen on "Goethe's *Dichtung und Wahrheit*: The Emergence of a Poet." Raymond Joly, in "La Fiction Autobiographique," shows what significant biographical information regarding an author can be gained from a novel written in the first person, for example *Gil Blas*. Indeed the difference between an avowed autobiography and such a novel is here affirmed to be slight. M. Joly's contribution is an example of the new and fresh kind of question that structuralists are beginning to ask about literary genres as well as a sensitive probing of the art of biography itself. An informative paper on "Archdeacon William Coxe as Political Biographer," "the first of the English political biographers to make extensive use of private manuscript collections," is contributed by Paul Fritz, one of the coeditors of the volume. "Boswell: The Cautious Empiricist," by C. Tracy, is a sparkling essay analyzing the reasons for Boswell's deficiencies "in a strabismic art such as that of biography." The essay by E. Cappadocia, "Benjamin Constant & Restoration Liberalism," reveals a man in whom "ambition and opportunism went side-by-side with a brilliant intellect and facile pen."

Of very great bibliographical as well as biographical importance is Ragnhild M. Hatton's "George I as an English & a European Figure." Professor Hatton's incomparable familiarity with the sparse and scattered source material, combined with her unrivaled knowledge of eighteenth-century diplomatic history, makes her essay a major event.

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GEORGE RUDÉ. *Europe in the Eighteenth Century: Aristocracy and the Bourgeois Challenge*. (History of Civilization.) New York: Praeger Publishers. 1972. Pp. ix, 290. \$15.00.

In a brief introduction to *Europe in the Eighteenth Century* George Rudé takes stock of the pitfalls besetting the historian of a complex period whose basic characteristics are implied in the title. He must contend in the first instance with the tendency to present prerevolutionary Europe as a sort of background to a revolution that was unique to France. The problem confronts him of how to stress movement and change, so pronounced after the 1760s, without neglecting the necessity to analyze existing structures and traditional continuities. There is the problem, too, of Britain, how to integrate trans-Channel developments with the course of events on the Continent.

To cope with those problems and tasks Rudé follows the approach of grouping them under three large sections, "People and Society," "Government and Ideology," and "Conflict." Within each of these sections he discusses in detail the themes relevant to it. Thus, in successive chapters he deals with these themes and their interrelations one by one: the population explosion, agrarian and industrial revolutions, the growth of a wealthy bourgeoisie, the Enlightenment and the enlightened despots, the *défi* to the monarchy and the aristocracy, popular protest, commercial expansion, and colonial wars. The procedure is not lacking in comprehensiveness.

Of the several chapters dealing with society the one on the aristocracy is a brilliant gloss on a major trend of the entire century. "In most countries in Europe," he writes, "an aristocracy of birth, wealth, and legal status was able to exercise a disproportionate influence over the lives of their fellow men, either as manorial lords, as monopolists of high office in army, church and state, or merely in their way of life or their opportunities for cultural attainment and foreign travel."

At the same time that the "privileged" were more than holding their own against the effort of government to make them contribute to the running of the state an increasingly opulent bourgeoisie was making extraordinary progress in the realm of the spirit. Rudé sees progress in the vitality of religious doctrine and practice as well as in new esthetic values and ideals. Continuing in that line of thought he discusses a new social climate that made it possible for ideas common to all philosophes to germinate, become adopted, and take root in France.

"Broadly . . . only those countries [meaning France] with a substantial middle class able and willing to adopt the ideas of the Enlightenment to its own use could really absorb them."

This analysis of structures and continuities is followed in the last of his three sections by a rounded account of the struggles of the several groups for control of the state within and warfare between the states without, all of which made the last decades what Rudé calls years of conflict.

One final problem remained for him to tackle directly, though he had already investigated it indirectly in preceding chapters—the problem of explaining why there was a revolution in France but not elsewhere on the Continent. On the score of France he states his position with exemplary clarity. "The French Revolution appears then," he concludes, "to have been the outcome of a combination of factors that arose from the conditions of the *ancien régime* . . . and in which all played their part."

As for the other states that escaped a revolutionary denouement, though much tension existed in one form or another, it was not the absence of any one or two particular facts but rather the absence of a combination of factors comparable to those in France that counted. Outside France there was no crisis of authority in society, Church, and state. There were no intransigent claims from the aristocracy, no propagation of radical ideas among wide sections of the people, no trigger of aristocratic revolt and popular rebellion. Outside France the old hierarchical order remained virtually unimpaired and dissatisfactions were absorbed in *ancien régimes* strikingly different from that in France.

With his strong narrative sweep, his skill in subordinating details to large developments, Rudé illuminates the obscurities inherent to his task with compelling persuasiveness. His endeavor is editorially aided by a workable index, carefully chosen illustrations, and a well-selected bibliography. It is patent throughout that by his major assumption concerning the process of history he subscribes to the orthodoxies of the Lefebvre school. But they are orthodoxies with reservations and qualifications that leave room for contemporary revisionist studies. He nowhere contends that the revolution had to take place as a culmination of a long evolution that made a capitalist bourgeoisie

master of the world. Nor are his assumptions synonymous with an objective law of the historical process: they allow for the play of the contingent and the unforeseen.

If both his generalizations and his details are open to disputation, no reader can easily deny the value of this substantial and engrossing work.

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CARLO BAUDI DI VESME. *Studi sul XVIII secolo: Le prime manifestazioni della rivoluzione d'occidente in Francia e nelle repubbliche oligarchiche (1748-1775)*. (Biblioteca di storia italiana recente, new series, volume 14.) Turin: Deputazione Subalpina di Storia Patria. 1972. Pp. 716. L. 15,000.

During the last few years Italian historians like Franco Venturi, Sergio Moravia, and Nicolao Merker have made noteworthy contributions to eighteenth-century studies. All three have concerned themselves primarily with the Enlightenment. Baudi di Vesme has chosen rather to examine the social, political, and economic aspects of European life from the 1740s through the 1770s. The book is divided into two parts: the first discusses France, and the second deals with the oligarchic republics of Genoa, Holland, and Geneva.

In these decades France faced a growing crisis both of leadership and internal stability. In contrast to the Prussian kings Louis XV failed to restructure and strengthen the state bureaucracy. He thus lacked a firm basis of power from which to deal with foreign and domestic affairs. Nonetheless, he continued to claim the prerogatives of an absolute ruler of a strong state. The result could only lead to disaster. By 1763 France's overseas empire had been lost; economic distress was widespread, and mounting criticism of the court and its policies discredited both the person and institution of the monarchy.

At the same time that Louis XV persisted in his ill-advised foreign policy the social character of France was being altered. A new commercial and manufacturing class was emerging, and it resented being excluded from government circles that remained in the firm control of the nobility and the great financiers. The increasingly acrimonious conflict between the king and the parlements and the criticisms

of the philosophes added to the general unrest and discontent. Baudi di Vesme agrees with Madelin that the first tocsin of revolution was sounded between 1740 and 1754.

The crisis within the small oligarchic republics of Genoa, Holland, and Geneva was no less serious. Each faced a different set of problems. What they shared was a superannuated political structure. After 1746 the Genoese oligarchy fronted increasing restiveness at home and in Corsica. It could rid itself of the Corsican problem by selling the island to France in 1768, but the unrest at home persisted to create growing tensions. In Holland a coup d'état in 1747 established William IV as hereditary stadholder, though Holland remained a republic in name. Conflict between the aristocracy and the supporters of the Orange family continued until the revolutionary changes at the end of the century. Geneva's problems derived mainly from the exclusivist character of the city's citizens who discriminated against the *natifs* and the *habitants*.

So much for the contents of this book. It is the result of wide-ranging research in Italian and foreign archives. The author reveals an encyclopedic knowledge of eighteenth-century sources, both published and unpublished. Unfortunately, the resulting book is pedestrian and disjointed. Except for the introduction and conclusion where some generalizations and comparisons are advanced each country is discussed as a self-contained unit. The parts remain parts and never become a whole. True, in his conclusion, Baudi di Vesme recognizes that the book is not an "organic whole," but he goes on to make sweeping generalizations that do not emerge from the contents of the book. Moreover, he claims to have proven that the Palmer thesis is untenable. Too many differences, according to Baudi di Vesme, exist between the American situation and the social, economic, and political accretions of the European past which created tensions that exploded in revolution. In addition, each revolution was the result of particular problems in each country. Thus, Baudi di Vesme rejects the idea that during the last decades of the eighteenth century Western civilization was going through a fundamental revolutionary change. He would have us concentrate on Europe exclusively, but he offers no persuasive reasons for so doing. In

conclusion, this is a disappointing book that, despite its impressive scholarship, falls short of its implied promise to deal with the "Western revolution."

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PEDRO SCHWARTZ. *The New Political Economy of J. S. Mill*. (London School of Economics and Political Science.) Durham, N.C.: Duke University Press. 1972. Pp. 341. \$11.75.

John Stuart Mill is one of the few major economists of whom an intellectual biography is both feasible and valuable. Professor Schwartz has admirably perceived and exploited this opportunity. He has given us a partial intellectual biography of Mill by tracing, through their various shifts and vicissitudes, his ideas about the "applications to social philosophy"—as Mill called them—of the *Principles of Political Economy*. Professor Schwartz summarizes his account under the four headings of "Trade Unions," "Laissez-faire," "Socialism," and "The Future of Society." Some of this is well-trodden ground—for example, the shifts of emphasis to and fro on the subject of socialism. But Professor Schwartz has much that is fresh and significant to offer. Nevertheless, in referring, on the subject of socialism, to Mill's "inexcusable subservience to his wife's opinions" (p. 191) Professor Schwartz rather abruptly dismisses, or fails to take into account, the arguments of H. O. Pappe on "the Harriet Taylor Myth."

The explanation of the adjective "new" in his title seems to be that Professor Schwartz wishes—up to a point quite justifiably—to emphasize the originality of Mill's discussion, especially in books 4 and 5 of his *Principles*. Yet Professor Schwartz himself emphasizes how "in practically every period in the history of economic doctrines, economists have thought that they were witnessing the birth of a 'new political economy'" (p. 15). Indeed, Professor Schwartz also emphasizes how, in some ways, Mill's *Principles of Political Economy* were not nearly "new" enough. He refers to Mill's "rigidity in his unwavering acceptance of the main tenets of Ricardian . . . doctrine" (p. 49). The point is that one cannot separate so clearly his "novel" social applications from his rigid ac-

ceptance of the "older" theories. Mill's social philosophizing was inevitably rendered somewhat insubstantial and hypothetical so long as he held to such central Ricardian positive theories as the "law" of population and natural wages, and the "law" of markets or the Smithian saving-is-investing doctrine. Though Mill made some moves (in book 4, chapter 5 of *The Principles*) toward jettisoning the latter doctrine as one of the now obsolete teachings of the old political economy, he never broke out explicitly. It was only possible for Mill to point toward new vistas that mankind might (or might not) be able to approach in the future. He could not advance himself, encumbered as he was by the Ricardian theories that fixed such severe limits for wages and public spending. Still, though he may overemphasize their novelty, or rather the significance of their novelty, Professor Schwartz has written an important and deeply interesting book about Mill's "applications to social philosophy" of his essentially Ricardian economic theories.

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was let slip, and, although Newgate was rebuilt, an observer noted seventy years later that it "continues down to our day one of the worst hot-beds of vice and moral disease in London." Meanwhile, some of the reformers—neither John Howard nor Elizabeth Fry were among them—had had their way, and the 1840s were marked by the experiment of the "separate system," which pushed the rate of insanity in the new model penitentiary at Pentonville up to ten times the rate in other prisons in Britain.

This is the story of crimes perpetrated against criminals and other prison inmates rather than of crimes committed by the criminals themselves, and Mr. Babington, as James Callaghan writes in the preface, has done a useful service in writing it. Yet crime and retribution (including a vicious prison system) are social phenomena and operate in a social context. But this is a facet that Mr. Babington has not explored, and it is a weakness of the book. This is an excellent compendium of knowledge, but it is not a social history of criminals and prisons.

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ANTHONY BABINGTON. *The English Bastille: A History of Newgate Gaol and Prison Conditions in England, 1188-1902*. New York: St. Martin's Press. 1972. Pp. 250. \$7.95.

This is the story of prison life in Newgate (and a few other prisons) over a period of seven hundred years. It is an informative and well-written account, but it makes depressing reading with its almost unchanging picture of filth and squalor, cruelty, avarice, and official indifference and its long succession of villainous keepers and turnkeys. Yet there are some bright patches to relieve it, such as the exploits of the elusive Jack Sheppard and the unusual humanity of one keeper, Richard Akerman. Such patches, however, are few. One of the most depressing features of the whole story is the stolid resistance offered by the authorities and the public to any plans for reform. A golden opportunity presented itself in 1780 when, during the Gordon riots, the "lower order" of Londoners performed the signal service of razing the prison to the ground. (This was three years after John Howard's first indictment in his *State of the Prisons*.) But the opportunity

S. B. CHRIMES. *Henry VII*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xv, 373. \$16.00.

"Schooled in adversity and disappointment," Professor Chrimes relates, the twenty-eight-year-old Henry Tudor ascended the English throne. He founded a dynasty that prevailed against pretenders, heretics, and foreigners. Sometimes seen as a more mysterious character than his son, grandson, or two granddaughters, Henry has not inspired the romantic interest or partisanship that a Henry VIII, Edward VI, Mary, or Elizabeth I have. Even staunch defenders of the Yorkist Richard III wish to absolve Richard of his nephews' deaths rather than to discover how Henry, whom they indict, did it. Ironically, they view Henry as so uninteresting a murderer that neither his personality nor crime merit investigation.

While Chrimes concludes that Henry was no murderer—the author believes Richard was responsible—he is not really interested in plucking out the heart of Henry's mystery. Admittedly the surviving voluminous records are not of the sort that facilitate a Freudian or

Eriksonian biography. Again, Chrimes's métier is political-administrative-constitutional history. For example, he has written two volumes essential for introducing students to fifteenth-century England, *An Introduction to the Administrative History of Mediaeval England* (1966) and *Lancastrians, Yorkists and Henry VII* (1966). Legal historians are also indebted to him for his exemplary edition of Sir John Fortescue, *De Laudibus Legum Anglie* (1942) and his revision of Sir William S. Holdsworth, *History of English Law* (1956). The virtues of these works are readily apparent in his *Henry VII*: patient historical reconstruction, close attention to detail, plausible explanation, and lucid presentation.

Chrimes's contribution is, as he says, an "interim report." He bases his study on a thorough review and analysis of all the printed sources as well as a discriminating reading of all secondary works. His synthesis of existing views of Henry's political-constitutional-administrative-fiscal ambience is a major contribution to Tudor historiography. Like the authors of two companion volumes in the English Monarch series—J. J. Scarisbrick, *Henry VIII* (1967) and Frank Barlow, *Edward the Confessor* (1970)—Chrimes is a demythologizer. His Henry VII is not a brave new monarch leading the English into the modern era. The old legend of Henry's avarice, however, is given some credence. Henry's attitudes are set in the context of his situation as a personal monarch ruling in the days of highly personal monarchy. Chrimes does not inflate Henry's importance by making him a creative systematizer, innovator, or enforcer of the law, nor a brilliant administrator or paternalistic despot. Thus Henry becomes the heir of his immediate predecessors and the product of a long, mediæval tradition; hence Chrimes gives the final *coup de grâce* to the notion of Henry's "new monarchy." Chrimes concludes that Henry was "not a creator but rather a stabilizer" and, as such, an important link in the development of English institutions.

Chrimes utilizes the work of such scholars as G. R. Elton, Paul Murray Kendall, J. R. Lander, W. C. Richardson, R. L. Storey, and B. P. Wolfe. However, he does not accept their work without reservation and in almost every case corrects errors of fact or interpretation. The result is a solid study worthy of its

series. Nonetheless, Chrimes's observation that Henry VII was "not personally interested in religion" is jarring, and one wonders why Henry would have requested ten thousand masses in his will if he were not. Because Chrimes informs us that Henry did not have an illegitimate son nor an army of paid informers and never fined the earl of Oxford £10,000 for illegally retaining, as Bacon maintained, the bloom is off Henry's new monarchy. Thus, Tudor historians must revise their lectures. Given the nature of the evidence, perhaps we will never have the intimate biography that we want.

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ARTHUR J. SLAVIN, editor. *Tudor Men and Institutions: Studies in English Law and Government*. Baton Rouge: Louisiana State University Press. 1972. Pp. viii, 294. \$11.95.

Dedicated to Professor Walter C. Richardson, this volume of essays by his friends and students provides new light on several corners of Tudor administration. The essays, mostly modest in length and unpretentious in scope, generally reflect the same careful scholarship and the same concern with the governmental machine in operation that characterized Professor Richardson's own major works.

The theme that unites most of the essays is concern for power—how Tudor ministers acquired it, how they employed it, and particularly the restraints that served to check it. These writers tend to find that Tudor officers were repeatedly frustrated by intramural conflicts and jurisdictional disputes, by restrictive administrative traditions, and by the stubbornly maintained interests of other functionaries. That is especially true of the longest of the essays, W. J. Jones's examination of the Exchequer Court of the County Palatine of Chester. Even though the chancellors were nearly always major court personalities, they were never able to overcome problems caused by squabbles with the city of Chester, factiousness of underlings, jurisdictional limitations, and the impossibility of close supervision from court. Indeed, some of the influence the Chester Exchequer Court retained simply reflected the

power of figures like Leicester and Egerton who were chancellors. Jay P. Anglin, looking at the efforts of the church hierarchy to enforce discipline on the Puritan clergy of Essex through the "Bawdy Courts," comes to similar conclusions. Because the courts had been given new duties, because of their dependency on reluctant and ill-prepared church wardens, because of opportunities for delay and evasion inherent in the canon law procedure, and because of the autonomy of the lower judges between visitations, the courts had no incentive for bearing down hard on the "godly" clergy. R. W. Heintze finds that the Statute of Proclamations of 1539 was ineffective because of the limits placed on the time for prosecution and on quorum requirements for the court when cases were heard. A. J. Slavin draws attention to the rivalries between major offices that killed reform of Chancery in the last years of Henry VIII. DeLloyd J. Guth concludes that the Court of Exchequer Pleas, once a major organ for protecting the interests of Crown officers, had declined into insignificance, a place where local officers could be sued for debts.

Only one of the articles, that of Mortimer J. Levine on Henry VIII's prosecution of the duke of Buckingham, stresses the breadth of Tudor power rather than its limitations. Levine sees the judicially and morally indefensible treason charge as a kind of flexing of the royal muscle, a demonstration to the realm that Henry could destroy any subject, no matter how great or how near the throne. Stanford Lehmberg purports to deal with Sir Thomas Audley's use of power, to see if his soul were actually as black and his heart as hard as the marble of his tomb, as his epitaph proclaims, but he is actually more interested in Audley's accumulations of offices and lands. The subject matter of two of the essays does not fit into the structure that has been imposed: J. R. Lander's on the 1475 campaign of Edward IV, which concludes that the French wars could no longer generate enthusiasm and had to be justified to the country in terms of defense; and Louis Knafla's study of the Elizabethan Inns of Court, which suggests a "revolutionary" expansion in matriculations and transformation in content of lectures.

The final essay, by G. R. Elton, is set apart in a different way: it is not a result of new research, as the others are, but a new round in

his debate with Joel Hurstfield on law and liberty in the Tudor state. Elton reaffirms here his conviction that Tudor rule was moderate, limited, and essentially constitutional. His emphasis is again on law: the prerogative law was considered a department of the common law; the Tudor monarchs did not make law by themselves or wish to acquire the power to do so and did not transgress the conventions of law in their rule. Therefore Tudor rule was not despotic. Hurstfield, on the other hand, is concerned not with law as such but with the area of freedom in which the subject feels able to move. For him despotism is a government which "enforces its will, suppresses dissent, and rules a society whose members have few means of influencing major decisions." By these standards, which Elton does not really refute, the Tudors were despots. Their perspectives are so different—Elton's legal, Hurstfield's humane—that they cannot meet on the same grounds. Each of these eminent scholars, ironically, has been forced to the conclusion that the other does not understand the essential nature of the Tudor state. Out of this kind of disagreement important new interpretations may come.

ROBERT W. KENNY

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BERYL PLATTS. *A History of Greenwich*. Newton Abbot: David and Charles. 1973. Pp. 231. £3.50.

A royal residence from the fifteenth to the seventeenth centuries and an important port in the days of sail, Greenwich, four and a half miles downstream from London Bridge, always had its fortunes joined with those of the metropolis. By Victorian times the two were physically linked: "London has stretched out its arms to its pretty cousin," observed George Rose Emerson in 1862; "fourpenny steamboats and crashing railway trains have cemented the union . . . and Greenwich . . . is really now nearly as much a suburb of the great city as Hackney." Greenwich deserves a full and careful modern study as much as any London suburb. Mrs. Platts's book unfortunately maintains the grand tradition of English local history in its anecdotal approach and its tendency to devote attention to the periods of Greenwich's past in inverse proportion to the amount of hard evidence available. Time spent demon-

strating its possible identity with pre-Roman Trinovantum and connecting it to incidents related by Geoffrey of Monmouth would have been better devoted to expanding its well-documented history from the Tudor period to the present. The book has a brief bibliography but no footnotes; the pleasant illustrations include a portion of Rocque's *Survey* of 1746 but no other maps.

DONALD J. OLSEN  
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JOHN CANNON. *Parliamentary Reform, 1640-1832*. New York: Cambridge University Press. 1973. Pp. xiv, 333. Cloth \$17.50, paper \$6.95.

This book is a useful pioneer presentation of the story of parliamentary reform in Great Britain from the English Civil War to the Reform Act of 1832. To avoid discursiveness Dr. Cannon has narrowed his investigation "to the two most important aspects of reform—namely changes in the franchise and changes in the distribution of seats." Other proposed reforms receive mention only in relation to these themes. His first chapter and supporting appendixes bring out interesting parallels between ideas and programs of the mid-seventeenth and the early nineteenth centuries. An extensive range of recent publications and original research is made the basis for rejection of an overstatic picture of eighteenth-century political society, which the work of Namier may have helped to consolidate. We may note here, however, the value of Namier's last and greatest scholarly enterprise, *The History of Parliament*. Five volumes now published covering 1714-90 illuminate the gradual transformation of the political system (which, as Mr. Cannon says, changed "subtly but inexorably, decade by decade") and so contribute to a better perspective of later eighteenth-century reform movements.

Cannon's book contains much interesting matter on the events leading up to 1832. Also the author has fresh things to say about the Reform Act; in particular, he emphasizes its conservative character and intention. He avows a special interest in tracing the relationship between ideas and actions, "to see why arguments which are pressed ineffectually at one moment become politically viable the next." In specific contexts this is achieved. But one would

have welcomed some reflection on the more general change of intellectual climate that was gathering momentum between the mid-eighteenth century and 1832. Men had previously thought in terms of a static order. Thus the science of politics was conceived as already established by human experience for all time, equally valid in ancient Athens and modern Britain. After 1750 growing knowledge and intellectual speculation were undermining this world picture at many different levels. By the early nineteenth century such thought patterns were yielding to ideas of flux, change, and human capacity for deliberate, engineered improvement. No doubt Mr. Cannon is right in saying that Bentham had little influence on the Whig reformers, owing to his tortuous style. But the advancing revolution in ideas and attitudes, of which Bentham's work was only a part, helped to sap Burkeian faith in prescriptive authority and to generate and sustain the public pressure before which Earl Grey and his colleagues felt compelled to yield.

IAN R. CHRISTIE  
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JUDITH BLOW WILLIAMS. *British Commercial Policy and Trade Expansion, 1750-1850*. With a bibliographical chapter by DAVID M. WILLIAMS. New York: Oxford University Press. 1972. Pp. x, 514. \$24.00.

This book appears in unusual circumstances. It was newly complete at the time of the author's death in 1956 but has taken sixteen years to achieve publication. Its appearance now is fully justified, for it is a work of fine scholarship on an important subject. Probably no other single volume has presented so much new information about the detailed content and conduct of British commercial policy. Judith Williams had not only thoroughly sifted the voluminous official papers but had mastered a larger and more varied range of contemporary printed materials than any other writer in the field. She never allowed the resulting abundance of material to overwhelm her, but presented it with great clarity.

Commercial diplomacy is the aspect of policy that receives most attention. Thus the book is very different from most studies of Britain's approach to free trade, which it both supplements and corrects. Free trade policies were

not achieved solely through the shift in the balance of power between rival interest groups at home. Their content was not molded simply by the conversion of politicians to a belief in minimal regulation of trade and by a consequent unilateral decision to legislate accordingly. This is a book about governments that were constantly active: always informing themselves about the policies of other states and adjusting to them; gaining more security for British merchants first in one place, then another; giving a little here to take a little there; resisting the more stifling foreign restrictions without demanding concessions on a scale that was politically impracticable. It is not the whole story, but it was a missing piece and is missing no longer.

The book's contribution to the history of trade expansion is much weaker. The main subjects studied under this heading are the acquisition and development of colonies and trading stations and the revelation of new territories by journeys of exploration. Much of the information is fascinating, but it throws little light on the growth of trade in most of this period. The approach results in inadequate attention to Continental Europe, which was still Britain's largest export region and had great potential for commercial expansion. This shortcoming of the book is the more serious because the author was remarkably incurious about the economic effects of particular measures of commercial diplomacy. She cited occasional figures for the value of trade with a few countries in particular years, but had no feeling for the long-term movement of quantities. When a set of duties or prohibitions was changed, or a new commercial treaty was brought into effect, she made little enquiry about the nature and magnitude of changes in the related trades. Only in a few cases where the changes were apparently negligible does she show a realization that trade expansion might depend far more on such things as transport and the nature of local industry than on commercial diplomacy. In this respect the book suffers from an old-fashioned treatment of its subject; but its study of the details of commercial diplomacy has old-fashioned virtues for which it will long be used.

WILLIAM ASHWORTH  
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PATRICIA B. CRADDOCK, editor. *The English Essays of Edward Gibbon*. New York: Oxford University Press. 1972. Pp. xix, 650. \$33.75.

Editing Gibbon is no easy task. His handwriting is firm and clear, but the vast range of his interests and his enormous erudition require of the editor a mastery of recondite references and a hard-won intimacy with one of the finest historical minds we have ever known. Fortunately, this edition is the product of this type of devotion and scholarship. With the exception of the autobiographical works—memoirs, journals, and letters—and the *Decline and Fall* it contains all of Gibbon's writings in English, from his bold chronology of world events, written in 1751, to his final reflections on the vocation of history, produced in the last months of his life. As a result of Patricia B. Craddock's labors we may glimpse Gibbon at work over forty years of his active intellectual life.

Gibbon's friend and literary executor, Lord Sheffield, created particular problems for all subsequent editors of the historian's memoirs and journals, for he believed it necessary to purge these papers of much important information. The editor of the present edition faced similar problems and overcame them. Where Sheffield corrected Gibbon's spelling we now read the original and have a footnote with the Sheffield variation. Hence, we have essays and notes, marginalia and commonplace books, just as Gibbon left them and not as his friend thought we ought to see them. Craddock's restoration satisfies our wish to read Gibbon in the original, and it is, moreover, interesting from a graphic point of view: uneven punctuation and misspellings make many of these pages curiously pleasurable to the eye. We have the authority of the original manuscript clearly deciphered for us, and we can still see what the manuscripts look like.

The plan of the edition is chronological, and we detect the turning point in the development of the historian to be the publication of the *Decline and Fall*. Before the *Decline and Fall* years he roams through chronologies and notes, commenting freely and critically on popes and ancient kings, on the antipathy between philosophy and Christianity, and on the history of the laws of England. After his great work reaches the public he is concerned with



the craft of history in a professional way. The longest, certainly the most entertaining, piece in this volume is Gibbon's *Vindication* of his fifteenth and sixteenth chapters. This brilliant bit of revenge was, of course, printed during Gibbon's lifetime, but the editor has done well to reprint it here. The *Vindication* forms a significant part of Gibbon's self-awareness as a historian, and it should be read in combination with his other notes regarding possible "corrections and improvements" for the *Decline and Fall*. Gibbon condescended to answer Henry Edward Davis's fatuous diatribe because he wanted the *Decline and Fall* to be accepted as the work of an accurate and critical historian, not merely as "a tale amusing enough." The same concern for critical history permeates his last works, notably his lengthy *Antiquities of the House of Brunswick* (1791), which until now has been available only as a fragment of the original, and his "Address," which he wrote only months before his death. In this last piece Gibbon calls for the publication in England of "our latin memorials of the middle age." He cites the works of the Benedictines as the model for this type of scholarly activity. He knew to whom he owed his debts.

More might be asked of the editor. There could be additional introductory material, and it would be convenient if the dates of composition were placed nearer the essay or marginalia in question; but it seems more appropriate to express gratitude for the accomplishments of this handsome edition. Obscurity and Lord Sheffield no longer separate Gibbon's audience from the complete body of his English works.

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Baltimore

BERTRAM H. DAVIS. *A Proof of Eminence: The Life of Sir John Hawkins*. Bloomington: Indiana University Press. 1973. Pp. xii, 436. \$10.00.

Although Dr. Johnson's unclubbable friend John Hawkins deserved his comment that "it is no less a proof of eminence to have many enemies than friends," Mr. Davis documents more acceptable achievements in a surprising variety of public roles. The only son of a carpenter, Hawkins briefly studied architecture,

became an attorney, acquired brilliant associates and aristocratic neighbors, and made his mark as antiquary, editor, music scholar, pamphleteer on local issues, magistrate, and biographer. As we can infer from this *Life* if we wish—Mr. Davis risks few generalizations—Hawkins's career demonstrates what was open to a diligent, energetic, and intelligent bourgeois of the eighteenth century, particularly if he married an heiress.

Limiting himself largely to what can be documented (unlike Percy Scholes in his 1953 *Life of Hawkins*), Mr. Davis concentrates on the external and the public. But if treating biography as a series of research reports (for example, "Chapter 6, A Shakespeare Quartet," on four scattered contributions) avoids Hawkins as a person, it lucidly shows us a great deal about his activities, particularly as magistrate. From freshly examined sessions papers, Mr. Davis usefully describes the operation of the quarter sessions and general sessions in the county of Middlesex; the problems of keeping the members of the court honest; day-by-day courtroom conditions; an intricate bureaucratic squabble over an official's incompetence; the details of prison maintenance and administration; and the building of a new sessions house at Clerkenwell Green, a major achievement of Hawkins's administration. Re-examining all the old evidence and uncovering a good bit that is new, Mr. Davis is able to exculpate Hawkins from M. Dorothy George's charges of dereliction of duty in the Gordon riots and cruelty toward the accused in his court ("Sir John Hawkins as a Justice of the Peace," *National Review*, 88 [1926]: 433-41).

On Sir John as a literary figure, Mr. Davis can add a degree of precision to the listing of early contributions to the *Gentleman's Magazine*, to pamphlet attributions, and to the quarrels deriving from Hawkins's literary competitions with Moses Browne over editions of the *Compleat Angler*, with Charles Burney over histories of music, and with Boswell and Mrs. Piozzi over biographical works on Johnson. But he will not speculate why Hawkins—evidently decent, able, and learned—insulted Burke beyond the Club's tolerance; antagonized old friends like Garrick and Percy and repelled likely new ones like Bentham; and made a point of attacking the best music and literature of

his day. Rather than hazard a possibly arbitrary definition of Hawkins the man, Mr. Davis provides scholarly assessments of the surface of his life. If Hawkins was only an attendant on literary history—Boswell's Rosencrantz or Guildenstern—who happened also to be very busy when off stage, this would be enough. In persuading us to the contrary, however, Mr. Davis creates a need he does not satisfy.

MORRIS GOLDEN

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Amherst*

JOHN W. OSBORNE. *John Cartwright*. (Conference on British Studies Biographical Series.) New York: Cambridge University Press. 1972. Pp. ix, 173. \$12.95.

Professor Osborne has written the first serious biography of Major John Cartwright, "the Father of Reform" (1740–1824). The weaknesses of Osborne's book derive from the limited available sources. Cartwright's doting niece published an uncritical *Life and Letters* (1826) just after his death, and those volumes still provide most of the personal details about the octogenarian radical. Lacking family or personal papers, students must depend on the major's voluminous and rather dreary political writings, some correspondence in other collections, and the often unflattering comments of contemporaries. Professor Osborne has drawn together what little we can readily know about John Cartwright, the person, but for all of Osborne's sympathy the man remains less than life size. He seems a crashing bore. Cartwright was frail, socially insensitive, didactic, stubborn, narrow, and brave. The major moved with a strong sense of family loyalty. He was friendly with such diverse, difficult people as Earl Stanhope, an irascible genius given to disinheriting his children, or Thomas Wooler, the colorful, quarrelsome, radical publicist.

Cartwright had a good eighteenth-century democrat's disdain for the lower orders, the unruly poor, the "crowd." Osborne repeatedly states that Cartwright was economically naive and thus insensitive—for instance, to the basis of Luddite discontent. But Osborne's own evidence runs precisely to the contrary. Cartwright shifted from a marginally profitable estate to a substantial holding at Brothertoft, near Boston,

Lincolnshire. With an eye to the rapid growth of the cloth industry, he organized a cartel of woad growers to hold down wages and control supply. He derived immense profits from the rigged market in this vital dyestuff. Cartwright's brother Edmund invented the power loom and went bankrupt. The major fought the patent infringements in the courts and helped to secure the £10,000 award to his brother in 1809 from Parliament. While unhappy that his nephew Edmund would not continue the profitable exploitation of Brothertoft after he died, Major Cartwright recommended banking to the lad as the easiest quick route to wealth.

Osborne is principally concerned with the political Major Cartwright. He leads the reader carefully and perceptively through the repetitive, limited range of Cartwright's ideas. Osborne demonstrates that there was some slight growth, although the grounds laid out in *Take Your Choice!* (1776) were modified more in detail than substance. Cartwright's program—universal manhood suffrage, vote by ballot, equal electoral districts, single member constituencies, annual parliaments—would be enshrined in the Chartist platform and, with the exception of annual parliaments, was to be enacted in the course of the next century. His notions on jury selection would shortly be accepted, while his advocacy of a freeholder militia added a quaint, outdated quality in the era of the French and Napoleonic wars.

How is one to explain the importance and influence of an incompetent practical politician, a less-than-marginally-successful political agitator, a man of relatively few novel or striking ideas and little literary felicity? Most of his notions were advanced by other contemporaries. Thomas Paine said more better but had less lasting influence in England. For one thing, as Osborne points out, Cartwright had longevity and consistency on his side. He earned the sobriquet "Father of Reform" by persistence and seniority. But he also won it by establishing his program on the only basis for successful radical propositions in England. Everything he sought, Cartwright argued, was merely a restoration of lost rights and freedom. The major was completely English. Scotland and Ireland barely came within his purview. He was pro-American, not anti-English, during the American Revolution. While a reformer, he was not a revolu-

tionary. He never succumbed to radical pro-French hysteria. He had the strength and limitations of his class, his age, and his patriotism. Professor Osborne and the Conference on British Studies Biographical Series have served us well with this volume.

EUGENE C. BLACK  
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RICHARD W. DAVIS. *Political Change and Continuity, 1760-1885: A Buckinghamshire Study*. (Library of Politics and Society.) [Hamden, Conn.:] Archon Books. 1972. Pp. 262. \$11.00.

This is a lively and contentious book of wider significance than its title may suggest. Professor Davis is concerned to challenge the view that the changes introduced into the British electoral system by the 1832 Reform Act were of only marginal importance and that the landed class was able to retain the greater part of its power because of the continuation of the habit of deferential voting in the counties and many of the smaller boroughs. He does not, of course, deny that the personnel of Parliament remained overwhelmingly landed after 1832, but he insists that this was not primarily because rural voters were dragooned into supporting them. Instead, he stresses the independence of many electors, the interest shown in public issues, the swings of opinion hardly accounted for by swings of property, and the skill and flexibility employed by the leaders of county opinion in order to maintain their position.

Professor Davis approaches his task with the zeal of an inquisitor rooting out heresy. His first victim is D. C. Moore, who has always seemed the most vulnerable of the exponents of "continuity." The attack on Norman Gash's arguments is, perhaps, less successful. Indeed, I am not sure that Davis and Gash are as far apart in their conclusions as the former appears to believe. The wealth of illustration that Professor Gash offered in support of his contention that many pre-Reform practices died hard may have obscured the moderation of his final assessment: while pointing out, for example, that a number of proprietary boroughs survived 1832, he admitted that "naturally the great era of the borough-proprietor had passed away for ever." Nor does Davis deny that landlords after 1832 tried to influence voters and that some

continued to evict recalcitrants. He is certainly able to demonstrate that Buckingham changed from a two-seater borough with no questions asked to one in which the duke could not be sure of keeping out an opponent—but Gash claimed no more than one seat for the Grenvilles after 1832.

What Professor Davis does establish is that there were no constituencies in Buckinghamshire after the Reform Act in which one could say of the landed class—in Professor Moore's words—that its command was absolute. The argument would carry even more weight were the book not marred by misprints and misdatings and cumbered with an erratic index.

Professor Davis is well aware that it would be rash to generalize from the experience of one county. It is unfortunate that so much of his evidence comes from Aylesbury, which, even in the snug days of the Pelhams, had a reputation for factious independence. It is still difficult to form an opinion whether the absence of domination in Buckinghamshire reflects merely the peculiar configuration of the landed property there or a growing independence on the part of voters generally. Nevertheless, Professor Davis has reopened an important discussion and produced a Bucks to set against Professor Gash's Berks. To what extent Victorian voters were deferential should prove sufficiently controversial to keep a whole generation of historians hard at work.

JOHN CANNON  
University of Bristol

W. R. WARD. *Religion and Society in England, 1790-1850*. New York: Schocken Books. 1973. Pp. ix, 339. \$10.00.

*The Early Correspondence of Jabez Bunting, 1820-1829*. Edited for the Royal Historical Society by W. R. WARD. (Camden Fourth Series, volume 11.) London: the Society. 1972. Pp. 240. £3.00.

Victorian religion has recently attracted considerable attention from sociologists and historians, but scholars of both persuasions have tended to study Victorian religious institutions from the inside. They have written ecclesiastical histories and sociologies of sects rather than studies that probe seriously the complex links between religion and society. The social history of religion in nineteenth-century England has

suffered from the absence of general ideas; surely it is because it inhabits such a sparsely stocked field that the well-known Halévy thesis remains so well known.

In a book bristling with sharply etched ideas about many aspects of early nineteenth-century religion, W. R. Ward, professor of modern history at Durham and current president of the Ecclesiastical History Society, has developed a general argument that has considerable interpretative power. Ward concentrates his attention, and much of his research, on Wesleyan Methodism, especially Methodism in the industrial districts of Lancashire and the West Riding. It is important to bear in mind the Methodist perspective of his work, for it influences profoundly the general framework he constructs for understanding the social history of religion in the first half of the nineteenth century.

Ward argues that at the end of the eighteenth century the vital center of English religion was located in the Evangelical revival, which he considers the religion of the people. The revival was nondenominational and nondoctrinaire, fostering nondenominational institutions and pragmatic theology. The revival was also socially open, appealing to men and women from all levels of society. During the first two decades of the nineteenth century, especially during the troubled years from Luddism to Peterloo, social tension generated pressures that encouraged movement away from the openness of the revival toward more rigidly defined and highly organized denominations. Ward's intricate description of this process at work among the Methodists is perhaps the most impressive portion of this generally impressive book. He shows how economic dislocation created a rift between the radical poor and the rich within the society and how deflation forced the itinerant ministers to become allies of the wealthy and, consciously in some cases, agents of social control.

During the twenties denominational lines continued to harden under the continuing influence of a complex interrelation of social and institutional pressures. By the thirties, the vitality of the revival had been destroyed in the name of denominational discipline. Denominations, moreover, had become aligned with social classes, and the poor, on the whole,

were left out. Too narrowly constructed to contain the energy of the revival or to comprehend major social tension, the denominations, in the age of the Chartists, carried religion away from the mainstream of national life.

In this provocative study of the social sources of denominationalism, written from assumptions strikingly akin to Richard Niebuhr's, Ward has left many questions unanswered. Is it fair to write off the Established Church as a victim of the 1790s? Is not the battle between Church and Dissent in the thirties and forties a sign of vitality, even if the working classes were not much involved? Most important, do denominations actually become closely aligned with social classes? This proposition, central to Ward's argument, is not convincingly demonstrated in this book. It is to be hoped that Ward's work will stimulate more investigation of the problems he has illuminated and that the clearheadedness of his arguments will be an example to those who follow.

A repository that researchers might well make more use of is the Methodist Church Archives, the location of the Bunting correspondence and much else as well. The enormous Bunting correspondence, as Ward points out in his useful introduction, is the major manuscript source for the history of Wesleyan Methodism for the thirty years after 1820. Ward's well-edited selection of the correspondence includes all the letters of importance written between 1820 and 1830. Bunting's son published the important letters for the period before 1820 as part of his father's biography, and Ward hopes in the future to publish a selection from the period of Bunting's greatest influence, the two decades after 1830. Bunting's correspondence is peculiarly interesting because it contains a very large number of "in" letters from preachers scattered over England. It is the authoritative record of Bunting's activities at the center of power, and an important source of information about Methodism in the field as well.

RICHARD J. HELMSTADTER  
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LEONARD G. WILSON. *Charles Lyell. The Years to 1841: The Revolution in Geology*. New Haven: Yale University Press. 1972. Pp. xiii, 553. \$17.50.

Britain produced a remarkable crop of distinguished nineteenth-century geologists, most

of whom became the subject of monumental Victorian biographies. Fascinating though these works are, they hardly satisfy the modern historian, and Professor Wilson's study of Sir Charles Lyell has been awaited eagerly as a step toward a better understanding of the men who did so much to create modern geology. Sadly, the book is a disappointment.

The volume traces Lyell's life down to the eve of his first visit to America, and by availing of a wealth of hitherto unused manuscript material Wilson is able to depict Lyell's activities in considerable detail. Indeed in many places a pruning knife might perhaps have been employed upon the minutiae to good effect. In other places (for example, pp. 96-103 and 173-77) there are overlong digressions possessed of little relation to the main theme, and throughout the volume the informed reader will be repeatedly aggravated by minor errors. The year of James Hutton's death is incorrectly quoted (fig. 8), for example, there are textual contradictions on pages 5 and 11, no British body has ever been styled "the Ordinance Survey" (p. 424), and the Saxon town of Freiberg is invariably misspelled as "Freiburg." A number of important recent works are strangely absent from the bibliography (among them Wilson's own 1969 essay on Lyell's *Principles*), and it is odd that a study which aspires to be definitive should contain no reference to the earlier Lyell biographies by T. G. Bonney and Sir Edward Bailey. Even the book's journal-like comprehensiveness may in part be illusory because at least one important event in Lyell's life has virtually escaped Wilson's notice—his 1818 visit to the scene of the catastrophic debacle in the Val de Bagnes.

Annoying though they are, such defects might be forgiven had Wilson offered a penetrating insight into Lyell's accomplishments, but this he has totally failed to achieve. Repeatedly—but regretfully—I have been forced to conclude that Wilson understands neither the true nature of Lyell's geological beliefs nor the scientific milieu within which he operated. Take, for instance, the origin of river valleys. Wilson praises Lyell for his fluvialistic interpretation of Auvergne in 1828, but he fails to appreciate that Lyell was by no means a convert to the thoroughgoing fluvialism of James Hutton and John Playfair. Although Wilson seems oblivious

to the fact, there were then serious geological objections to the fluvial doctrine, and Lyell, who was only too well aware of the real situation, in 1833 specifically rejected a fluvialistic interpretation of the geomorphology of south-eastern England. Similarly, in complimenting Lyell for his speedy acceptance of the glacial theory in 1840, Wilson forgets that on mature reflection Lyell retreated from his 1840 position and perhaps never wholly returned to its occupation. Time and again Wilson's interpretations are overly simplistic, and his habit on the one hand of lauding those tenets of Lyell's creed that he deems to have been progressive and on the other hand of slurring over those elements that would seem to have been less happy places him in the role of Lyell's eulogist rather than his critical expositor. Most serious of all—and despite the book's subtitle—there is only the most rudimentary attempt to explain the nature of the revolution effected by Lyell's *Principles*. Wilson is content to accept the book's seminal character, and he seems to imagine that his task is satisfactorily executed by tracing the history of the work's authorship and publication and by presenting a conspectus of its contents.

In short, as a picture of the everyday life of a nineteenth-century gentleman of science the book is both interesting and useful; as a study of the intellectual development of the world's most influential geologist it is unsatisfactory and even misleading.

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University of Dublin

PHILIP ZIEGLER. *King William IV*. New York: Harper and Row. 1973. Pp. x, 372. \$12.50.

To describe William IV as the most severely limited of all of George III's sons is indisputably a most generous euphemism. And there were serious flaws in the character and behavior of this loquacious boor to justify the constant scolding and urging that formed the bulk of the letters from his parents when he was at sea or on stations in North America and the West Indies. As the third son, he was far enough removed from the throne to justify his father's decision to send him to sea at the age of fourteen and into a milieu woefully inadequate as preparation for the awful responsibility in his

destiny. It turned out to be a bad decision on another count. The young prince could not properly be entrusted with the command of a frigate; his exalted position, on the other hand, made him hanker for the command of the fleet.

Ziegler has written the first biography of William based on the unpublished materials in the Royal Archives and on Aspinall's monumental edition of the papers of George, Prince of Wales. Despite these sources, there is little that is startling or new in the biographical narrative save some random anecdotes confirming the picture that shows itself in other biographies of the king and his contemporaries. The stay in New York, the liaison with Mrs. Jordan, the display of geniality in public, are retold in a lively style and in a manner that makes the royal *gaffes* amusing and even attractive. In his public appearances the "Coconut Head" of England was remarkably like the "Pear Head" on the other side of the Channel.

The king himself destroyed a good part of his earlier correspondence when it was current, and after his death his secretary, Sir Herbert Taylor, destroyed the bulk of the official correspondence. William IV's private purposes are left to the diarists and letter writers of the period, to Croker, Creevey, and Greville and to the fierce partisanship of the Regency and early reform eras. Ziegler has done a commendable job of separating the wheat from the chaff in the delineation of the king's personality.

In light of William's behavior toward the Grey and Melbourne ministries and his bizarre attempts to maintain Peel's first administration in power, Ziegler's encomium of the king as the "first truly constitutional monarch" of Great Britain seems a bit strong. A good case can be made for pushing the prefiguration of Ziegler's conception of constitutional monarchy back to George III in the nineties. And the Byzantine life style of George IV should not blind us to his constitutional reliance on ministers and to his rejection of the heir-apparent cycle that had spawned his early recalcitrance.

Students of Georgian England will also be puzzled to read in Ziegler's prologue that when George III acceded to the throne in 1760, "he found himself King of a country which economically, socially, politically, had evolved little in the preceding sixty years." It is also as harsh as it is inaccurate to dub Sir Herbert Taylor a

"privileged pen-pusher." Indeed, the first man to hold that most delicate and precarious of all court posts, the private secretaryship, appears to be a candidate for further study and investigation as a result of fresh material supplied us by Aspinall from the Royal Archives at Windsor Castle.

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Hartford

CECIL WOODHAM-SMITH. *Queen Victoria: From Her Birth to the Death of the Prince Consort*. New York: Alfred A. Knopf. 1972. Pp. xii, 486. \$10.00.

Mrs. Woodham-Smith's biography of Queen Victoria, based chiefly on the queen's journal in the archives at Windsor, is a masterly and detailed survey, abundantly documented and vigorous in style, of the period before 1861. Among the new manuscript sources used are the Palmerston Papers formerly at Broadlands; the Conroy Papers at Balliol College, Oxford; the Gibbs Papers (Frederick W. Gibbs was tutor to the Prince of Wales); and the Leiningen Papers of Victoria's half-brother, Prince Charles.

This portrait of the queen is not notably different from that of Lady Longford's *Victoria R. I.* (1964), a book that was also based mainly on Victoria's journal; nor are the estimates of leading politicians—Melbourne, Peel, Russell, Palmerston—much different from those of Lady Longford.

The new biography provides the fullest account we have of Victoria's childhood and early life. Where Lady Longford allotted to the pre-coronation period less than one-seventh of her narrative up to 1861, Mrs. Woodham-Smith devotes a third of her book to it, with special attention to the "Kensington System," which involved the struggle for the control of the young queen waged by her mother and Conroy against Baroness Lehzen. There are, however, no really significant changes in interpretation.

Mrs. Woodham-Smith emphasizes strongly the political and social background of the reign. She dwells at length on the royal dukes, whom Lady Longford treated cursorily. As in Edith Sitwell's *Victoria of England* (1936), there is a full account of the Tolpuddle case, even though

it would appear that the queen may never have heard of the affair. The Spanish marriages, the 1848 revolutions, the Crimean War, and the Indian mutiny are discussed at some length. There is even a brief account of the Australian gold rush of 1851. The results of these digressions is that the queen's own personality does not emerge as sharply as in Lady Longford's portrayal.

Mrs. Woodham-Smith's judgment of Victoria is highly favorable. "Honesty, generosity and loyalty were her good qualities. She never bore malice." These qualities are held to "compensate for her displays of emotionalism," of which her married life furnishes a good many examples. There is scant stress on the obstinacy, peevishness, and self-pity that even at this early stage were present, though their full development occurred in the years of widowhood.

"One of the few valid criticisms of Queen Victoria," writes Mrs. Woodham-Smith, "is her lack of concern for social conditions"—a neglect for which Melbourne is held responsible. Victoria's treatment of her unfortunate lady-in-waiting, Lady Flora Hastings ("that odious Lady Flora . . . such a nasty woman") is termed an exception to "the normally generous impulses of the Queen's nature." (Victoria, in fact, wished to have two of Lady Flora's supporters flogged and the whole Hastings family hanged.)

Although the queen's almost pathological immersion in grief for the death of Prince Albert does not come within the scope of this volume, its morbid intensity is clearly foreshadowed in the extravagance of mourning displayed by Victoria after the death of her mother—whom, living, she had disliked and resented for years. On this, Mrs. Woodham-Smith refrains from comment; nor does she attempt to interpret either Prince Albert's extraordinary lack of a will to live or his extreme revulsion from sex—a revulsion felt so strongly that the news of his eldest son's escapade with Nellie Clifden hastened his death (for which, indeed, Victoria held the Prince of Wales responsible).

One awaits with interest Mrs. Woodham-Smith's interpretation of Victoria's relations with Disraeli and Gladstone in the volume that is to follow.

GIOVANNI COSTIGAN  
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JOHN CLIVE. *Macaulay: The Shaping of the Historian*. New York: Alfred A. Knopf. 1973. Pp. xiv, 499, xxxvi. \$15.00.

In recent years there have appeared a remarkable number of biographies, spacious in scope and excellence, of personages in modern British history; Blake's *Disraeli*, Woodham-Smith's *Queen Victoria*, Brooks's *George III*, and Gash's imposing and magistral life of Peel, are notable among them. The burgeoning is a little surprising. Leon Edel has suggested that biography is a most amorphous literary form, one with no established canons in which the writer is supposed to be omniscient and infallible. The problems of a historian writing biography are especially great. He has so much latitude, so many possible approaches, so many materials, so great a need for varied kinds of expertness, that if he aspires to completeness and balance he runs serious risks of formlessness, superficiality, and amateurism. It is scarcely surprising that many historians, in particular those of the more strictly analytical variety in the United States, have sometimes regarded biography as a marginally legitimate kind of historical writing.

It is an important part of Professor Clive's success that he has written with a definite and coherent purpose and so achieves a definite and coherent form. He is setting out to explain why Macaulay became a historian and why he was the kind of historian he was. His book stops at the moment when Macaulay began the *History of England* in 1839. Professor Clive has done all a biographer could hope to do: he has written a political biography, a psychological biography, a life and times, and a life and letters, but these are subordinated to his theme and serve to illustrate it. He has achieved not only an imposing work of historical scholarship but a work of art.

Macaulay's personality and ambition emerge as a blending of his own great brilliance and marvelous humor with the influence of an evangelical prig and tyrant of a father and his adoration of (and by) his two much younger sisters. The psychology is unobtrusive and certainly undogmatic, and it persuasively links individual and cultural forces. The changing, cheapening, rigidifying nature of the great intellectual influences of the day—evangelicalism, Benthamism, and romanticism—are suggested with imagination and originality through their effect on an exceptional young man irrevocably

but not exclusively shaped by them. And the author succeeds in making his subject understandable as a personality; we feel we know what it would be like to sit next to him at dinner.

There is much that is, from the professional's standpoint, original. Professor Clive elucidates for Macaulay's age the importance of what we have learned from Caroline Robbins and others—that Whiggery had two faces, one the tradition of the Commonwealthmen that led to Fox and the American Revolution, the other that produced the Venetian oligarchy. The Whig interpretation is not so simple as it is sometimes made to sound, and there was nothing simple, either, about the indoctrination and mind of the Tory-turned-Radical-turned-Whig who wrote the *History of England*.

It is interesting, too, to find how reasonable and indeed prophetic Macaulay's views on Jews, Indians, blacks, and women, sound in the second half of the twentieth century—and not less interesting to find how narrow was his view of the lower orders and their proper political role and how practical, indeed cold-blooded, his determination to overcome, in a society still potently oligarchic, the liabilities of a lack of pedigree and fortune. Similarly, there is illumination (at least for those of us who know more about the history of the United Kingdom than of India) of the minute on Indian education, so much discussed and reprobated. Clive makes no excuses for what turned out to be the defects of the educational system that Macaulay, a European and Liberal, if a singularly flexible one, supported; but Professor Clive shows that the decision for Westernization was the result of long-developing controversies in India itself and that it was wanted, indeed demanded, by educated Hindus of Calcutta. Most important of all, he has shown us the really remarkable extent to which Macaulay contributed to what we now understand as "history"—the systematic analysis of society as a whole, the belief that men are least as likely to be shaped by their culture as to shape it—a major part, in short, of the intellectual apparatus of subsequent generations.

Only the captious would seek out defects. There is, perhaps, a problem about the audience for which the book is intended. Some of the background is explained in rather elementary terms, as if the book were intended for the

general public, but in fact it is only readers with a fair degree of expertness who could understand most of it or—it must be said—be sufficiently interested to read it at all, despite the author's elegant style and even more elegant wit. There is perhaps, too, a slight tendency to applaud Macaulay for percipience while apologetically explaining the limitations of his understanding when he was, from the somewhat telic point of view of contemporary judgment, wrong. But no biographer would, or could, undertake so large a study of someone he did not admire, and Professor Clive never fails in his critical detachment. He has not merely written a very important biography, but he has contributed to our understanding of what a biography can and should be.

LAURENCE D. LAFORE  
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STANLEY PIERSON. *Marxism and the Origins of British Socialism: The Struggle for a New Consciousness*. Ithaca: Cornell University Press. 1973. Pp. xiv, 290. \$10.75.

It is often regarded as a historical paradox that Marxism should have made so little progress in Britain, the first major industrial country, where Marx himself lived for so much of his working and writing life. The story has largely been told in the past in terms of the little socialist organizations that came into existence in late nineteenth-century Britain, but A. M. McBriar has written a careful study of the influence of Fabianism up to 1914. There is a need for an equally careful study of the Marxist minority itself and its influence, to establish not only who belonged to it but also what aspects of Marxism they embraced or indeed had the opportunity of embracing—that is to say, what works were translated into English or at least, in view of the average Englishman's inability to cope with German, into French.

Professor Pierson's new work has evidently been in preparation for a good many years, and it is for the most part a careful and accurate work; but it is disappointing to find that it stops short at 1900 and is so largely devoted to surveying ground already covered, without any fresh sources or new insights. Dr. Pierson's method involves the use of a series of short biographies of individual socialists, and although this results in a certain novelty, to be



sure, it is bound to be somewhat confusing to anyone who uses the book as an introduction to British socialism. On the whole the author seems more interested in the Ethical Socialists and the "Religion of Socialism"—including the Labour churches—than in Marxism; but this is only an inference that the reader may make, as there is no critical bibliography and indeed no criticism of sources at all, in spite of the discrepancies that exist among them. The book is also curiously Anglocentric: the sources at Madison, Wisconsin, for instance, have not been used, and we are not told anything about Edward Bellamy's *Looking Backward*, to which William Morris's *News from Nowhere* was consciously written as a reply. Of course the Labour party, founded in 1900, owed relatively little to socialism and hardly anything to Marxism; and if students are to be encouraged, as the blurb suggests, to use this book to "illuminate . . . the origins of the Labour Party," they should perhaps notice that there is a major slip on page 209, where it is suggested that a trade-union fund came into existence in 1893 to finance parliamentary candidatures.

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VINCENT ALAN MCCLELLAND. *English Roman Catholics and Higher Education*. New York: Oxford University Press. 1973. Pp. x, 453. \$22.50.

By the end of Victoria's reign the number of Roman Catholics in England and Wales was still small, comprising under five per cent of the total population; but the growth was nevertheless impressive, for Catholics had doubled in the course of the nineteenth century. At no point and in no manner was it or had it been a homogeneous population, neither occupationally, nor ethnically, nor from the standpoint of social status or income. Not being homogeneous in these ways, it could hardly be so educationally. The Catholic population was also divided according to connections maintained with prominent Protestants or according to the influence sought and enjoyed in Rome. And of course there were religious and political differences. Into this already highly stratified and divided community came yet another group, small but potent and dogged—

the famous converts from the Oxford Movement, some of them fashionable, some well connected, and all of them active and bursting with ideas for the progress, spiritual and material, of the Roman Catholic community in England. In one way or another most of McClelland's study is concerned with the activities and ideas of John Henry Newman (or the converts) and forms an interesting addition to the vast existing literature on that talented and annoying figure.

The Catholic leadership conceded that the provision for higher education substantially lagged behind that of the wealthy Protestant establishment. Oxbridge had revived in two waves, other universities were being founded, Anglican public schools grew prodigiously, and national schemes of elementary education were being launched, with Protestants fighting among themselves for control. The privileged and wealthy Catholic aristocracy wanted greater participation in the attractive social and political life of the governing elite, and the growing Catholic middle classes were interested in professional careers. If the Catholic community was to take advantage of its enfranchisement in the 1820s, make the most of the re-establishment of the hierarchy in the 1850s, and maintain its coherence in the face of the overwhelming influences of industrialism, liberalism, and free-thinking, then traditional insularity and educational backwardness had to be overcome. There were new challenges, opportunities, and dangers.

It is one thing to perceive a need for a better educated population (or desire it), yet another to decide which form of education fits the need best. This is where most of the fascinating educational controversies in higher education took place in the nineteenth century, and so it was with the Catholics. The nature of the education provided depended upon the recipients. As it was the flower of the Catholic territorial aristocracy that was mainly talked about, some form of liberal education was the answer. All Catholics agreed that the education provided should be dogmatic enough to combat religious skepticism. There was also the problem of the charm, confidence, beauty, and cultivation of the finest Anglican institutions. The emigrés from Oxford could never forget the dreaming spires. Consequently it had to be decided whether Catholic youth should be in-

sulated from Oxford's pernicious influences by being educated in purely Catholic foundations or whether they could be trusted to attend Protestant institutions with control exerted through separate communal residences. London University, nonresidential and converted into an examining institution, posed no particular threats.

By the opening of the twentieth century the educational situation had changed remarkably, for England had changed. The Catholic community was no longer dominated by the fashionable old families. Newman was gone. The hierarchy was better known and not so severely distrusted, at least at the upper levels of English society. Economic and numerical expansion had produced a more significant Catholic middle class. English Protestantism no longer seemed quite so formidable, as modern scholarship, science, and the attack on privilege and established institutions reduced many of the dangers of "mixed" education. History, however, continued to defeat the converts. Newman's imperial university had come to nought. And even when the ban on Oxbridge was lifted by the hierarchy the dons rose in loyalty to the memory of Elizabeth and defeated a proposal for a Catholic college in Cambridge.

McClelland's clear and highly useful narrative is a story told from on high, being an account of bishops, thinkers, organizers, their letters to one another, their essays, and their communications to Rome. There is fine material here. But a conventional criticism should be made. The problem as seen from the other end is largely unstated. There are relatively few references to the changing character of the Catholic community in general, the changing occupational and geographical distribution of Catholics, and the pressures exerted directly or indirectly on the hierarchy. Nor are the Irish in England visible. The state and nature of religious belief within the Catholic community at large is passed over. The reader has only a slight idea of what is happening below, and therefore the historical relationship between education, social change, and religious leadership within the Roman Catholic community in England cannot be fully grasped.

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GARETH STEDMAN JONES. *Outcast London: A Study in the Relationship between Classes in Victorian Society*. New York: Oxford University Press. 1971. Pp. xiv, 424. \$15.25.

"Outcasts," "aliens," the "residuum," the "dangerous class," the "undeserving" or "unrespectable" poor—under one name or another, this group was a persistent feature of nineteenth-century England. It troubled contemporaries and it continues to defy historians. We still have not determined the size and boundaries of the group, the relations of subgroups within it, or their relations to the rest of the population. How much mobility was there within this "class" or between it and the "working class" or "respectable poor" outside? In what grey areas of legality or illegality did the "street-folk," as Henry Mayhew called them, operate? We know of seamstresses falling into prostitution, but how irrevocable was that fall, how fateful in their own perceptions of themselves and in the judgments of others? To what extent did this world perpetuate itself from one generation to another?

The questions multiply, and this book is a serious attempt to come to an understanding of some of them. But only some of them; some are precluded from being asked by the framework of analysis adopted here. For the "Outcast London" of the title, which inspires these and similar questions, is used by the author ironically, to express what he takes to be the "mythology" of Victorian England, a mythology designed to conceal the true social "reality." That reality, as he sees it, was nothing more or less than unemployment: structural, cyclical, and seasonal unemployment that gave rise to "endemic forms of poverty" for a considerable class of "casual labor." In addressing the problem of casual labor in these terms, the author has amassed a considerable and valuable quantity of information about the nature and extent of unemployment, the living conditions (especially housing) of those subject to one or another variety of unemployment or underemployment, and the effects upon them of various attempts, private and public, to cope with and alleviate those conditions.

One concludes this book grateful to Mr. Jones for the lively intelligence and the commendable industry he has brought to his subject. But one may also have some reservations about a conception of social reality that sees the

problem entirely in terms of casual labor and unemployment. This is not the familiar complaint of the reviewer who wishes the author had written a different book. It is an objection that goes to the heart of the book, and perhaps also too much recent writing of history. For the author's presumption, like that of many historians, is that he can see the problem more clearly and objectively than contemporaries could. They were caught up in the "mythology" of moral judgments; he can expose the "objective" economic reality. They saw things through the "distorting lens of middle-class aspirations to gentility," through the "deformations" of the liberal "ideology"; he is free of all such distortions and deformations.

The difficulty, of course, is that Jones is dependent upon those "mythologizing" contemporaries for much of his evidence. He must, therefore, use that evidence selectively, picking and choosing those bits that support his thesis. In the present case, this means praising Mayhew, for example, for seeing "clearly" what others presumably failed to see: the importance of seasonality in the London economy, the connection between seasonal labor and low wages. But if Mayhew saw so clearly on these subjects as to warrant extensive quotation, why does Jones never quote him on other subjects that preoccupied Mayhew far more: the peculiar physique and mentality of the "nomadic race" of street-folk, a race that had a positive "repugnance to regular and continuous labor," an "inability to perceive consequences," a "passion for stupefying herbs and roots," an "immoderate love of gaming," an "absence of chastity," and more, very much more, in the same vein, all illustrated and documented at great length in the four volumes of *London Labour and the London Poor*.

Another thesis of this book is that the fears about the "outcast" class grew as the century progressed, reaching their height in the 1880s. Here one is caught between admiration for the author's subtlety in interpreting the evidence and wariness of that subtlety. The dock strike of 1889, for example, might have been expected to bring those fears to a climax. But since it was perfectly evident that contemporaries did not, in fact, panic in the face of that strike—indeed, that much of "middle-class London" was enthusiastically in favor of the strikers—

Jones ingeniously concludes that the strike came as a "cathartic release from the social tension of the mid-1880s." Perhaps so. Or perhaps not. Perhaps the tension was never as severe as Jones made it out to be. Perhaps we should take more seriously, even respectfully, the work of philanthropists like Octavia Hill and the Charity Organization Society, or the legislation of the seventies and eighties, in alleviating that tension. Moreover, in a work where chronology is so central to the thesis, one is made uneasy by the casualness with which Jones lumps together or quotes indiscriminately from Mayhew (most of whose work dated from 1849–52) and from Charles Booth (whose data was collected almost forty years later).

These demurrals testify to the provocative-ness and importance of *Outcast London*. It is a book all social historians will use, and some will quarrel with, for a long time.

GERTRUDE HIMMELFARB

City University of New York

BRIAN BOND. *The Victorian Army and the Staff College, 1854–1914*. London: Eyre Methuen. 1972. Pp. xv, 350. £5.50.

Generals represent the elite of the military profession. How Britain selected this elite from the officers who attended the Staff College at Camberley in the late Victorian era and how these professionals became the commanders of the army by World War I are the subjects of this thoughtful work.

A new sense of professionalism pervaded the military world by the 1850s, due to changes in warfare during the French Revolution, the elevation of standards for prestigious civilian occupations, and the screening of candidates for commission. Warfare was now increasingly scientific. Technological developments effecting the movement of troops made it apparent to reforming war ministers that the Wellingtonian army had to be modernized. The day of the blundering amateur-at-war was over at Balaclava.

After the Crimean War, the curriculum at Camberley was improved to make the courses in tactics, logistics, and strategy more realistic. Yet graduates of the college lacked prestige, and the commander-in-chief, the duke of Cambridge, was critical of educational innovations. After

the Boer War, however, the army's new breed of intellectuals won some victories. The reactionary Cambridge had retired in 1894; the master of the small colonial war, General Sir Garnet Wolseley, had demonstrated the value of staff specialists; and competition for appointments to the college became keener. Furthermore, the South African war revealed serious defects in the organization of the army, deficiencies that might have been corrected by a general staff, which was not established until 1906. Due to the entente with France and the growing animosity of Germany, the college, now the nursery for the general staff, had a definite purpose as its students pondered the possible deployment of the British Expeditionary Force to the Continent. Staff College training, in fact, became virtually obligatory for coveted appointments on the eve of World War I.

The test of these experts was on the French and Belgian battlefields in 1914. Although Bond notes the difficulty of assessing the college's influence during the early campaigns on the western front, he admits that the disastrous consequences of ill-coordinated offensives in 1915 demonstrated the inability of British generals to adapt their tactical doctrine to static trench warfare.

This is a useful work. Yet the excessive use of ponderous quotations occasionally mars the narrative; the possible influence of other national staff colleges is slighted; and the British army is described as seemingly unaffected by the gradual transformation of the contemporary social structure. And surely the inclusion of a collective biography is necessary to categorize this elite. However, the author has presented his thesis in an illuminating manner, and he has made a definitive contribution in clarifying the influence of the Staff College on military thought.

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College at Brockport

JOSÉ HARRIS. *Unemployment and Politics: A Study in English Social Policy, 1886-1914*. New York: Oxford University Press. 1972. Pp. x, 411. \$24.00.

This book bears all the marks of a revised doctoral dissertation. It is earnest, contentious,

heavily footnoted, and slightly tedious. But nevertheless it also tells a great deal about British efforts to grapple with the problem of industrial unemployment in the three decades before the First World War. Perhaps the chief fault is not the narrowness of the treatment, which is after all characteristic of works of this kind, but that even within the close limits which the author set for herself, she attempts to do too much. There are endless extracts from easily available documents. The work of the Webbs on the Minority Report of the Royal Commission on the Poor Law is assessed yet another time. The varied failures and successes of the Unemployed Workmen Act of 1905 are detailed and cataloged. In all of this the spectacular political daring that was the experiment with unemployment insurance is almost lost. No one ever says that Churchill and Llewellyn Smith were in fact taking a very large political chance in compulsory, treasury-supported—for that in fact is what it was—out-of-work compensation paid as of right. It was not an idea plucked from the flood of writing about what to do with the destitute poor left over from the Poor Law, nor was it a response to the Labour party's perennial Right-to-Work Bill. It came, as does most important legislation, from hard politics and personal ambition. It bore about the same relation to the generation of ideology that had preceded it as does NASA to good science fiction writing. Mrs. Harris knows this and says so on page 362, at the end of her summary, by pointing out that there was "little conscious reference to theory in Liberal reforms." In doing so she makes the first half of the book, which is about theory, irrelevant.

This is still a book that was worth writing. One could wish that the author, hard-working and serious, would remember that the New Liberalism has been worked and reworked in the last few years and that a new footnote does not necessarily change an old fact. Everyone agrees that Churchill and Lloyd George were not supermen, that much that they accomplished had been proposed by others, and that the welfare state did not come into being with the National Insurance Act of 1911. But the work of these men did lay the foundations for the present institutions of British social security, and if historians will keep their sense of detachment, perspective, and above all their sense

of humor, they will find in the New Liberalism a political philosophy that is at once coherent and very English.

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RICHARD PRICE. *An Imperial War and the British Working Class: Working-Class Attitudes and Reactions to the Boer War, 1899-1902.* (Studies in Social History.) Toronto: University of Toronto Press. 1972. Pp. xiii, 279. \$11.50.

Here is an attempt to show that the idea of working-class enthusiasm for the Boer War of 1899-1902 was no more than a myth. Myth it might have been, but that is not really proved by the evidence produced by Dr. Price, who has taken as his principal samples the membership of workingmen's clubs with a strong background of radicalism. Of course, it is extremely useful to have the middle-class leadership of mob jingoism amply demonstrated and to be steered firmly away from the realms of untruth. It is, nevertheless, only a partial service to take away "2x" of falsehood with one hand, only to replace "x" of it thereafter. Some emphasis on the metropolis was certainly justified. London was the scene of the most jingoistic outbursts. Even so, the provinces should have been taken much more into account.

By the end of the nineteenth century the Liberals were supported by the majority of British workingmen, but the Conservative minority remained substantial. That minority would certainly have tended toward jingoism, albeit mainly led by its social superiors. Then, too, there certainly were large pockets of Liberal Imperialists dotted all over working-class areas. These, too, would be for the Boer War. Dr. Price's initial analysis was faulty, if in fact he ever made one. One suspects he did not, because the book is essentially episodic. Nothing in it is uninteresting or badly written. On the contrary, much of it is actually fascinating. At the same time it is often irrelevant, assuming the theme to be the one set out about working-class attitudes. The tables presented for comparison are not always suitably chosen.

The actual course of the failure of radicalism to combat the war policy and its propaganda, the accounts of the origins of the working-class

clubs, and the course of the 1900 general election and the makeup of the jingo crowd, not to mention the details as to recruitment, all make worthwhile reading. While the theme of the book is unsustained, its details frequently illuminate. While there are some mistakes in details, the general picture is one of commendable accuracy.

MICHAEL HURST  
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BERNARD GAINER. *The Alien Invasion: The Origins of the Aliens Act of 1905.* New York: Crane, Russak and Company. 1972. Pp. 305. \$12.75.

The recent furor over legislation to regulate Commonwealth immigration to England has generated two studies of the British response to Russian Jewish immigration at the end of the nineteenth century. John A. Garrard's *The English and Immigration 1880-1910* (1971) contends that Liberals and Socialists adhered to traditional ideals and opposed restriction, despite the vocal opposition of a portion of their supporters; with greater consistency than the Labour party did in the 1960s. Covering much the same ground, Gainer, in a more incisive but less satisfactory work, focuses on the agitation that produced the Aliens Act of 1905, a measure that sought to restrict entry into the United Kingdom of those deemed socially undesirable.

Antialien sentiment derived from a misconception, fostered by inaccurate and occasionally distorted statistics, that England was being subjected to an invasion by a horde of unskilled social misfits who displaced English workers and congregated in overcrowded East End London slums. In fact, as *The Alien Invasion* reveals, the number of immigrants was relatively small—roughly 80,000 from Eastern Europe by 1901. While it is true that the Jewish refugees gravitated toward the sweated workshop trades, they aggravated an already existing problem, identified decades earlier by Mayhew and Kingsley, and did not create a new one. It was increasing provincial and foreign competition, as well as mechanization, that led to unemployment in cheap clothing manufacture and the lack of state supervision that permitted the sweating system to flourish.

Gainer explores the varied sources of anti-alienism—working class insecurity, latent anti-Semitism, protectionism, and anxiety about ebbing national efficiency—that could be tapped by proponents of restriction, like Arnold White and Major William Evans-Gordon, but he fails to explain adequately its translation into political action. By his own estimation the antialien phenomenon remained geographically limited to London, where organizations like the British Brothers League, which advocated regulation as a panacea for economic ills, never mobilized the dedicated following typical of Victorian pressure groups. He suggests that the Conservative party endorsed restriction as an expedient palliative for working-class grievances. It was able to court working-class voters by prescribing a remedy for unemployment that would not impinge upon the economic interests of its wealthier supporters. But there is little evidence that the alien issue was electorally significant even in London. Tory strength in working-class constituencies was greatest in 1895, when party leaders were indifferent to the immigration question, whereas six East London seats were lost in 1905 immediately after the passage of the Aliens Act. Restriction gained the party little support in the provinces, and the coupling of antialienism with protection probably damaged its electoral prospects in London.

Although carefully researched and abundantly documented, Gainer's work suffers from an excessively narrow focus. The author says little about the response of the Jewish community to the new arrivals and tends to underestimate the British commitment to the right of asylum and to unrestricted entry. Too much of the book is devoted to a recapitulation of parliamentary debates and Royal Commission testimony, and its topical organization lends itself to a good deal of repetition.

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HAROLD MACMILLAN. *Pointing the Way, 1959-1961*. New York: Harper and Row. 1972. Pp. viii, 504. \$15.00.

This is the fifth volume of Harold Macmillan's memoirs. Inevitably, comparisons suggest themselves with what Winston Churchill wrote about his own stewardship of Britain's affairs. There

are some obvious similarities between what Macmillan writes and what his more illustrious predecessor chose to say. Each is preoccupied with the "special relationship" with the United States; both continue to concern themselves with what was once called the Empire and had become the Commonwealth; the state of the alliance is very important to them. Such similarities, however, are superficial when compared with the differences between the two memoirs. Churchill lived in heroic and dangerous times; he wrote of great events. The events Macmillan describes are great only in his own memory. Much less depended on what he did (or said) than he realized. The events of 1959-61 seem curiously more remote to the world of 1973 than do those of the years 1939-45. Macmillan constantly worried and fretted about the "crises" of his time; he alludes to his anxiety much more frequently than Churchill ever did. While it would be ungenerous to say, with the advantage of historical hindsight, that he ought not to have worried so much, there is a lack of self-awareness here that is genuinely disconcerting. The ball was not as frequently in Macmillan's court as he imagined it to be. In 1960 Macmillan produced a document that he half-jokingly referred to as "The Grand Design." That document seems more positively old-fashioned, parochial, and out of date than almost anything that Roosevelt and Churchill ever fashioned.

These statements should not be taken to imply any major dissatisfaction with the book. They simply emphasize how quickly the events of the late Eisenhower and early Kennedy years have receded into history. Macmillan, in this volume, as in the others he has written, shows himself a master of English prose and a deft portrayer of political personalities. Despite his genuine liking for Eisenhower, he cannot conceal the dismay he felt at the ineptness shown by the president in his handling of the U-2 episode, which contributed to the failure of the Paris summit meeting of 1960. Eisenhower, in Macmillan's eyes, had sterling qualities as a man; this did not make him a successful president, particularly in his ill-starred second term. For de Gaulle there is grudging admiration and a great desire to emphasize the excellent rapport that developed between the two. In many of de Gaulle's remarks, reproduced by Macmillan,

there is an obvious tribute intended—both to de Gaulle's insight and to his intelligence; the decisiveness of the man clearly appealed to Macmillan. On Adenauer he is much more circumspect; the fundamental distance between the two is never concealed or denied. As for Khrushchev, Macmillan represents him as a ham actor; truculent, boastful, vain, and not always very consistent. Some of his most interesting remarks he reserves for John Kennedy. Having not known him before he entered the White House, he emphasizes the rapidity with which he established a close and confidential relationship with him. He is pleased that Kennedy could be an "intellectual" as well as a political companion. Kennedy is generally portrayed in two poses: the inveterate questioner and the callow youth soliciting advice from the man of experience. Kennedy, overwhelmed and shocked by his meeting with Khrushchev in Vienna, is represented as wanting to meet with Macmillan alone so that he can learn from him. Many questions suggest themselves. Would Macmillan have written in this way if Kennedy had lived? Was the relationship quite as one-sided as he makes it appear? Was there no guile in Kennedy? Finally, why is it easier for Macmillan to find chinks in Kennedy's armor than in that of any of his British colleagues? His comments on his own associates make them all appear to be wise, generous, and farseeing.

It is curious that one's interest in the book is almost wholly with Macmillan's depiction of individuals. The political issues appear much less compelling. Rarely does Macmillan contribute very much to our knowledge about individual events. There is relatively little analysis of the nature of the rifts that existed within the alliance, and the evidence on Allied relations with the Communist world is superficial. The book is entertaining; in places it is candid; it is the work of a civilized man. But it all seems to have happened so many, many eons ago.

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DAVID MCKIE and CHRIS COOK, editors. *The Decade of Disillusion: British Politics in the Sixties*. [New York:] St. Martin's Press. 1972. Pp. 250. \$9.95.

The sixties are still much too close to us to handle without some unifying and simplifying idea, and perhaps for Britain "disillusion" will

do as well as any other. However, the authors of these essays have not worked out what they mean by the word. Contrast Leslie Stone's neat and competent account of the British withdrawal from the illusions of empire and world power, which still flourished in the late fifties, with Peter Sinclair's gloomy demonstration that each of the economic policies tried in the sixties led to failure, and it emerges that these two creditable essays differ on whether the British had successfully got rid of their illusions or had an inexhaustible supply still to come. The retreat from empire and world power is, of course, too much for a fifteen-page essay, and Stone's handling of the topic makes one hope for a book. Almost the only other person with the elements of a book in his essay is David McKie, who writes about "The Quality of Life" in a way that shows he cares about both technology and the environment (although *The Limits of Growth* came a little later, he could probably write about it sensibly), and he also concisely fits in the relaxation of so many over-moralistic laws under Labour.

There is an informative essay—perhaps too solid to wish it longer—about immigration by Roy Hattersley. A curious essay on "Social Welfare and Housing" certainly makes one wish for more about housing, because it gives only two pages to a subject of more or less universal interest, but Anne Lapping rather clearly feels that the social services are a fragile plant that would suffer if discussed in terms of disillusion. Education might have seemed quite a good subject for disillusion, but Brian MacArthur has the same sort of feeling that education ought to be defended. I hope his dreams come true, but he is not writing within the framework of the title.

Victor Keegan's study of industry and technology has enough disillusion for anyone; nihilistic pessimism is perhaps the word for Harold Jackson's suggestion that what the British really want to do about Ulster is to leave and let them fight like the Kilkenny cats. Hugo Young has written an essay about the various pressure groups for good causes that flourished in the sixties, but it is confused by his conviction that nobody less respectable than the Confederation of British Industry ought to put pressure on the government. As a result he writes about organizations like the Campaign for Nuclear Disarmament in terms so

apocalyptic that they would make sense only if CND had wanted to become the government. Chris Cook is much more reasonable about minor parties, though it seems rash of him to write of Liberal by-election successes as the result of disillusion and then discuss their 1970 general election losses as though the British would never again be disillusioned with their politicians.

This may all seem rather a muddle, but it might be cured by a strong central essay. Unfortunately the main essay (John Barnes on "The Record") is also the worst. Apart from the bad grammar and the shaky economics, it is written like a newspaper story. Faced with a deadline, obliged to write without knowing how the story ends, a newspaperman cannot reflect or analyze much—if only because next morning may show he was wrong. With less cause, Barnes writes from one headline to the next, and instead of analyzing, he prints the figures of the opinion polls. The events of the sixties are mentioned, but they are submerged by the frenetically allusive style, so that only someone who knows what happened in Britain in the sixties will be able to follow the essay. And someone who knows will not need to read Barnes.

TREVOR LLOYD  
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W. A. MAGUIRE. *The Downshire Estates in Ireland, 1801-1845: The Management of Irish Landed Estates in the Early Nineteenth Century*. New York: Oxford University Press. 1972. Pp. vi, 284. \$19.25.

With the appearance of Dr. Maguire's excellent book, historians need no longer lament the absence of useful works on Irish agricultural history. He has thrown much light on this hitherto obscure subject by a study mainly confined to the resources and management of the vast Irish property (some sixty thousand acres) of the third marquis of Downshire. To limit oneself to a single estate may prompt critical murmurings in sophisticated circles about statistical significance. But Dr. Maguire has taken care to examine an estate that not only was very large but also comprised several properties both in the north and south of Ireland, thus encompassing important regional differences, and, what is more, one that has left an extraordinarily rich archive of estate papers. He has,

moreover, put these manuscripts to very good use. He has read them in the light of what he has learned from less formidable manuscript collections of eight or nine other estates, from a large contemporary literature, notably the Devon Commission Report, and from a careful study of the work of English estate historians (none of whom, curiously enough, has ever written a large-scale study of a single estate). The happy results are that he settles down on the right topics, asks the right questions, and thereby opens up enough fascinating lines of thought to inspire doctoral candidates for a long time.

Perhaps his most startling conclusion is the following: "The striking fact that emerges from a study of estate records is the importance of the agricultural crisis at the end of the war with France. It is of course true that the Famine caused, or at least brought to a head, a financial crisis for many landlords, but the years 1815-20 rather than 1845-50 mark the real watershed between the land situation as it was in the eighteenth century and the crisis of the late nineteenth century" (p. 250). The eighteenth century had been marked by the prevalence of the long lease that in Ireland, as Dr. Maguire contends, helped to encourage the subdivision of tenant holdings and the spread of retrograde farming practices. The post-1815 fall in agricultural prices nurtured the beginning of landowners' resistance to these trends, in which the marquis of Downshire took a leading part. Impelled largely, it would seem, by an un-Keynesian abhorrence of debt, this otherwise unremarkable nobleman built a system of estate management quite as businesslike as the most businesslike of great English estates. In analyzing the marquis's achievement, Dr. Maguire has provided a model of what needs to be done not only for Irish but for English estates as well, wherever the documentation is so abundantly available.

DAVID SPRING  
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W. B. STANFORD and R. B. MCDOWELL. *Mahaffy: A Biography of an Anglo-Irishman*. Boston: Routledge and Kegan Paul. 1971. Pp. xiii, 281. \$10.00.

John Pentland Mahaffy was one of the more interesting Irishmen of the past century. His



biographers describe him—without exaggeration—as “historian and philosopher, man of letters and musician, conversationalist and controversialist, sportsman, publicist, diner-out and don.” Born in 1839 and an authority on Hellenistic Greece, he was a scholar of Trinity College, Dublin, and its professor of ancient history, and he ended his life in 1919 as its provost.

This biography deals expertly with his life as a scholar, describing his love of Greece; his published works, which did much to perpetuate public interest in the classical world; and his controversies with other scholars in Trinity and in Oxford and Cambridge. He was arrogant and inaccurate as a scholar and was never accepted as a great authority; he was a snob in social life as well as in academia, but his honesty and courage atoned for much.

This book is very well written and is always entertaining. It seems much more the work of Stanford—a professor of Greek—than of McDowell, a historian. The historical background is scarcely dealt with. Mahaffy might as well have been a don in Oxbridge or Edinburgh for all that is told of his relations with the Irish population. His attacks on the Gaelic Revival and Home Rule are only lightly etched in. He did much harm to his college by his arrogance in this context. The Catholic population was only emerging from second-class citizenship in the nineteenth century and suffered from an inferiority complex in regard to the Protestant Ascendancy. It felt Mahaffy's disdain keenly and, naturally enough, blamed it on his college. In 1921 this new de facto Catholic democracy took over. Thanks largely to Mahaffy, Trinity found itself isolated and only gradually found its feet in the new Ireland. Its survival was due as much to the religious tolerance of the new rulers as to its own efforts. Even though he did his college harm, Mahaffy must be acknowledged as a distinguished scholar and Irishman.

MAURICE R. O'CONNELL  
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ROBERT M. ISHERWOOD. *Music in the Service of the King: France in the Seventeenth Century*. Ithaca: Cornell University Press. 1973. Pp. xiv, 422. \$17.50.

Musical topics are dealt with all too rarely by real historians. Musicologists often lack the

kind of professional training in historical method so essential for perceptive research in matters political, sociological, and esthetic, with the result that much writing about music restricts itself to the purely analytic. On the other hand, a historian with a background in practical music can offer an abundance of information seen from a new angle, as Woodfill did in his admirable study of musicians in the time of the Tudors and Stuarts. Professor Isherwood, also a practical musician, provides a comparable service for those fascinated by the extraordinary state of musical affairs in seventeenth-century France.

Although he begins with a useful but necessarily brief account of musical philosophy and continues with a lively summary of court music and masquerade in the Renaissance, the author devotes by far the greater part of his book to the operas, ballets, and divertissements written by Lully for the amusement of Louis XIV and his scintillating entourage. The relationship between a musician of humble origin and a king who shone as the sun in the heavens was a strange one, and it has often been discussed in works that vary from the scholarly to the scandalous. Here we see things in perspective. Lully is portrayed as a man anxious for personal advancement, which he obtained successfully by doing precisely what was expected of him. Artistically involved in serving an absolutist monarch, Lully was absolutely ruthless in all his dealings. His creations, in which he took an active part as composer, dancer, and director, were in some ways similar to present-day musicals, for the accent was on spectacle, costume, choreography, and melody. And just as the modern composer of musicals frequently works as the chief of a team, so Lully sketched his scores and then handed them over to secretaries, who filled in harmonies and added details in orchestration.

Lully may have been the dominant musical figure of his day and age, but he was far from being the finest musician. Charpentier's effortless superiority as a craftsman and as a contrapuntist drew from the king a veritable paean of praise when the opera *Médée* was first performed. Its prologue, of course, contained the accustomed element of flattery, but Charpentier could claim that he was serving the monarch in the expected manner, just as he wrote special

music for the dauphin and for other eminent members of the court. Although Professor Isherwood's flowing style and solid grasp of sources command our attention, many readers would gladly exchange some of the complicated opera plots for a closer look at other figures such as Charpentier, Dumont, and Robert. The Abbé Robert, for example, may be almost a biographical cipher, but his published volumes of motets show him to be a French Gabrieli in his skillful manipulation of orchestra, soloists, and choruses. In many ways the music of the royal chapel also deserves our respect and study, and the account of it in the chapter on ceremony and celebration could go deeper. Perhaps the author will follow up this excellent foundation study with others dealing with Lully's contemporaries.

DENIS STEVENS  
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LIONEL GOSSMAN. *French Society and Culture: Background for 18th Century Literature*. Englewood Cliffs, N.J.: Prentice-Hall. 1972. Pp. viii, 149. \$6.95.

This book is briefer and more condensed than John Lough's *An Introduction to Eighteenth-Century France* (1960)—probably the standard introduction to eighteenth-century French society for English-speaking students of its literature—but also more sophisticated in its conception and ambitious in its scope. Lough aimed simply to provide students of literature with a background knowledge of society, and he did this very well. Gossman invites them (albeit very tentatively) "to relate the cultural forms by which a society seeks to attain consciousness of itself and of the world to the organization by which it secures subsistence and continued existence" (p. vii). The result is a challenging piece of work, introducing students to intellectual approaches and concerns that have come to the fore in the past decade.

Professor Gossman begins with an excellent brief sketch of social classes and conditions which serves to introduce the thesis that "the Enlightenment presented itself in universal terms, but the concrete reality in which it was embedded was the struggle of one minority social group to wrest power from another minority social group" (p. 38). The exact nature

of this struggle between the bourgeoisie and the privileged orders is not really elaborated either in this first chapter or in the rather scanty second chapter on social change and attempts at reform. Instead, Gossman goes on to summarize Labrousse's thesis concerning the crisis of the French economy in the 1770s. This summary suggests that social tensions may have increased in the last decades before the Revolution, but it says little about the nature of the struggle that Gossman postulates as the concrete background to the emergence of enlightened ideas (that is, before 1770). Furthermore, while the account of attempts at fiscal reform is brief but informative, the discussion of economic reforms is too rudimentary to sustain the argument that the economic prosperity of the mid-century led to "the progressive withdrawal of the state from the economic sphere and the collapse or at least the weakening of established political and economic institutions and values" (p. 57). This withdrawal (sporadic, perhaps, rather than progressive) probably had as much to do with relatively long-term developments in the political institutions and values of the monarchical state as it did with immediate economic conditions. The omission of any discussion of such political developments weakens this section of the book.

In the remaining two chapters, however, Professor Gossman comes into his own. The third chapter—complicated, uneven, far too compressed to be readily grasped in this form—is, in essence, a sketch for a brilliantly rich book he must surely go on to write, analyzing the strains and tensions of the French Enlightenment as a bourgeois world view, in categories suggested by Franz Borkenau's *Der Übergang vom feudalen zum bürgerlichen Weltbild* (1934). Many characterizations of the Enlightenment as a "bourgeois" phenomenon seem to remain rather sterile: they label the phenomenon in a way that tells us relatively little about it. Gossman's analysis, on the contrary, suggests some real insights into the ideas of the philosophes. His general characterization can and will be debated. But his discussion—involving such themes as the reception of Lockean ideas in France, the strains in materialism, and the "pessimistic" sources of the radical critique of society that threatened enlightened views—remains very suggestive.

Finally, Gossman concludes with an analysis of some recent approaches to the sociology of literature and a tentative exploration of the sociology of the literary form in eighteenth-century France. For historians who probably know relatively little of the work of literary scholars in this field, this will be one of the most interesting chapters of a stimulating little book.

KEITH M. BAKER  
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V. G. REVUNENKOV. *Parishskie sankiuloty epokhi velikoi frantsuzskoi revoliutsii* [Parisian Sans-Culottes at the Time of the Great French Revolution]. (Leningradskii Ordena Lenina i Ordena Trudovogo Krasnogo Znameni Gosudarstvennyi Universitet imeni A. A. Zhdanova.) [Leningrad:] Izdatel'stvo Leningradskogo Universiteta. 1971. Pp. 174.

This small volume by Professor Revunenko of Leningrad State University, a senior Soviet specialist on modern French history and on the history of revolutions in modern times, was designed for students, graduate students, and professors at all levels of Soviet higher education. It relies heavily for its factual basis on the works of Saboul and Rudé and on Soviet editions in Russian of the collected works of Marat, Danton, Robespierre, and of translations of the works of Lefebvre, Mathiez, and Thorez. So far as one can tell, Professor Revunenko has not conducted research in France or visited that country.

*Parishskie sankiuloty* reflects the growing Soviet interest in French history, which began to burgeon about fifteen years ago, a decade or so before a similar interest in American history appeared. This book illustrates the application of pious Marxist-Leninist formulas to historical developments. Revunenko argues that the revolutionary bourgeoisie and the national masses formed a block, but that the Jacobin bourgeois dictatorship then destroyed the power of the sans-culottes, liquidated the national elements that had led the August 10, 1792, uprising, and, as an instrument of the bourgeoisie, defended that class from the popular masses.

Revunenko cites Marx and Engels to support his view that 1793 represented the dictatorship of Paris and the bourgeoisie over France. He notes that Lenin identified the death of the

"first revolution" with the Jacobin dictatorship and that this was natural and indeed inevitable, since the Jacobins established a dictatorship of the bourgeoisie in the conditions of a bourgeois revolution. On the other hand, the Bolsheviks, according to Lenin and Professor Revunenko, united dictatorship with democracy and waged a civil war while providing for wide participation of the masses in state affairs. They provided Russia with its first Soviet constitution, which strengthened the dictatorship of the proletariat, denied the "exploiters" the right to elect or be elected, and simultaneously established a "democracy." In short, the enemies of socialism were not allowed to use "Soviet democracy" for their interests, while the rights of all, or at least of the workers, were protected and even expanded. The Russian Revolution was therefore more democratic than the French Revolution.

An American finds it difficult to distinguish between these dictatorships. Revunenko is able to condemn the Jacobins and praise the Bolsheviks because to him the goals of the two dictatorships are so different, the ends justify the means, and historians serve political causes.

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DAVID H. PINKNEY. *The French Revolution of 1830*. [Princeton:] Princeton University Press. 1972. Pp. ix, 397. Cloth \$16.00, paper \$8.50.

Fifteen years ago Professor Pinkney formulated what deserves to be known as Pinkney's Law: the more specialized their research on France the less American historians contribute. Instead of competing on unequal terms with their French colleagues, American scholars, Pinkney maintained, should offer broad interpretations of the French past to American readers. Pinkney's Law, greeted with approving nods, did not deter the noddors (myself included) or their graduate students from continuing to grind out monographic studies. Rational assent was neutralized by the deeply felt conviction that the law applied to the other fellow only. As to budding young scholars, they remained governed by the iron law of dissertation topics. Yet I am convinced that Professor Pinkney was right, and with the courage of his convictions he has now published

this book as a sort of exemplar. Consequently it deserves to be considered on its own merits and as a model to emulate or shun.

The July revolution has been relatively neglected by historians not only in the United States but in France as well. Professor Pinkney, drawing on a considerable memoir and monographic literature—as well as on his own substantial archival research—has produced the only up-to-date scholarly book on the political crisis that toppled the last Bourbon king in France. It is thorough, interestingly written, and well organized—first-rate in every way. Though a narrative of political events from 1829 through 1830 is central, at least three chapters, “Sources of Opposition,” “The Crowd in the Revolution,” and “Purge and Replacement,” stress analysis.

Aside from its greater depth and complexity Pinkney's synthesis does not greatly alter the conventionally accepted picture. When it comes to Charles X and his ministers their bumbling incompetence appears to defy any revisionism. The liberal opposition, never militant in traditional accounts, seems even more timid and passive. Indeed, Pinkney is most convincing in depicting the revolutionaries' artisan background and their autonomy from the parliamentary liberals. He makes short work of the myth—well entrenched by 1848—of a republican revolution confiscated by monarchists. He presents evidence for a strong undercurrent of popular Bonapartism, a much weaker republicanism, and no indication that either faction was seriously organized. The Orleanists won out because of able leadership, skillful propaganda, and Louis Philippe's availability.

Among the more original aspects of the book is the author's survey of the provinces and his assessment of the class character of the revolution of 1830. The provinces, it seems, were restive enough to welcome the overthrow of the Bourbons. As to the problem of the “bourgeois revolution,” the vast administrative purge in the wake of Charles X's fall brought government jobs to thousands (most of them former Napoleonic officeholders) who were otherwise socially indistinguishable from the officials whom they displaced. Nowhere was there an influx of businessmen into positions of power.

I do have a few quibbles. Why list Louis Chevalier's “dangerous classes” among the

sources of opposition when Pinkney himself goes on to demonstrate that the revolutionary crowd (echoes of Rudé) comprised respectable artisans? Or why mention rural grain riots at all when they do not affect the course of events in 1830? And does not the account of replacing paving stones belong to gentle anti-quarianism? But these are not serious criticisms.

In shifting to a consideration of this work as a model of synthesis for other American historians to follow I am troubled by what I perceive as lost opportunities. Pinkney's synthesis strikes me as unduly restrictive. If Pinkney's Law is to be seriously applied—and I think it should be—the context of historical problems should be as broad as possible and questions of comparison and significance raised without false modesty.

At least four such questions come to mind. First, while Pinkney treats the revolution of 1830 as a unique event he fails to consider it as a phenomenon, as belonging to a class of events. Just in the last fifteen years, modern revolution has been analyzed in numerous theoretical studies of varying quality, which, as Lawrence Stone has pointed out, we ignore at our peril. Second, granted that the administrative purges of 1830 failed to invest the bourgeoisie with state power, should our investigation stop there? I worry over Louis Philippe, an aristocrat to his finger tips, suddenly affecting a bourgeois life-style after 1830. Why did he if nothing had changed? Is it just possible that the social climate did alter even though the social and professional background of politicians and officials did not? Third, I remain perplexed by the constitutional issue that triggered the overthrow of Charles X. Why was it that this constitutional impasse over ministerial responsibility never recurred during the eighteen years of the July Monarchy? The question is central in assigning to the July revolution its proper place in French constitutional history. And though it unfairly transcends the time span of Pinkney's book the question will trouble our “American readers.” Fourth, I am puzzled by the birth, or rebirth, of popular republicanism in Paris following the revolution of 1830. If Parisian republicanism was so insignificant in July 1830 and the insurgents animated chiefly by hatred of the Bourbons, how do we account for the revolutionary republican challenge that

developed between 1831 and 1834? Did this republicanism grow out of the experience of 1830? If so, how and why? The question will not be dismissed by pointing out that December 31, 1830, marks Pinkney's chronological terminus.

By conventional standards *The French Revolution of 1830* is an excellent piece of scholarship, intelligently organized, elegantly written, and beautifully produced. As a pioneer effort to reorient American scholarship on France we may hope that it opens the way to even bolder and more searching reassessments of the French past.

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Dearborn

JEAN-PAUL ADAM. *Instauration de la politique des chemins de fer en France*. (Publications de l'Université de Rouen.) Paris: Presses Universitaires de France. 1972. Pp. 201. 35 fr.

This short monograph, concerned with the establishment of a general policy to govern railroad construction in France, focuses upon the role of the extraparlimentary commission, appointed in August 1839 and presided over by Jules Dufaure, the minister of public works. The recommendations of this commission subsequently provided the basis for the Railroad Law of 1842. The first question to be resolved by the commission was who would build the railroads: the state, private companies, or a combination of the two. Since the Chamber of Deputies had already rejected a government bill in 1838 providing for state construction of the major lines, this was not a politically feasible possibility. The commission recommended a joint effort, with the state acquiring the right of way and preparing the roadbed. Private companies were to lay the rails, provide the locomotives and rolling stock, and operate the lines. Mr. Adam concludes, wrongly in my view, that the choice was imposed because the total cost was beyond the resources of either the state or the private sector alone. A better explanation would be that the mixed system neatly balanced competing political and economic interests.

Among the many other questions taken up by the commission was how the state could provide additional aid to the companies. Outright grants

from the state were rejected, but the commission recommended that loans, purchase of shares, and guarantee of interest on shares issued by the companies could be used according to the circumstances. Mr. Adam concludes that of the three permissible means, the commission favored guaranteed interest on railroad shares, which influenced subsequent adoption of this method. Proposals by the commission for ameliorating the law for expropriating private property were legislated in 1841.

While older works have concentrated upon the parliamentary history of the Railroad Law of 1842, the chief merit of Mr. Adam's study is to describe the preparatory work of the commission. Although he adds some important details, the overall story is not altered significantly. Mr. Adam's self-imposed limitation of concentrating narrowly upon the work of the commission results in neglecting outside political and economic forces that contributed to shaping French railroad policy.

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ZEEV STERNHELL. *Maurice Barrès et le nationalisme français*. Preface by RAOUL GIRARDET. (Cahiers de la Fondation nationale des sciences politiques, 182.) Paris: Armand Colin. 1972. Pp. 395. 75 fr.

ROBERT SOUCY. *Fascism in France: The Case of Maurice Barrès*. Berkeley and Los Angeles: University of California Press. 1972. Pp. x, 350. \$15.75.

Both these works challenge the argument that fascist ideology had no significant "roots" in the French political tradition. Each author tries to trace such roots back to Maurice Barrès, whose appeal to young French nationalists before the First World War was surpassed only by that of Charles Maurras. Sternhell gives us an exhaustive intellectual biography of Barrès, describing the influence of other thinkers on him and the various stages of his ideological development. This is good old intellectual history modified to accommodate the vagaries of a self-dramatizing literary publicist. Sternhell seeks the ideas of Barrès in his journalistic writings in order to avoid Barrès's retrospective revisions of his own thought. Contrary to Sternhell, Soucy maintains that the source of Barrès's ideas is

most apparent in his novels and stresses the autobiographical character of the early ones. Soucy also sees the depiction of Barrès's youthful rebellion in *Under the Eyes of the Barbarians* (1888) and *A Free Man* (1889) as expressing a generation gap between authentic and idealistic youth and the phony and corrupt older generation. At the time of the Dreyfus affair Barrès bridged the gap by presenting racist nativism and integral nationalism to France's university youths as the best means for revitalizing their existence. After 1903 Barrès himself became less fascist and more conservative, but both Sternhell and Soucy insist that his fascist influence lived on into the postwar years.

In their documentation and interpretations of Barrès's role in pre-1914 French politics both authors are usually impeccable. Unaccountably, however, neither one mentions Frederic H. Seager's definitive study, *The Boulanger Affair: Political Crossroad of France, 1886-1889* (1969), which emphasizes the initial support for General Boulanger by lower-middle- and lower-class Frenchmen who felt unrepresented in the existing political system. This view is somewhat different from Sternhell's, according to which Boulanger's supporters on the extreme Left wanted to overcome the immobilism of the Opportunist ministries. The difference in interpretation is important for the origins of fascism, which both Sternhell and Soucy seek through Barrès. Their problem is that Barrès came to view Boulangism as an attempt to mobilize the masses through populist nationalism only after the movement had shifted to the right and was already disintegrating.

What is lacking in Sternhell's study is a feeling for Barrès as a person and a certain type of alienated intellectual. There are fairly obvious psychological and sociological dimensions to Barrès's turning from anarchic individualism (*culte du moi*) to nativist nationalism as a means for working out his self-image and his rootlessness in an uncongenial (the code word was "decadent") society. Barrès's behavior was an early example of this kind of projection. In the twentieth century numerous other literary publicists have sought salvation through collectivist political ideologies of both the Right and the Left. But neither Barrès nor most of these others ever resolved the paradox of elitist intel-

lectuals idealizing "the people" as a community while disdaining them as real-life individuals. Indeed, as Sternhell points out, beginning with *The Uprooted* (1897) Barrès adopted an openly patronizing attitude toward "the people."

Soucy provides some of the insights lacking in Sternhell. He says that the narcissism of Barrès in his twenties was partly an escape from the "barbarian" bullies of his childhood. Then he shows how Barrès, discovering that his escape from reality made him feel more insecure than before, found both the security and reality he longed for by integrating his *moi* into the folk soul. "*La terre et les morts* gave him something solid and lasting to identify with, erased his sense of alienation, and offered him the emotional reassurance that comes from being a member of the herd" (p. 106). This last conceit was probably more a pose than a sincere conviction. But Soucy is surely right when he ascribes Barrès's penchant for politics to a desire to test his ideas in the brutal combats of the real world and when he notes that Barrès was a good enough politician to modify his ideas to fit the reality he found there.

The weakness of these two books lies not in their scholarship but in their reasoning about relationships between Barrès's ideas at the end of the nineteenth century and the fascism of the 1930s. The observation that two successive things are similar does not prove that the later one has a direct connection with the earlier one. To assert such a connection without empirical evidence is a logical fallacy: *post hoc, ergo propter hoc*. And to use the similarities as the evidence is to argue in a circle. As both Soucy and Sternhell admit, the fascist Barrès of the turn of the century gave way to a conservative nationalist Barrès who never returned to his earlier position and whose fascism—unlike the integral nationalism of Maurras—was not perpetuated in any movement or by any disciples. Hence there is no proof of "roots" (a sloppy metaphor that should be banished from historical analysis). Terms like "precursor" or "harbinger" of fascism also beg the question of a direct connection; used with caution they can be suggestive, though they strain one's credulity when extended, as they have been by other historians, to Napoleon III, Bismarck, and Giolitti. Individuals and movements take what they want from the past, selectively, and often

use it in perverse ways. After all, Marx stood Hegel on his head.

Maurice Barrès was a bad man by humane, liberal standards, and he was the first to admit this. The same thing can be said of Friedrich Nietzsche, Ezra Pound, Filippo Tommaso Marinetti—or even Jean-Paul Sartre in his recent “Maoist” stance. Nevertheless, in their zeal to prove that in 1900 Barrès’s ideas were closer to fascism than to traditional conservatism, Pétainism, or Gaullism, Sternhell and Soucy may unwittingly obscure the real horrors of fascist practices in the minds of some younger readers for whom all these things are only history.

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JEAN BOUSQUET-MÉLOU. *Louis Barthou et la circonscription d'Oloron (1889-1914)*. Preface by GEORGES DUPEUX. (Bibliothèque de l'Institut d'Études Politiques de Bordeaux, Centre d'Étude et de Recherche sur la Vie locale. Série Vie locale, 3.) [Paris:] Pedone. 1972. Pp. 268. 35 fr.

Louis Barthou is known to most people at the moment of his death in 1934 when, in the process of mounting a diplomatic coalition against Hitler, he was assassinated along with King Alexander of Yugoslavia. Very little has been written about his life before that. In this meticulous work Jean Bousquet-Mélou analyzes the complex relationship between Barthou, the Basses-Pyrénées politician, and his “electoral fief,” Oloron-Sainte-Marie, during the quarter century before World War I. Barthou’s years as deputy from 1889 to 1914 correspond to a logical electoral unit in French history because the *scrutin d'arrondissement* was reintroduced in 1889 to undermine the political appeal of General Boulanger’s followers, and this voting system remained in force through the election of 1914. Although Barthou’s political career did not end until 1934, this early period clearly constitutes a self-contained unit.

The author demonstrates that Barthou was a moderate by temperament. His moderate position was endorsed by a majority of the electors in his mixed rural-urban legislative district, a convergence that proved fortuitous as he was elected to the Chamber of Deputies seven times. The first election was very close; the twenty-seven-year-old blacksmith’s grandson encour-

tered stiff opposition from his conservative opponent, the scion of a “notable” local family. Barthou received a majority of only 300 out of 13,763 votes. The only other difficult election in the first half of the statesman’s career came in 1906 when he, as a consistent anticlerical, lost substantial Catholic support when the separation of Church and state became an emotional political issue.

One of the most significant trends in recent French historiography has been an emphasis on regional case studies. Local centers of historical research, such as the Institut d'Études Politiques de Bordeaux, have particularly encouraged such work. And these detailed political treatments furnish essential building blocks in reconstructing the history of France. This monograph is particularly valuable to students of the Third Republic concerned with the impact of local issues on national politics. Clearly a major figure in this period, Barthou was a sophisticated politician whose talents have too long been overlooked. Bousquet-Mélou shows just how skillful he was, both in the capital and in his home district. In so doing the author presents a careful analysis of the relationship between regional pressure groups and Oloron’s permanent deputy in Paris.

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FÉLIX PONTEIL. *Les bourgeois et la démocratie sociale, 1914-1968*. (Collection “L'évolution de l'humanité,” number 32.) [Paris:] Éditions Albin Michel. 1971. Pp. 560. 12 fr.

In his foreword M. Ponteil confesses that his work lies somewhere in the gray zone between journalism and history and chooses to leave the verdict to his readers. Companion to *Les classes bourgeoises et l'avènement de la démocratie (1815-1914)*, published in 1968, this volume figures in the series “L'évolution de l'humanité,” and, given its title, it is striking that more than eighty per cent of its pages are devoted to France while the remainder are given to perfunctory and often superficial analyses of the Russian Revolution, the Weimar Republic, and England under the Labour party, 1945-51. Sometimes elegant, always intelligent, this is a study of the evolution of French politics and

society since World War I by a determined partisan of social progress. Unfortunately it is uneven in its treatment of key problems and marred by the author's unyielding addiction to hyperbole, overtly polemical characterizations (Léon Blum, "afraid of reality," or, in an approving quotation from an undisclosed source, denunciation of a Socialist leader as that "heavy-jowled equivocator of European Social Democracy"), and by an intransigent insistence upon right versus wrong, upon history in black and white.

Briefly put, M. Ponteil argues that despite a certain progress social democracy is far from realization, notwithstanding fleeting opportunities and working-class heroics, notably in the aftermath of World War I, in the Popular Front period—dismissed as a "straw fire"—and in the violence and ambiguity of 1968. The two camps, bourgeoisie and workers, were frequently in open combat, both conscious of their class solidarity, although the lineup has been altered somewhat by the children of the bourgeoisie who have thrown in their lot with the workers. The barriers to socialism and genuine democracy, argues M. Ponteil, are intellectuals whose very methods sow dissension and dissolution, politicians for whom lying is a way of life, changes in classes, lack of cohesion on the Left, the instinctive conservatism of the French, and, most especially, Socialist politicians. All of this may provoke controversy and further speculation but certainly little surprise, and it may lead readers to conclude that this is not formal historical interpretation or even a synthetic textbook but a tract, and a very long one at that. Despite the setbacks, the pusillanimous behavior of working-class leaders, and the evolution of capitalism M. Ponteil believes the future cannot fail to belong to socialism. The apparent convalescence of capitalism since 1945 is but a temporary remission of its terminal illness, and, like Jean Jaurès, M. Ponteil is convinced that social democracy, by slow progression, will be the fulfillment of the emancipatory process begun in the Revolution.

Whatever the interest evoked by these arguments it has to be said that the text is ill-organized and ill-disciplined. Accusation and compartmentalization often take precedence over careful analysis. If much attention is given to the crucial years of the Third Republic, 1919–

40, the familiar themes of decadence and decline are reaffirmed without doubt as to their truth, and in speaking of domestic crises little or no mention is made of the impact of foreign affairs in the 1930s. Specialists will learn little that is new from this volume, but if the reader is not wholly dismayed by the ease with which M. Ponteil classifies and characterizes entire classes, he will gain insights into the historical methodology of an articulate advocate of social democracy.

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R. WILLIAM RAUCH, JR. *Politics and Belief in Contemporary France: Emmanuel Mounier and Christian Democracy, 1932–1950*. The Hague: Martinus Nijhoff. 1972. Pp. 351. 47.25 gls.

In *Le Monde* of April 6, 1973, André Fontaine identified the task of those who lost in the March elections in France as one of continuing to challenge the "established disorder." When Fontaine reminded his readers that the "established disorder" was Mounier's expression, he testified to the continuing echo of Mounier's voice in the political consciousness of his society. Mounier will also be remembered for having founded the important journal *Esprit*, but beyond this major accomplishment his achievement remains debatable and precarious. Roy Pierce in his judicious *Contemporary French Political Thought* (1966) accurately observed that Mounier "literally erected ambivalence into a philosophical principle" (p. 84). William Rauch's meticulous account of Mounier's thought and action proposes that Mounier's example made possible the pluralism of contemporary French Catholicism.

This study, conceived and executed in the style of Adrien Dansette, is an admiring, but not exactly hagiographic presentation of Mounier. He is characterized as Péguy's worthy and noble intellectual heir and praised endlessly for the "profundity" of his "spirituality." Yet it is impossible for those uninitiated into Mounier's ascetical politics to determine exactly the substance of his messages. Mounier intended to be a Christian witness in the modern world. But what is striking, if unintentional in Rauch's careful analysis, is the unmistakable evidence that Mounier remained



a confessional moralist consumed by the debates within the Catholic community of France.

Professor Rauch persuasively argues that Mounier's criticism of the doctrinal pride and elitism of the Christian democrats devastatingly exposed the limits and prejudices of this political camp. The historian would like to know, however, in a more convincing fashion, why Mounier's piety demanded his uncertain judgment of February 6, 1934, his hesitations concerning the Popular Front, his initial confusion with regard to Vichy, and the trembling of his hand when he offered it to communists. Jacques Maritain, in his most reactionary *Le Paysan de la Garonne* (1966), claimed that Mounier invented the expression "personnaliste et communautaire," and Maritain further added that it became through Mounier a "tarte à la crème" for Catholic thought and rhetoric (p. 82). Rauch suggests that Mounier brought the bread of life to his generation. Rauch's valuable study will allow his readers to decide the merit of this interpretation or that of Mounier's *cher maître*, the peasant philosopher from the Garonne.

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JUAN FRIEDE and BENJAMIN KEEN, editors. *Bartolomé de Las Casas in History: Toward an Understanding of the Man and His Work*. DeKalb: Northern Illinois University Press. 1971. Pp. xiii, 632. \$20.00.

Almost five hundred years after his birth in 1474 the figure of Bartolomé de Las Casas continues to fascinate and infuriate people of every degree. A man capable of unleashing as many passions (e.g., the attack by Menéndez Pidal) as the fiery bishop of Chiapa must a priori be a subject of considerable historical interest. This interest is heightened by the fact that the problems he confronted and fought—racism, colonialism, the conflict of cultures, the justice of war, social justice, ethnography—are with us yet. The fact that he found his way toward solutions and fought for over half a century in behalf of these solutions, and so in behalf of the oppressed and exploited everywhere, makes of Las Casas a man who has as much to say to our age as he had to his own. As Manuel Giménez Fernández has observed, the

men of Vatican II were his spiritual brothers.

All the more remarkable then, especially in view of the deluge of Lascasian literature in this century alone, is the fact that so much about the man remains to be studied. Hence this handsome volume is a welcome addition to the already abundant Las Casas literature. It serves as a fine introduction for the scholar who is not well acquainted with the subject and as a fine survey for the one who is. Its twelve essays embrace the high points of Las Casas's career and thought and do so with remarkable thoroughness and balance. The various offerings maintain a consistently high standard and are developed with logic and continuity.

The first two essays would by themselves justify the book. These are Benjamin Keen's historiographic survey ("Approaches to Las Casas, 1535-1970") and Giménez Fernández's moving if occasionally cryptic survey of Las Casas's life ("Fray Bartolomé de Las Casas: A Biographical Sketch"). Juan Friede ("Las Casas and Indigenism in the Sixteenth Century") follows his career from preacher to politician to activist and traces the radicalization of his thought. This essay also contains a good summary of the Spanish colonial experience in the Canaries and of the effects of the American reality on Las Casas's thought. It can be argued, however, that he overemphasizes the decline of the Lascasian school after and even before the death of the bishop. More recent researches on such events as the Third Mexican Council (1585) and some of the later Mexican bishops indicates that the movement still had vitality around the end of the century.

In the essays dealing with the ideology of Las Casas that of Venancio D. Carro, O.P. ("The Spanish Theological-Juridical Renaissance and the Ideology of Bartolomé de Las Casas") is to me the least satisfying in the volume. It is marred by excessive praise for the Dominican order. Not only is such glorification distasteful but it ignores the historical reality that there were also non-Dominicans who were on the side of the angels. Carro is also on questionable ground when he uses Vitoria as the yardstick to measure the correctness of Las Casas's opinions. Angel Losada ("The Controversy between Sepúlveda and Las Casas in the Junta of Valladolid") summarizes the material to be

found in the still unpublished *Apologia*. Father Manuel Martínez, O.P. ("Las Casas on the Conquest of America") gives a more balanced interpretation than does Carro, especially about Las Casas's ideological relationship to Vitoria.

The third section, "Las Casas in America," begins with a fine essay by Marcel Bataillon ("The *Clérigo* Casas, Colonist and Colonial Reformer") that emphasizes two early, formative influences: his experience as a diocesan priest and the residual effects of the colonial outlook. Father Benno Biermann, O.S.B. ("Bartolomé de Las Casas and Verapaz") gives a good account of this gallant but eventually unsuccessful experiment. The final section, "The Heritage of Las Casas," deals mostly with literary and historiographic material, with essays by Juan Comas ("Historical Reality and the Detractors of Father Las Casas"), V. Afanasiev ("The Literary Heritage of Bartolomé de Las Casas"), and Raymond Marcus ("Las Casas in Literature"). The latter has also contributed the excellent bibliography.

The format of the book is attractive and the illustrations well chosen. Remarkably for such an extensive work typographical and other errors are almost entirely absent. It should be pointed out, however, that "Castilian" on page 12 should clearly be "Catalan" and that "zeal for God's house" (p. 337) does not refer to the Dominican convent but is a citation of Psalm 68 in the Vulgate numbering.

This volume is a fitting memorial not only to Las Casas but also to the late Manuel Giménez Fernández, to whom it is dedicated. Like Las Casas he was a long-time Christian critic of his own government, and his concluding words are an apt tribute to his own life as well as that of the bishop of Chiapa: "Las Casas is the most admirable of the sons of Seville; and around his doctrine, properly applied to present-day conditions, can rally all who are disillusioned with anarchistic individualism, with totalitarianism which degrades the human personality, and with the servile legalism that grovels before Caesar."

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Historians have a success syndrome that causes failures to be understudied, though failures—whether politicians, policies, enterprises, or whatever—are the essential obverse of successes and often play an important role themselves. The present volume helps right that imbalance, for the Spanish Company (with origins traceable to 1517 or earlier) was a perennial failure throughout its existence.

The book contains a 45-page introduction, equally divided between "The First Spanish Company, 1530-1585" and "The Revival of the Company, 1604-1606," plus 124 pages of documents regarding the final period: the company's *Register Book, 1604-06* (73 pages); the *Book of Oaths, Acts and Ordinances* (21 pages); the 1605 charter (19 pages); and five additional documents (11 pages) referred to in the *Register Book*. (Length matters particularly in this sort of volume: in a typical secondary work, pagination would be almost double the tall pages and small type here.) Editing is excellent throughout, including an 18-page index (for 169 pages of text).

The book is useful on three levels. Croft's admirable introduction will reward anyone interested in economic or business history or interested in the period in general. The documents contain much hard data for the researcher. And, in between, from these documents the attentive reader will gain considerable insight into fundamental problems not broached in the introduction. For example, the need for six consulates ringing the peninsula (pp. 50-51), reflecting difficult internal communications, contrasts sharply with the typical chartered company's more effective single entrepôt with access to its whole market area. A merchant's servant arrested by the Inquisition for religious utterances, released, then re-arrested nine months later "for the same words so spoken" (p. 119) suggests that the English brought some of these troubles on themselves.

These documents originate in England and deal mainly with events in London, not in the market area; though Croft's introduction goes well beyond these documents, it is, understandably, similarly limited. The full story naturally requires use of the English ambassadors' reports and Spanish conciliar and other sources, but work is already afoot in

those, and meanwhile we have here a very useful volume.

This ninth annual volume of the London Record Society has, like the eighth, ramifications far beyond the bounds of municipal history. Such an unparochial publication policy bodes well for the series.

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LEON TROTSKY. *The Spanish Revolution (1931-39)*. Introduction by LES EVANS. New York: Pathfinder Press. 1973. Pp. xix, 22-446. Cloth \$10.00, paper \$3.95.

PIERRE BROUÉ. *La révolution espagnole (1931-1939)*. (Questions d'histoire.) [Paris:] Flammarion. 1973. Pp. 190.

HERBERT L. MATTHEWS. *Half of Spain Died: A Reappraisal of the Spanish Civil War*. New York: Charles Scribner's Sons. 1973. Pp. ix, 276. \$10.00.

WILLIAM E. WATTERS. *An International Affair: Non-Intervention in the Spanish Civil War, 1936-1939*. (Exposition-University Book.) New York: Exposition Press. 1971. Pp. 423. \$12.00.

The most interesting of these four works is the collection of Trotsky's writings. From the moment that the king dismissed Primo de Rivera in January 1930 the exiled Russian leader had eagerly followed Spanish developments. He knew that the Communist party was very small and faction-ridden. He hoped to win its best militants for a "Bolshevik-Leninist" rather than a Stalinist policy, and he was optimistic about the combative quality of the Spanish working class. He did not think that the parliamentary stage of revolution could be skipped, but he pictured it as being very brief. He advised his friends not to endorse, much less participate, in any bourgeois government, and he urged them to give ideological and organizational leadership to the uncoordinated strikes taking place in 1931-32. From early 1933 until October 1934 Trotsky was fully occupied with the German situation and with his own efforts—he had moved from Turkey to France in July 1933—to found the Fourth International. Writing in November 1934 he attributed the defeat of the Asturian rising to the "unprincipled political reformism" of the Spanish Socialist party, which, like the Mensheviks and Social Revolutionaries in Russia,

had shared power with bourgeois parties and thereby prevented the development of the proletarian revolution. He believed that Socialist collaboration with Azaña had strengthened anarchosyndicalism on the Left and allowed "social Catholic demagoguery" (i.e., the CEDA of Gil Robles) to exploit the disappointment of the rural masses at the absence of any radical social reform. He was also bitterly critical when his former disciple Andrés Nin joined forces with the "muddled" and numerically insignificant Workers and Peasants Bloc of Joaquín Maurín to found the POUM. If Nin and his followers had been true Bolshevik-Leninists they would have joined the Caballero Socialists in order to prevent the revolutionary wing of the party from making a close alliance with the Stalinists, as happened when the Socialist and Communist youth movements were merged in April 1936.

Trotsky viewed the Popular Front not as an instrument of revolution, but as a means whereby the frightened bourgeoisie obtained Socialist and Communist cooperation to prevent revolution. During the Civil War he called on his followers to be the "best fighters" against the fascists, but he absolutely rejected the Communist and right-wing Socialist slogans about winning the war before thinking of revolution. On the contrary he argued that the masses, and especially the peasants, would have no reason to prefer the Popular Front to Franco unless there was going to be a fundamental land reform. The way to win the war (as in the Russia of 1917) was to urge the workers to seize the factories and the peasants to take the land. "The fascist army could not resist the influence of such a program for 24 hours," he wrote on July 30, 1936. And in April and May 1937 he reiterated that without the proletarian revolution even a possible victory for the Republican armies would only be a roundabout path to fascism. His exposure of vacillations and contradictions often contain delightful flashes of humor as when he writes, concerning *La Batalla's* fumbling endorsement of the Popular Front: "One cannot say on Monday that the League of Nations is a band of brigands; on Tuesday urge the voters to vote for the program of the League of Nations; on Wednesday explain that it was only a question of electoral action, and that today one has to resume one's

own program." But Trotsky, for all his brilliance, fundamentally misjudged the Spanish context. Constantly drawing parallels to the October Revolution, he was unable to recognize that the Spain of 1931, and of 1936, had not (in contrast to the Russia of 1917) been ravaged by three years of war, typhus, and transportation breakdown; nor did he realize that the army, the Church, and the middle classes possessed corporate strength far greater than that of their Russian counterparts. For him all was a question of boldness, but he may have been right in his psychological judgment that Nin did not really want to lead a revolution. The book is well indexed and contains very useful thumbnail biographies of the many persons mentioned in the documents.

The first hundred pages of the Broué volume are devoted to a brief analysis of the Spanish Left in the 1930s. The interpretation is very similar to that which he gave in Broué and Témime, *La révolution et la guerre d'Espagne*, but the discussion concentrates more on ideas and political organizations than upon narration. This section is followed by fifty pages of documents well chosen to illustrate the evolution of the Socialist and Communist parties, the CNT, the FAI and the POUM, and the incursion of Soviet politics in Spain. Perhaps the most valuable feature of the book for scholars is the twenty-page "state of the question" essay. No other readily available volume identifies and places so many political leaders of the Spanish Left, and no other scholar has so successfully clarified their significant ideological, political, and personal differences. The footnotes contain unique bibliographical references for the immense periodical literature on the subject.

Herbert Matthews was one of a handful of truly superior journalists who both reported the Civil War vividly and interpreted it with great insight. The present book, however, has a curiously disjointed character. It is as if the author had reviewed his own dispatches, recalled his emotions and interpretations of the time, read Gerald Brenan, Burnett Bolloten, Hugh Thomas, Stanley Payne, and Gabriel Jackson, decided where he agreed and where he disagreed with each of them, and then sat down at the typewriter. The factual outlines, seen from a generally pro-Loyalist perspective, but

without whitewash, are there. Nevertheless, his new reading, and the history of the last three decades, do not seem significantly to have affected his thinking. Thus the book has an inert, anachronistic quality, and it is likely to disappoint those who have read such earlier Matthews works as *The Education of a Correspondent* (1946) and *The Yoke and the Arrows* (1957). Mr. Watters's book will not be of great interest to historians. It is superficial diplomatic history, based principally on newspaper sources, parliamentary records, and published diplomatic papers. He is uncritical of these sources, and he seems to be almost completely unaware of the hundreds of books and articles that could have contributed to a really scholarly synthesis concerning the international aspects of the Civil War.

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A. H. DE OLIVEIRA MARQUES. *History of Portugal*. Volume 1, *From Lusitania to Empire*; volume 2, *From Empire to Corporate State*. New York: Columbia University Press. 1972. Pp. viii, 507; 303. \$15.00 each.

With the publication of this two-volume study, Oliveira Marques has provided the English reader with the most significant general history of Portugal and her empire to date. The work is a decided improvement on its predecessors. The author, who is already noted for monographic studies in the medieval period and, most recently, for a history of the First Republic, has brought together the wide range of his research to present a balanced narrative. The introduction and chapters 1 and 2 compress an array of factual information into a discussion of Portugal's origins as a state and her medieval development. Chapters 3 and 4 are allotted to the discoveries, rise, and collapse of the first overseas empire, and the impact of these events upon Portugal during the years 1400-1700. Three chapters treat the seventeenth and eighteenth centuries, one on Portugal and the remaining two on Brazil and the rest of her tridimensional empire. The second volume, encompassing the period from the nadir of Portugal's power in the 1820s until the present, is almost equally divided between the nation's own gradual internal evolution

toward republicanism and the New State and the creation of a third empire in Africa.

The tone of this work is anything but impartial. In his quest for broad underlying trends and long-range movements to describe Portugal's development, the author presents a tour de force of interpretive history. Three particular examples are outstanding. Oliveira Marques thrusts himself into the long-standing debate over the creation of Portugal from a cultural uniqueness in the dim past versus political events in eleventh-century Iberia. He opts for the former and shows convincingly how Portugal emerged from centuries of fusion between two distinct cultural regions in the western peninsula—the first is an area north of the Douro River containing dominant Celtic-Suevian-Christian influences, while the second region lies south of the Tagus, with its deep African-Muslim traditions. The *reconquista* is viewed as a bringing together of these divergent cultures to create a national identity.

It is refreshing to note that the author rejects the notion of an "artificial seignorial non-feudal Portugal" that has been the former trend among most historians. Utilizing recent scholarship in this field, he argues convincingly for a Portuguese feudal society with unique elements not found elsewhere in European feudalism. In particular, emphasis is laid upon Mozarabic and Muslim feudal society, which is derived from the southern portion of the kingdom. In his own words, "once the idea of a monolithic and geographically restricted feudalism is put aside, the interpretation of medieval and early modern Portugal as a feudal state ceases to be a riddle."

The third major theme the author develops concerns the close economic relationship between Portugal and her colonial possessions. He traces the development of a new colonial policy in the late nineteenth and twentieth centuries and seeks to understand colonial growth as being inextricably linked with Portugal itself. The African colonies of Angola and Mozambique, in particular, are visualized as a "magnified replica of the history of Portugal, with all of her slow economic development, defects in social structure, and cultural backwardness."

Oliveira Marques interprets Salazar's New State as a totalitarian regime, despite its façade

of representative democracy. Marques's treatment of Portugal's economic and political development since 1926 is particularly valuable to those readers who have not seen Hugh Kay's *Salazar and Modern Portugal* (1970), which, incidentally, is included in the bibliography.

Perhaps the least successful portion of the work is the treatment of the constitutional monarchy (chapter 10). Though every page is filled with statistical data and a recounting of the labyrinthine political and economic maneuvers, the author never quite comes to grips with the period as a whole. His discussion remains somewhat fragmented and difficult to follow. Perhaps one of the reasons for this is the placement of the reciting of chronological events at the end rather than at the commencement of the chapter. Another flaw is the misplacement of maps (pp. 63, 112).

Despite these minor shortcomings, much merit remains. It is precisely Oliveira Marques's persistent concentration on the social and economic, rather than political, aspects of his history that makes it a welcome study for students seeking a significant introduction to Portuguese history. Even the experienced scholar will find something of value in the interpretations presented here, although he may not agree on all points. Also of value is the annotated bibliography at the end of each volume, which presents the most recent research, including works by Magalhães Godinho, Paulo Merêa, Russell-Wood, and the author himself.

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JORMA KALELA. *Grannar på skilda vägar: Det finländsk-svenska samarbetet i den finländska och svenska utrikespolitiken, 1921-1923* [Neighbors on Separate Roads: Finnish-Swedish Cooperation in the Finnish and Swedish Foreign Policies, 1921-1923]. (Historiallisia tutkimuksia, 84.) Helsingfors: Suomen Historiallinen Seura. 1971. Pp. 313.

Finnish scholars have been relatively slow in coming to grips with their country's history during the period of independence, but recently there have appeared a number of book-length studies in particular about several aspects of the international relations of Finland. Jorma

Kalela's work falls into this last category. It is a study about Finland's relations with Sweden within a two-year period. Kalela has sought to answer the question of why these two neighboring states did not establish closer connections and cooperation with one another. His basic conclusion is that their conceptions of their basic self-interests were too different to allow for close military or diplomatic cooperation. According to Kalela this fundamental fact rather than any single issue, like the dispute over the Åland Islands or the language and nationality conflict in Finland between the Swedish-speaking minority and the Finnish-speaking majority, kept the two states politically apart from one another.

In order to solve his research problem Kalela thought it necessary to rely heavily for his theoretical framework, interpretation, and terminology on such American political scientists as Richard Snyder, Harold Sprout, Michael Brecher, and Herbert Kelman. His as such commendable effort to define and be precise in terminology has, however, led him to excessive use of social-science jargon in discussing relatively simple and clear-cut historical issues with the result at times of obscuring rather than clarifying them. The clarity of the study also suffers from insufficient integration of the historical material with the theoretical framework, in spite of efforts to make it fit to the extent of downplaying or bypassing evidence that does not seem to support the adopted theoretical framework and hypothesis. In general the author ignores or underestimates the influence of emotional or irrational factors, the plain logic of events or the dynamics of policies as reactions to one another. Just to mention one example, he neglects to assess or analyze the impact of Sweden's refusal to join the "Whites" in the Finnish civil war.

The work adds little in the way of original contribution to the theoretical discussion of the nature of international relations, and, because of its narrowly circumscribed subject matter, it does not either provide much in the way of significant new historical information. It may familiarize some Scandinavian readers with a few standard American social-science concepts that they perhaps had not been confronted with previously. Its main value seems to lie in the author's effort to give balanced

treatment and coverage to both sides when discussing the relations between the two states.

On the whole the bulk of the work is extensively documented, but there are some odd omissions in the bibliography and a few factual mistakes. But Kalela strives to be objective and unbiased in discussing the policies of the two countries, and by and large he succeeds in this effort.

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H. C. ERIK MIDELFORT. *Witch Hunting in Southwestern Germany, 1562-1684: The Social and Intellectual Foundations*. Stanford: Stanford University Press. 1972. Pp. viii, 306. \$11.50.

In his examination of witch-hunting in southwestern Germany Professor Midelfort makes a solid contribution to our understanding of this strange phenomenon of the sixteenth and seventeenth centuries. He set himself the task of testing previous explanations of witchcraft and also of addressing himself to new questions raised by legal historians, anthropologists, and psychoanalysts by studying local, legal, and archival evidence within a limited area, that comprising Baden and Württemberg with their many different political and ecclesiastical jurisdictions. To make the mass of materials manageable, he examined in detail only the large witch-hunts, which he arbitrarily defined as those resulting in twenty or more executions a year.

Avoiding the sensational, Midelfort considers the three crucial elements involved in witchcraft, namely, the pact with the devil, demonic power, and divine permission. He points out that there was no clear orthodox theory concerning the essence of the mania but a combination of theories based on current legal practice, scholastic reasoning, and practices of heretics. Although virtually no one doubted the power of the devil or the presence of witches, men differed with respect to the significance they placed on the witch's pact with the devil, the difference between harmful and harmless magic, the ability of witches to cause physical harm, and the providential role in storms and other disasters.

The most important conclusion of this study

is that the large witch-hunts of southwestern Germany came to an end, not because of intellectual enlightenment but because the witch-hunters had reached a crisis of confidence in their ability to identify witches. Among other important conclusions are the following: that greed played little or no role; that Europe in the Reformation era was too far advanced to warrant the application of findings in primitive societies; that there is no evidence to support the contention that Catholicism was more severe on witches than Protestantism, even though there were more large trials in Catholic lands; that the stereotype of the old woman as witch broke down; that no one class provided the victims; and that the large trials served no discernible social function. The book contains a detailed bibliography, maps, illustrations, and numerous tables of data concerning the trials.

HEROLD J. GRIMM  
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HERBERT SCHOTTELIUS and WILHELM DEIST, editors. *Marine und Marinepolitik im kaiserlichen Deutschland, 1871-1914*. Düsseldorf: Droste Verlag. 1972. Pp. 328. DM 38.

The Imperial navy, even as contemporaries recognized, represented a microcosm of German society. For the most part, however, historians of the 1897-1914 period have treated the building of a navy as the visible sign of Wilhelmian Germany's *Griff nach der Weltmacht* while neglecting the economic, social, and political implications of the fleet-building program. With the centralization of the military archives in Freiburg, scholars were able to study in detail an institution that was of pivotal importance for both the foreign and domestic politics of Imperial Germany. This volume, the record of the April 1972 Kirchzarten conference on the theme "Navy and Naval Politics in Imperial Germany 1871-1914," demonstrates the breadth and direction of the new research and the nature of the issues involved in the debate over the building of the German fleet. In his summary essay, Friedrich Forstmeier adroitly presents the historical verdict in six key areas: the justification for the building of a fleet; the tasks and aims of the German fleet; the development of German-English relations in the period of competition; the impact of the *Dreadnought* on the naval race; the domestic political

implications of the Tirpitz plan; and the importance of Tirpitz's personality for the program and the development of the navy.

The most important contribution of the thirteen German and foreign scholars is Volker Berghahn's "Der Tirpitz-Plan und die Krisis des preussisch-deutschen Herrschaftssystems." Berghahn describes the building of the German navy as the result of a deliberate "crisis strategy" in order to maintain the status quo of Imperial Germany's ruling elite against the Reichstag and the growing threat of socialism. At the same time, the Imperial navy would force England to make way for Germany's overseas expansion.

The thesis of a duality in domestic and foreign politics, explored more fully in Berghahn's work on the origins and fall of the Tirpitz plan (*Der Tirpitz-Plan. Genesis und Verfall einer innenpolitischen Krisenstrategie unter Wilhelm II* [1971]), constitutes a central theme of the contributors of this volume and establishes a valuable framework for further research. Peter-Christian Witt's study of Reich finances and armaments policy reveals the government's unsuccessful attempts to replace the antiquated tax structure, which had been strained considerably by the increased costs in shipbuilding after the *Dreadnought* "breakthrough." The financial needs of the military, especially the navy, threatened the system-stabilizing *Sammlungspolitik* and, in the end, caused the government to lose the support of those very political, social, and economic groups whose interests it was trying to preserve. With the Morocco crisis, the dilemma of German *Weltpolitik* became obvious and the armament needs of the army received priority. This return to Germany's more traditional Continental policy in 1911, as corroborated by Harmut Pogge v. Strandmann's essay on the role of the national leagues, clearly demonstrated that building of a navy had failed to fulfill either Germany's domestic or foreign policy goals. And discussions of the "Prussianizing" of the naval officer corps (Holger Herwig) or the problems of German naval strategy (for example, Paul Kennedy and Edward Wegener) reflect just how completely the hopes (and claims) of the navy's supporters were shattered in the last years before 1914.

KEITH W. BIRD  
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FRITZ T. EPSTEIN. *Germany and the East: Selected Essays*. Edited, with an introduction, by ROBERT F. BYRNES. Bloomington: Indiana University Press. 1973. Pp. xix, \$6.95.

Fritz Epstein is widely known for his bibliographical expertise, and countless researchers are indebted to him for sound advice and generous assistance. His historical essays, scattered through various German journals, will be known only to a few specialists. Some of these essays—originally published (with one exception) between 1954 and 1966 and now slightly revised—have been translated and brought together as a tribute to Epstein by his former colleagues at the Russian and East European Institute at Indiana University. Robert F. Byrnes, who signs as editor, has contributed an affectionate introduction, blending biographical detail with an appreciation of Epstein's scholarly interests and accomplishments.

The nine essays in this collection are grouped around the theme set forth in the title. Five deal with the ways in which German politicians and historians have conceived of Eastern Europe and Russia and how these conceptions shaped historical thought, political analysis, and, especially, foreign policy. To elucidate, Epstein focuses on Germany's *Ostpolitik* during World War I, which, as it culminated at Brest-Litovsk, aimed at controlling and exploiting the East. Epstein first traces the intellectual roots of this wartime policy back into the mid-nineteenth century, to the concept of *Mittleuropa*, which was as much defensive as it was offensive. Then, in two long review essays of the work of Fritz Fischer and of some of his critics, Epstein examines the dissemination and application of this policy, assessing both its impact and its importance. Finally, in pieces on Otto Hoetzsch and Friedrich Meinecke, two conservative historians, he presents a contemporary critique of the policy. Taken together, these essays provide perspectives on Germany's relationship with her Eastern neighbors, which are as relevant today as they were sixty years ago.

Two of the remaining essays discuss Soviet Russia and the Versailles Treaty. In "Russia and the League of Nations" Epstein reconstructs the young League's attitude toward the Soviet Union. Soviet animosity toward the League, it would appear, arose not only because Western statesmen and White Russian generals saw the League as an instrument to counter

(or destroy) bolshevism. It grew also because the possibility of League mandates over Russian territories, proposed at the Peace Conference and promoted by representatives of various Russian nationalities, challenged the Soviet commitment to a unified state. In "The Question of Polish Reparation Claims, 1919-1922" Epstein examines Russia's (and Poland's) rights to reparations under the peace treaty, disentangling neatly the juridical and political aspects of these rights. There is also an essay on Soviet educational policy (dating from 1932) and one comparing the selection and publication of Germany's diplomatic documents after the two wars—the *Grosse Politik* and the *Documents on German Foreign Policy*.

Epstein's strength lies in accumulating evidence and reporting his findings. Each essay is carefully constructed and documented, drawing on a wide range of archival sources and recondite printed material and on a full knowledge of the secondary literature. The essays are monuments to great learning, to precision and attention to detail. They have suffered little from the passage of time.

CHRISTOPH M. KIMMICH  
Columbia University

HAGEN SCHULZE, editor. *Das Kabinett Scheidemann: 13. Februar bis 20. Juni 1919*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1971. Pp. lxxvii, 554.

MARTIN VOGT, editor. *Das Kabinett Müller I.: 27. März bis 21. Juni 1920*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1971. Pp. lxxi, 375.

PETER WULF, editor. *Das Kabinett Fehrenbach: 25. Juni 1920 bis 4. Mai 1921*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1972. Pp. lxxx, 720.

MARTIN VOGT, editor. *Das Kabinett Müller II.: 28. Juni 1928 bis 27. März 1930*. Volume 1, *Juni 1928 bis Juli 1929; Dokumente Nr. 1 bis 256*; volume 2, *August 1929 bis März 1930; Dokumente Nr. 257 bis 489*. (Akten der Reichskanzlei: Weimarer Republik.) Boppard am Rhein: Harald Boldt Verlag. 1970. Pp. lxxxviii, 835; v, 837-1682.

These volumes are part of an ambitious documentation project sponsored jointly by the Bavarian Academy of Sciences and the West



German Bundesarchiv, with Karl Dietrich Erdmann and Wolfgang Mommsen serving as general editors. The aim is to publish selected documents from the files of the Reich Chancellery for each of the cabinets of the Weimar Republic. One previous volume, dealing with the chancellorship of Wilhelm Cuno (November 1922 to August 1923) appeared in 1968 (*AHR*, 75 [1970]: 1150). After completion of the Weimar series, the editors plan to add additional volumes containing chancellery documents from the Third Reich. When coupled with the volumes on the period from October 1918 to February 1919, prepared under the auspices of the Commission for the History of Parliamentarism and Political Parties in Bonn, the Weimar and Third Reich projects will eventually provide a voluminous public record of central governmental authority in Germany between 1918 and 1945. This will be an achievement unparalleled in any other major country for such a recent period of history.

Although these five volumes make available in print more than a thousand documents, they contain no surprising revelations, since the full collection of Reich Chancellery files has been accessible to researchers at the Bundesarchiv for more than a decade. Part of the collection, mainly the minutes of the cabinet meetings and attached documents, was issued as a microfilm publication of the National Archives of the United States nearly twenty years ago and has similarly been used by many scholars. The value of these printed volumes lies in making possible a wider dissemination of the documents and in ordering them in such a manner as to maximize their intelligibility. The latter effect is achieved by the fortunate choice of a chronological system of organization that juxtaposes documents on concurrent developments rather than attempting to group them topically. This accords the researcher an opportunity to grasp the concatenation of events by reading seriatim documents that are scattered about in the archival collection, filed away under a variety of rubrics. Included are not only the cabinet minutes, but also legislative drafts, internal chancellery memoranda, and correspondence with other government agencies, the governments of the federal states, trade unions, lobbies, and other pressure groups. Taken together, these convey a sense of the circum-

stances in which decisions were made that can easily be missed if one is investigating a specific topic, reading only the apposite files in the Bundesarchiv.

Those embarking upon studies of limited topics must still have recourse to the full archival collection, but they, too, will profit from these volumes. By means of the exhaustive indexes of names and topics with which each volume has been provided, it is possible to follow a given theme throughout the tenure of each cabinet and identify those archival files where additional information on it may be found. Also illuminating are the extensive introductory essays by each volume editor. These contain an account of the origins of the cabinet in question, identifying information about each minister, and a description of the major issues that confronted the cabinet, with references to key documents.

Since the cabinet minutes themselves usually consist of laconic, not to say bland, summaries rather than verbatim records of what was said, the decision to supplement them with other documents was a wise one. By reading memoranda and correspondence surrounding a cabinet meeting, one often becomes aware of differences that were minimized or concealed—possibly deliberately—in preparing the minutes. A particularly difficult problem was presented by the cabinet of Philipp Scheidemann, for whose early meetings in 1919 minutes were either not kept or have since been lost. The editor of that volume has done a commendable job of finding substitutes for the missing records by drawing upon a variety of other documentary collections and published memoirs. This is one of the rare instances in which the editorial principle of limiting the volumes to material from the Reich Chancellery collection is—of necessity—extensively violated.

There is little to criticize in this carefully conceived and scrupulously executed undertaking. Two minor cavils can, however, be raised. It is unfortunate that only two of the volumes thus far published (those covering the Cuno and Scheidemann cabinets) contain Erdmann's informative foreword about the past vicissitudes of the documents of the chancellery, about that office's structure and procedures, and about the project's editorial principles. It would enhance the value of future volumes if this foreword

could be reprinted in each. Also useful would be the inclusion in each subsequent volume of basic bibliographical information about all of the preceding volumes, something unaccountably missing in those thus far released. This would be especially helpful to those wishing to order back volumes.

These minor deficiencies in no way detract from this admirable collection's value as a quarry of information for a wide variety of historical inquiries. It will be an indispensable research tool and a boon to the training of advanced research students. One can only hope that it will also inspire other countries to emulate the fine example it sets.

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Yale University

BARTON WHALEY. *Codeword Barbarossa*. Cambridge, Mass.: MIT Press. 1973. Pp. 376. \$10.00.

Although rather narrow in time and topic, Dr. Whaley's book is a valuable supplement to the broader diplomatic histories. And the authors of future, more general accounts will find it very useful indeed. It is a detailed study of how Hitler achieved strategic surprise in his attack on the Soviet Union.

The author carefully examines the various intelligence services and the information they obtained. Almost everyone, it seems, was reading everyone else's telegrams. Only the important Soviet codes remained unbroken. Dr. Whaley devotes three chapters to the many warnings Stalin received: from the United States, from Churchill, and from his own agents in Switzerland and Japan. Hitler succeeded, the author concludes, not in concealing his troop concentrations but in deliberately fooling Stalin regarding their aim. Stalin apparently believed Hitler would make certain demands, and the build-up was for the purpose of enforcing them. General Antonescu shared this same belief until June 11, 1941; the Finns until about June 15.

An interesting question remains. Why did this explanation make so much sense? As he intimated to Cripps on July 1, 1940, Stalin was convinced that the British blockade prevented Hitler from consolidating his conquests. As long as it continued, Germany and Western Europe depended on the Soviet Union for food and raw

materials. And these could be secured in greater volume, and much more easily, by blackmail than by force.

It still is not generally realized, however, that Hitler based his decision to attack Russia on sound strategic grounds: Molotov made his demands in Berlin in November 1940 during the first Axis reverses of the war (the Greeks were driving the Italians back into Albania). This convinced Hitler that he could not trust the Russians. "They can't call the tune on their own," he declared on December 5, "but they will exploit every opportunity to weaken the position of the Axis." At the time he also expected eventual American intervention in the war. The obvious solution was to conquer Russia before this happened. Then, with assured supplies of food and raw material, and with Germany's rear secure, Hitler could easily defend himself against an Anglo-American alliance in the west.

The moral of this story is simple. When they have a choice, policy makers should not plan on the basis of what they think their opponent will do but on what he can do.

JAMES E. MCSHERRY  
Palo Alto, California

MICHAEL BALFOUR and JULIAN FRISBY. *Helmuth von Moltke: A Leader against Hitler*. [New York:] St. Martin's Press. 1972. Pp. x, 388. \$16.95.

This is an important addition to the growing literature on the German Resistance movement. It was natural that historians concentrate first on people involved in the abortive putsch of July 20, 1944. Now a very different group is receiving the attention that it deserves. Ger van Roon's *Count von Moltke and the Kreisau Circle* (1971) dealt primarily with the ideas and activities of the group as a whole. The present biography rounds out the picture by tracing the life of Moltke as an individual.

Named after the Moltke family estate in Silesia, the Kreisau Circle was not a tightly organized band of conspirators but an informal association of well-placed young Germans concerned about their country's future. Their aim was not to engineer the overthrow of Hitler (they were not in agreement about the method or even the wisdom of doing that). Instead,

they were engaged in planning the new Germany that they wanted to see replace the Third Reich, however its end might come.

Helmuth von Moltke was the moving spirit of the group, and he typified it in his person: he bore a historic name, was young (only thirty-seven when he was executed in January 1945), held a second-echelon government post (as the expert on international law in Admiral Canaris's *Abwehr*), and believed that Germany could only become capable of stable and civilized self-government after a moral rejuvenation based on Christian principles. He also believed that the country must be politically decentralized, since only in the "face-to-face community" could the citizen achieve true participation and experience. Finally, the new Germany must rest on a fully open society with equity for all (Moltke had actually voted for the communist Thälmann in the presidential election of 1932!).

The great merit of this biography is to show how a Prussian aristocrat arrived at these views, so foreign to the traditional outlook of the ruling class and so different from the motives of many in the military wing of the Resistance. A good deal of the story is told in Moltke's own words through extracts from his voluminous correspondence, especially with his wife. What emerges is not so much an account of events—though Moltke's travels through occupied Europe are fascinating and the description (pp. 216–22) of what life is like in a totalitarian state is the best that I have seen—as of the development and steeling of a noble character.

Throughout the book the authors use the German word *Widerstand* to remind the reader that resistance inside the Third Reich was necessarily different from that in conquered countries. The term is particularly apt when applied to Moltke and the Kreisau Circle. Their "stand-against" the Nazi regime was moral rather than practical. Though Moltke did communicate with the enemy and though he used his office to ameliorate the lot of hostages, POW's, and forced laborers, this is not why he was indicted and executed. The real reason was stated at the trial by the infamous Judge Freisler: "The mask is off. . . . Only in one respect are we and Christianity alike; we demand the whole man." Moltke and his circle possessed humanity and conscience; this was

too much for their survival under the Third Reich. As Moltke said in a moving last letter to his wife, he was to be killed not for what he had done but for what he had thought.

For the authors, this book was a labor of love in the literal sense: both knew Helmuth and the von Moltke family intimately. In the preface they state their desire that the reader should "through the book come to know the man." They have succeeded. Countess von Moltke's appendix, "The Last Months in Kreisau," not only provides a satisfying end to the story but also shows why this remarkable man should have felt the need to communicate almost daily with an equally remarkable woman.

ROBERT E. NEIL  
Oberlin College

PAUL P. BERNARD. *Jesuits and Jacobins: Enlightenment and Enlightened Despotism in Austria*. Urbana: University of Illinois Press. 1971. Pp. ix, 198. \$7.50.

Jacobins in Austria, judging by the number of recent works on this subject, still stir imaginations. But Bernard is not really all that concerned with them; his title is misleading since he deals less with Jesuits and Jacobins than with what was in between. Using many contemporary publications, he makes not only a cause but some assumptions—such as that historians have more or less neglected an Austrian Enlightenment intelligentsia, to which he then does justice. Arranged somewhat forcibly into chapter categories are a series of vignettes of Austrian literati worth being written about. Their aims, however, whether utilitarian or humanitarian, are not always clear.

Drawn mainly from the bureaucracy, frequently trained by Jesuits or associated at one time or another with Masons and Illuminati, Austria's social-political critics looked to the state for redress. Despite a capricious censorship that briefly loosened early in Joseph's sole reign, they poured out their occasionally insolent products, mostly, to be sure, the expected attacks on intolerance, inequality, birth privilege, peasant abuse, feudal barbarity, guild restriction, and clerical presumption. They ranted, ridiculed, and rationalized as partisans, as applauders or, even, as outdoers of Josephinian reform. When, cresting in a *Broschurenflut*, the

publication honeymoon ended in the mid-eighties with Joseph's clamping down on Masons and a toughened censorship, skepticism mixed with disenchantment. Bernard's array of mostly petty writers does include the inevitable and imposing Sonnenfels—"for a time one of the great German liberals"—and culminates with a most perceptive social critic—Joseph Richter, the author of the provocative *Why Is Kaiser Joseph Not Loved by His People?* This "complete Josephinian" was also a clever adapter, but more important, a mean between moderates and radicals.

Yet, as Bernard emphasizes, it was not a Jacobin revolution that threatened Austria in 1790 but rather one by the national estates. There were indeed a few Austrian Jacobins and they were not merely imitators of the French; they had native roots. Of course, of the various small groups declaiming on tyranny, not all those using the radical arguments of the Enlightenment were necessarily either radical or enlightened. Bernard insists that the literati had nothing to do with the supposed Jacobin conspiracy and, rather playing down the idea of a disgruntled fourth estate, he terms as nonsense the notion that the absurd conspiracy charade was the logical consequence of the Josephinian Enlightenment.

Reviewing a spectrum of not always accurate historical opinions, Bernard concludes on an eclectic note, that one cannot ignore the Austrian literati who were in some ways more genuine than Joseph and who helped create a climate favorable to reform, and who, one might add, despite all their complaining, were something more than the typical *Wiener Raunzer*. After all, as Bernard indicates, though their political ambitions were as modest as their literary quality, in their own Austrian way they had made their point. And that is the point of this useful, compact book.

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Knoxville

OLIVER LOGAN. *Culture and Society in Venice, 1470-1790: The Renaissance and Its Heritage*. New York: Charles Scribner's Sons. 1972. Pp. 344. \$12.50.

Oliver Logan's study of Venetian culture and society in the last three centuries of the re-

public is a splendid, scholarly, and suggestive work; splendid because it fully samples the feast of that sumptuous civilization, scholarly because it re-evaluates old formulas, drawing on archival and contemporary sources as well as recent studies, and suggestive because a multiplicity of interdisciplinary bridges marks Mr. Logan's map of Venetian life. In his first chapter on the mythology of Venice he sketches the mythic profile of the *città galante*, rich and Epicurean, enjoying perpetual liberty and a perfectly balanced constitution. Briefly summarizing the governmental structure and social framework from which these idealized images of *stato di libertà* and *stato misto* derive, he traces the myth's formulation in Venetian and Florentine accounts of the early sixteenth century. Much of its potency and creativity he attributes to the very uncertainty of the city's nature, "which enabled men to see in her the concrete confirmation of their own different ideals." But it is not that myth which concerns Mr. Logan so much as a scrutiny of the social and cultural realities of the city and a definition of the erudite and austere Venetian ethos. The tensions between the active and contemplative lives that Venetian society experienced and expressed, the scholarly institutions it fostered, and the generations of Barbari, Bembi, and Grimani who maintained its intellectual commitments, occupy the early chapters, along with the cultural role of the Venetian dominions, a crucial matter that provides a leitmotiv of the whole work.

The major emphasis falls upon the relationship of this Venetian and provincial society to literature, the visual arts, and music. A long chapter is devoted to patronage and the collecting of art, and this section is amplified by a substantial appendix listing art patrons of Venice and the Veneto in the sixteenth and early seventeenth centuries. Here Mr. Logan's heroes may be found: "the great antiquarian Federigo Contarini," "the great Alvise Corner," "the great Leonardo Donà." Mr. Logan's use of wills and contemporary inventories such as the *Anonimo Morelliano* is exemplary. Not only has he tapped these often neglected resources, but he goes beyond mere cataloging and at every occasion seeks out possible implications of his cultural evidence. Did the antiquarian mania of the later sixteenth cen-

tury reflect the uncertainties of international trade? "Heavy artistic expenditure is a sign of recent wealth, but not necessarily of expanding or stable wealth." Was not art collecting, as represented by the "studio," an acceptable form of expenditure as opposed to ostentatious weddings and extravagant female dress? Such broader considerations are accompanied by close analyses of the artistic, literary, and musical traditions: the Venetian interpretations of *colore* and *disegno*, the development of *popolaresco* comedy, and the qualifications and salaries of *maestri di capella*. The final chapter, following this same method of analysis and hypothesis, returns to the theme of Venetian mythology in its eighteenth-century guise. No longer is Venice the central subject of pictorial apotheoses, but families such as the Pisani and Rezzonica rise to glory on the ceilings of private *palazzi*. The old Venetian aspiration toward grandeur has been abandoned, and the book ends, as do most books on this period of Venetian history, with references to nostalgia and escapism.

One might ask for more specific page references: where is the English ambassador's remark that the passion for luxury increased as trade declined? Where does Sanudo (in his fifty-eight volumes) describe the performances of Ruzzante's comedies in Venice? One might, in conclusion, speculate that Venice in the eighteenth century had less to mourn and escape than we tend to provide, submitting as we do to those poetical and mythical resonances we so ably identify. Yet a myth may have a certain likelihood, and insatiety and speculation are surely profitable fruits of this valuable addition to the field of Venetian studies.

PATRICIA H. LABALME  
Barnard College

DERMOT FENLON. *Heresy and Obedience in Tridentine Italy: Cardinal Pole and the Counter Reformation*. New York: Cambridge University Press. 1972. Pp. xiii, 300. \$19.50.

This monograph is a study of Pole's Italian career, by far the greatest part of his adult life, and, secondarily, it claims to be an investigation of the influence of the Protestant Reformation on Pole and his circle. The book is fundamentally an intellectual biography of Pole, and

of other figures insofar as they touched him. After an analysis of Italian evangelism, which the author defines as an attempt to accept Luther's justification by faith within the framework of the Church, Fenlon moves to Pole. Not trained in theology, Pole came to his religious convictions from a humanist background and from Scripture. For Pole, faith justified and love directed men to good works, while the fundamental question of whether works were meritorious was left unanswered. At the same time, Pole was unwaveringly loyal to the papacy and Catholicism and, to a certain extent, did not seem aware of the contradictions in his thought. Held fast by the twin anchors of justification by faith and fidelity to Rome, Pole would sometimes equivocate and dissimulate in order to avoid difficulty. He looked forward to the council for approbation of his own views and for Christian union, but was grievously disappointed. From that point, although he came close to the papal tiara in 1550, Pole was increasingly the target of slander and suspicion from both Italian Protestant exiles and zealots like Cardinal Carafa. Fenlon concludes with a picture of Pole as a sincerely religious if difficult man in whom caution and intransigence stood side by side. Ineffective as a public figure, fate put him into an era and a role for which he was unsuited. Twentieth-century religious developments perhaps will stimulate in contemporary men more sympathy and understanding for Pole than he enjoyed in his own time.

Fenlon accomplishes his major purpose, that is, to provide a religious biography of Pole. He has not uncovered any new sources, nor is the essential story novel, but he has read carefully and discriminately, and his analysis provides new insights. For example, his account of the authorship of the *Beneficio di Cristo* is perceptive and convincing. The major weakness, one which does not detract substantially, is that the author's treatment of the Italian religious milieu is sketchy. Bibliographical deficiencies sometimes lead him to make dubious judgments on the extent and nature of Italian Protestantism. Although Pole and his circle were the most unworldly of men and women, more information on ecclesiastical politics would also have helped.

In summary, this is a competent monograph

that accomplishes most of what it set out to do.

PAUL F. GRENDLER

University of Toronto

HUBERTUS BERGWITZ. *Die Partisanenrepublik Ossola: Vom 10. September bis zum 23. Oktober 1944*. With a foreword by EDGAR ROSEN. (Veröffentlichungen des Institutes für Sozialgeschichte, Braunschweig.) [Hanover:] Verlag für Literatur und Zeitgeschehen. 1972. Pp. 165.

This little book, well-documented, sober, and lucid, is a valuable contribution to the growing literature enabling post-World War II generations to understand the wartime European Resistance. The Alpine valley of Ossola (or Valdossola), wedged on two sides between Swiss territory, was one of several areas in German-occupied northern Italy that was liberated for a brief period by partisans in 1944 and in which an antifascist provisional government successfully ensured the functioning of public administration. As evidenced by hundreds of footnotes and by a six-page bibliography, the author has diligently collected from Italian (both partisan and fascist), German, and Swiss sources the information available on Ossola during the few weeks in September and October between the formal establishment of a five-party coalition junta, which recognized the authority of the Italian government in Rome and cooperated with the Allies, and the German reoccupation of the area. Although the author describes the composite miniscule partisan army (three to four thousand men) and its poor armament and summarizes the major phases of the German-fascist offensive, his main interest is not military but civilian: the structure of the administration and the policies of the junta concerning law and order, the judiciary, the schools, local finances, and commercial exchanges with Switzerland—the only source of supplies (several Swiss citizens helped the junta). The author rightly sees in the “republic” the expression of what—rhetoric apart—the Resistance generally wanted to achieve politically: democracy as defined by Lincoln. The experiences in Ossola and the other “republics” prepared the ground for the democratic republican constitution of 1947. The book is valuable in another aspect: the distinction between Italians and Germans is unimportant; what matters is the conflict be-

tween democracy and dictatorship. It is a pleasure to see postwar scholars transcending the narrow nationalism that until now has prevented Europeans from seeing the influence one nation has had on the others and that has been a major source of distortions.

M. SALVADORI

Smith College

VLAD GEORGESCU. *Ideile politice și iluminismul în principatele române, 1750–1831* [Political Ideas and the Enlightenment in the Romanian Principalities, 1750–1831]. (Academia de Științe Sociale și Politice a Republicii Socialiste România. Institutul de Studii Sud-Est Europene. Biblioteca Istorică 32.) Bucharest: Editura Academiei Republicii Socialiste România. 1972. Pp. 226. Lei 14.50.

VLAD GEORGESCU. *Political Ideas and the Enlightenment in the Romanian Principalities (1750–1831)*. (East European Monographs, 1.) Boulder, Colo.: East European Quarterly; distrib. by Columbia University Press, New York. 1971. Pp. 232. \$7.50.

In recent years the place of the Romanians in the eighteenth-century European Enlightenment has been the subject of many scholarly Romanian works. Generally, the emphasis has been upon intellectual and literary influences. The book here under review explores an aspect—political thought—that has hitherto lacked a synthetic treatment based upon a thorough investigation of contemporary writings. Georgescu has now to some extent filled that gap. He has examined the works of over seventy authors and in the process has rescued a number of them and their works from undeserved obscurity.

The first section of the book sketches the social and political background of the period, and the second offers a composite portrait of the intellectuals and describes the sources of their ideas. Two themes constantly recur: the influence of the Western European Enlightenment on Romanian social and political thought and the nature of the so-called Phanariot regime and its effects upon the intellectual and cultural life of the principalities. The author's treatment of both is incomplete and at times vague. For example, the great problems of the Enlightenment (and of the post-Enlightenment period as well) are described in too cursory a manner to enable the reader to place

the works of Romanian intellectuals in proper perspective. It is also difficult to share the author's condemnation of the Phanariot period as decadent because, as he asserts, it interrupted the "natural course" of development of cultural and intellectual life in the principalities, which was gradually turning toward the West. Georgescu does not make a convincing case. To do so, he must show that indeed such was the trend of Romanian development, a task that requires, among other things, an examination of the importance of Orthodoxy (in the broad meaning of the term) and an analysis of the Greek world of the period.

The best part of the book is the third and final section where the author analyzes the thought of Romanian intellectuals topically and in detail. He stresses the fact that Romanian political thinkers during the period were little given to theorizing but were instead concerned with the practical political problems confronting Moldavia and Wallachia. As a result, their writings deal mainly, though by no means exclusively, with the international situation of the principalities, especially their relationship to the suzerain Ottoman Empire, and with their internal political organization. Of interest also is the short chapter devoted to their ideas about the nature and evolution of human society. A short introductory essay on the historiography of the problems treated and a lengthy, but unannotated, bibliography are useful tools for further research.

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VĚRA OLIVOVÁ. *The Doomed Democracy: Czechoslovakia in a Disrupted Europe, 1914-38*. Translated by GEORGE THEINER. Montreal: McGill-Queen's University Press. 1972. Pp. 276 \$12.50.

JÍŘÍ PELIKÁN, edited and with an introduction by. *The Secret Vysočany Congress: Proceedings and Documents of the Extraordinary Fourteenth Congress of the Communist Party of Czechoslovakia, 22 August 1968*. Translated from the Czech by GEORGE THEINER and DERYCK VINEY. New York: St. Martin's Press. 1972. Pp. vi, 303. \$11.95.

GALIA GOLAN. *The Czechoslovak Reform Movement: Communism in Crisis, 1962-1968*. (Soviet and East European Studies.) New York: Cam-

bridge University Press. 1971. Pp. viii, 349. \$16.50.

Few periods in the recent past have already become so indisputably part of history as the 1968 Czechoslovak "spring." The Soviet intervention in August of that year cut the continuity of development, thus making the preceding events a fascinating, though tantalizing, object of inquiry. In different ways the three books reviewed here suggest three major clues about why this happened—or, in Marxist terms, was "bound" to happen.

Olivová's study is an interpretive survey of Europe's interwar history as seen from the author's Prague vantage point. This perspective is the source of both strength and weakness. On the positive side, what she says about events in Czechoslovakia is often enlightening for Western readers. Her account of them is original and reliable, the regrettable absence of source references notwithstanding. But Olivová is somewhat less than refined in relating Czechoslovak to general European problems—a defect only partly explicable by her insufficient access to Western literature. Indeed, the amount of the malice she imputes to the Right and of the virtue she attributes to the Left makes her writing occasionally reminiscent of history "as little Moritz imagines it." Thus, for example, Hitler's intervention in the Spanish Civil War allegedly served to clear the way for his advance into Africa and Asia (p. 213). And Czechoslovakia's prewar alliances with France and Russia were supposedly formidable because they enjoyed the support of the Communists (p. 172).

Yet too exacting academic standards should not perhaps be applied to a book that is, above all, a passionate confession of faith—that almost mystical Eastern European faith in traditions. The author's two heroes symbolize two traditions that inspired the Czechoslovak Communist reformers. One is Masaryk, the nation's first president and for most Czechs the embodiment of their democracy, the other the Soviet Union, for most Marxists still the epitome of the best in communism. The co-existence of these two strange bedfellows on the pages of Olivová's book is symptomatic of the reformers' self-delusive ambition to reconcile democracy with their devotion to the

"fatherland of socialism"—the ambition that caused them to misjudge so disastrously Moscow's intentions in August 1968.

The volume of documentation about the secret party congress, which met in the wake of the catastrophe in a Prague factory under the noses of the unsuspecting Russian invaders, is somewhat disappointing. Contrary to the assertion on the flip cover, it contains little of "pivotal significance." But it would really be unfair to expect that any such problems could have been discussed and disposed of at a meeting under such extraordinary circumstances. It was enough that the congress met at all and confirmed a reformist majority in the central committee. Otherwise, the fundamental discussions had actually preceded it, and their results were merely summarized in some of the documents prepared for the occasion.

Both these documents and the proceedings give glimpses of a critical problem that faced the party—that of overcoming its disreputable past, for the reform movement had brought to light all too many embarrassing misdeeds, which, to be sure, had been abetted also by quite a few of the reformers themselves in their earlier days. Although the new leadership displayed an unprecedented candor, it still lacked the nerve and perhaps even the ability to face the whole truth and its probable political consequences. On the one hand, for example, a special report to the congress admitted that Czechoslovakia in the late 1950s had been conspicuously less inclined to exploit the opportunities for reform than other Communist nations. On the other hand, however, the same report glossed over the sordid circumstances of the Czech Communists' rise to power in 1948—circumstances likely to account for some of the later deficiencies in their moral fiber.

The third book is by an Israeli author—the citizen of a country whose political and intellectual elite has particularly strong links with Czechoslovakia. A political analyst rather than a historian, Golan used superb documentation including a variety of Czechoslovak press and radio reports monitored in the West. Hers is a sympathetic yet unsentimental account, respectful of the hard facts and free of dubious generalizations. Unlike Vladimir V. Kusin, the

author of the best study on Czechoslovakia's intellectual ferment, Golan focuses upon the equally momentous institutional and organizational changes. She covers in systematic detail the time until January 1968 and adds a sketchy epilogue on the subsequent events, which she proposes to discuss in a separate book. The copious material is well organized in both chronological and topical fashion. More than most authors—who overemphasize the Czech character of the reform movement—Golan gives weight to its less ideological, but equally dynamic, Slovak component.

Golan explains the dramatic developments that eventually culminated in the unique attempt to abolish the authoritarian structure of the Communist party itself and end its monopoly of government. This innovation, utterly alarming from the Soviet point of view, was a third factor that—besides the reformers' ideologically conditioned misreading of Moscow's intent and the debilitating heritage of their own past—forebode their doom. With hindsight, it is difficult to imagine how they could have acted otherwise. The element of predetermination in their actions makes the three books melancholy but nonetheless rewarding reading.

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WOLF ZEEV RABINOWITSCH. *Lithuanian Hasidism*. Foreword by SIMON DUBNOW. New York: Schocken Books. 1971. Pp. xiii, 263. \$7.00.

Hasidism is a Jewish religious movement that began in the 1730s and, like the revivalist trends of Protestant Europe and the Great Awakening in America, strove to make religion warm and emotional and to create "a religion of the heart" with an underlying mysticism but a decline in normative behavior. In so doing it opposed formalism and challenged some of the established institutions. The leader around whom the followers congregated was the illuminate, the charismatic personality who found his way to God.

Hasidism's development started in southern Poland (Podolia and Volhynia), where, among other things, some splinters of the Schismatics (*Raskol*) of the Russian Greek Orthodox Church had sought refuge from persecution.



Some, mostly external, similarities are discernible between these sects and Hasidism. When the latter penetrated into Lithuania and White Russia in the 1760s, it encountered sharp opposition and persecution that was to last for over a generation from the established Jewish communities.

Hasidism subsequently changed. Discarding much of its permissiveness it developed into the mainstay of orthodox Judaism in Eastern Europe. Leadership became hereditary with son following father, and there was division into dynasties. After World War II, offshoots of Hasidic groups that had survived Hitler and Stalin reached Israel and the United States, where they are trying to continue their way of life.

Dr. Rabinowitsch's book depicts the development of the various strands of Hasidism in Lithuania. (It is a translation of his work in Hebrew previously published in Israel.) The author, who discovered some unknown source materials, published a number of studies all of which he has utilized in this book. He tells about the beginnings of Hasidism in Lithuania and White Russia in the second half of the eighteenth century, the persecution of Hasidim between 1772 and 1800, and the growth of their groups over the years, and he includes English translations of some Hebrew documents. The book traces the development of the Hasidic "dynasties," charting genealogical tables of the leaders. The reader can find here a great many details about numerous Hasidic groups and dynasties, some of which stretched over periods of almost two centuries.

Such fragmentation into many small pieces of biographical information, however, greatly detracts from the book's worth. The modern reader is less interested in the biographical notes about over one hundred Hasidic leaders than in a synthetic picture of the movement and its main ideas and philosophy. It would also have been advisable to tell the story in comparative terms, with Western thought trends to facilitate understanding for those who are not familiar with Hebrew texts and esoteric thought.

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WAYNE S. VUCINICH, editor. *Russia and Asia: Essays on the Influence of Russia on the Asian Peoples.* (Hoover Institution Publications 107.) Stanford: Hoover Institution Press, Stanford University. 1972. Pp. xiv, 521. \$15.00.

The ten essays in this volume were read at a conference at Stanford University in 1967 on "The Russian Impact on Asia." The contributors examine the Russian view of Asia (N. V. Riasanovsky); Oriental studies in Russia and the Soviet Union (Richard N. Frye and Wayne S. Vucinich); and the impact of Russia on the European Muslims (Alexandre Bennigsen), Armenia (Vartan Gregorian), Georgia (David M. Lang), Central Asia (Manuel Sarkisyanz), the peoples of Siberia and the Far East (Stephen P. and Ethel Dunn), China (Mark Mancall), and Japan (George A. Lensen).

The list of participants is impressive, and the information imparted is wide-ranging, but the complex subject and manifold approaches make the contributions somewhat uneven. No attempt is made to define the task and the criteria involved. Kliuchevskii's distinction between "contact" and "influence," and the concepts of diffusion familiar to cultural anthropologists could have provided bases for such a standard. As it is, only Stephen and Ethel Dunn discuss methodology.

Without guidelines certain contributors dwell on diplomatic, political, and military events instead of getting down to the means and tempo of transmission, receptivity, and other matters closer to the conference theme. Sarkisyanz, for example, could have assumed more knowledge by his readers of the size and ethnic and cultural diversity of Central Asia and devoted more time to his theme of transformation and acculturation. For this he might have used Barthold's highly relevant *Istoriia kul'turnoi zhizni Turkestana* (Leningrad, 1927). He might also have cited several pertinent works that have appeared during the long period since the conference, by Elizabeth E. Bacon, Violet Conolly, or Teresa Rakowska-Harmstone. Sarkisyanz is the only one, however, who cites Walter Kolarz's keen analyses, based on Soviet sources, in *Russia and her Colonies* (1952) and *Peoples of the Soviet Far East* (1953), which were relevant to several of the essays.

The peoples discussed differ widely as regards Russian influence. Some have been under

Russian rule for centuries (the Kazan Tatars), some never; some are numerically insignificant (the Siberian peoples)—though interesting nonetheless—yet receive space equivalent to that allotted much larger groups, notably the Chinese. Some, like the Armenians and Georgians, are hardly “Asian” at all—there might as well have been an article on the influence of Russia on Israel. The Turks and Persians were discussed at the conference but are omitted from the book, perhaps because of negligible impact. Russian influence on some of the other peoples discussed has also been tenuous.

Thus, Mancall asserts that the Russian impact on China has been “tremendous,” but most of what he relates is political and diplomatic rather than cultural. Attempting to analyze this impact “within the context of particular configurations in the total paradigmatic perceptual scheme through which the Chinese viewed themselves and reality,” he states that Western European culture brought a perception of “diachronic conflict and discontinuity, the past versus the present, the old versus the new,” while the Russian influence introduced a “synchronic” element of class struggle within Chinese society. Chinese culture, however, has resisted outside influence since ancient times, Chinese scholars were conscious of the distinction between old and new, and the country has a long history of struggle between landlord and tenant, of peasant rebellions, and of secret societies. It would seem difficult to attribute the inculcation of such characteristics either to the West or to Russia.

Inevitably there was disagreement on certain points. Bennigsen alleges the Soviet policy of destroying the heritage of the past of minority peoples by changing alphabets, introducing Russian words into written language, and other manifestations of a policy of integration and eventual assimilation. Frye on the other hand dismisses any idea of the Soviet government forcing minorities to become Russianized. Sarkisyanz, although decrying those who see “the division of Russian Central Asia into ethnic territorial units as a mere Machiavellian device to split a ‘Turkestan’ allegedly constituting a natural unity,” notes nevertheless the Stalinist prohibition of economic associations among the several republics. Lang points out the material advance of Georgia under Soviet rule, which

could apply to most of the other republics.

The inherent shortcomings, however, of any conference of this nature and the predictable differences of opinion should not obscure the fact that this is an interesting and valuable book. Aside from their intended stress on Russian influence the essays provide a high-level survey of Russian history and policy toward minorities and neighboring peoples that will be useful for reference and as a source of ideas on a wide range of subjects. Skillful editing has minimized overlapping and differences in translation. There are abundant notes (pp. 369–465) and a satisfactory index.

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L. L. MURAV'eva. *Derevenskaia promyshlennost' tsentral'noi Rossii vtoroi poloviny XVII v.* [Rural Industry in Central Russia in the Second Half of the 17th Century]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 204.

This book is a detailed and comprehensive survey of the character and geographical distribution of village industry in the central regions of Russia during the second half of the seventeenth century. It is a valuable supplement to the work of Iu. V. Got'e on this topic, for the author exhausts all presently known sources and does not hesitate to list the names of hundreds of villages and settlements and the type of economic activity to be found in each. The author proceeds from a basic assumption of Soviet historiography—that in the second half of the seventeenth century there occurred in Russia an upswing in industrial activity, a more sophisticated division of labor, the creation of a national market for goods, and the beginnings of Russian capitalism. Murav'eva's contributions to this assumption are that much of this economic activity could be found in serf villages and that many peasant settlements resembled commercial towns in the character of their industrial production and the division of labor. The evidence is clear that such economic activity occurred in the villages of the seventeenth century. What is not proven by the author or by Soviet historiography in general is the assumption that there actually was an upswing in industrial production in that cen-

tury. One could, perhaps, more readily accept the arguments of D. P. Makovskii and Got'e, who hold that production at the end of the seventeenth century was no more advanced than in the sixteenth. Murav'eva states that village production in the seventeenth century became more common and typical of central Russia than it was in the sixteenth century, when it was, she says, more "sporadic." In fact, however, what is more typical of the seventeenth century is the availability of source materials, and what is typical of the sixteenth is the "sporadic" survival of records.

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J. G. PURVES and D. A. WEST, editors. *War and Society in Nineteenth Century Russian Empire: Selected Papers Presented in a Seminar Held at McGill University, 1969-1971*. Toronto: New Review Books. 1972. Pp. 188. \$7.50.

This volume of essays deals with the effect of Russian life and society upon the military system. In the opening article, J. G. Purves analyzes Russia's problems in expanding and modernizing her military establishment throughout the century. He holds that, in spite of costly efforts, it usually lagged because of technical, educational, and economic backwardness and the transportation problem. Hryhorii Fil' indicates that up to 1856, religion was effective in ensuring the loyalty and devotion of the army, but he does not discuss its efficacy against revolutionary influences affecting the army at the end of the century. D. Fattal finds that Bakunin, Kropotkin, and Tkachev believed that armies and wars were by-products of the state, which was the real enemy. This problem seems to require wider treatment.

Dr. West presents an administrative history of the military system under Alexander I during the frantic effort to centralize, modernize, and expand it to prepare for war with Napoleon—a subject that has had little previous attention. In the Ukraine, however, the government decentralized by urging the people to form troops to combat the French. Roman Serbyn believes that the people hoped thereby to recover their lost freedoms. Similarly, E. W. Laine, using Finnish material, shows that Nicholas and especially Alexander II treated the Finns mildly and won their loyalty. It was not until the

German danger appeared that the Russians tried to force military control on the Finns. P. Hidas, using Magyar sources, states that in 1849 the Russians had the sympathy of the Slavs and, thanks to the Russians' discipline and their kindness to the vanquished, of the Magyars. With the Austrians, however, they were often at odds.

Essays by M. McMullen and J. Soley on the role of British opinion and Palmerston in inciting and inflaming Russophobia during the Crimean War, while well done, offer little that is new. In "The Baltic during the Crimean War," J. Knoppers shows that British power could do little there, except to frighten the Russians into massing troops, which otherwise might have turned the tide in the Crimea.

O. Smal pictures the Slavophiles as believing that Russian Orthodoxy would convert the West. When the West backed the Muslim Turks, however, the Slavophiles urged the Balkan Slavs to rise and, when they did not, to put their trust in Russian arms. After the Crimean War, D. A. Miliutin—no Slavophile—modernized the army while urging peace in Europe, although he favored the less risky expansion in central Asia. According to Alexander Pidhainy, however, Miliutin, as Russia got in deeper and deeper, finally insisted that the army had to fight the Turks, although he warned that it was far from ready. How unready it was A. L. Smith makes all too clear, so that it is amazing that the army eventually won a full victory.

P. Spillberg in "The Nation in Arms" indicates that by the end of the century, Russia had no modern military doctrine but regarded the loyalty and bravery of the rural population as more important than firepower and technique. Finally, J. Desmarais gives a detailed and convincing account of the efforts of France to win Russia as an ally.

These essays are based on original research, and some make useful contributions, although it is doubtful whether the scope and nature of the work justify this form of publication. The editing is not always as careful as it should be.

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GEORGE KATKOV *et al.*, editors. *Russlands Aufbruch ins 20. Jahrhundert: Politik—Gesellschaft*

—*Kultur, 1894-1917*. Olten: Walter-Verlag. 1970. Pp. 347. 25.50 fr. S.

GEORGE KATKOV *et al.*, editors. *Russia Enters the Twentieth Century, 1894-1917*. New York: Schocken Books. 1971. Pp. 352. \$12.00.

For many years before 1933, German scholarship was in the forefront of Western study of Russian history and culture, profiting by and based on the long association between the two states. Perverted and debased for propaganda purposes during the twelve years of Nazi rule, German *Ostforschung* since 1945 has once again taken its place as an indispensable component of serious scholarship on Russia and its past. It is fortunate, indeed, that this should be the case, for German students of things Russian work in a tradition whose depth, duration, and richness lend to their work a solidity sometimes lacking in the writings of the scholars of other Western nations, including our own.

An encouraging sign of the growing recognition of these facts is the publication in excellent English translation of this work, a symposium whose preparation was facilitated by a grant from the International Documentation and Information Center (Interdoc), the Hague. The thirteen contributors include seven West German scholars (Oskar Anweiler, who writes on education, Hans Bräker on the Muslim revival in Russia, Helmut Dahm on philosophy, Erwin Oberländer on political parties, Lothar Schulz on constitutional law, Gerhard Simon on church, state, and society, and Karl C. Thalheim on economic development), three British scholars (Violet Conolly on the nationalities question, Michael Futrell, coauthor of the article on foreign policy, and Harry Willets on the agrarian problem), and three Russian émigré scholars (George Katkov, coauthor of the article on foreign policy, Nikolaus Poppe on the economic and cultural development of Siberia, and Elisabeth Stenbock-Fermor on literature). Together these scholars of varied background provide a broad and at times surprisingly detailed survey of Russian society and the Russian government as they moved toward their final rendezvous with war, revolution, and collapse.

In its general organization as well as in many of its individual contributions the volume is traditional and fact- rather than hypothesis-oriented, a characteristic that springs sharply into view if one contrasts it with a recently

published American symposium covering the same period (Theofanis George Stavrou, ed., *Russia Under the Last Tsar* [1969]). Whereas the basic theme of the American volume is the question of whether or not the Bolshevik Revolution was the inevitable response to the failure of the tsarist regime to solve its most pressing economic and political problems (industrialization and democratization), the authors of the German volume tend to avoid speculation, at most mildly suggesting the possibility that the tsarist regime might have kept going indefinitely, given a less calamitous succession of accidental and avoidable mischances. There is a need, however, for both approaches, and students of the period, including university lecturers, will find the conscientious and sober documentation of the German volume valuable.

While space limitations rule out consideration of each paper, mention should be made of several. Of particular note is Oskar Anweiler's treatment of education, a shortened version of the first three sections of his *Geschichte der Schule und Pädagogik in Russland vom Ende des Zarenreiches bis zum Beginn der Stalin-Ära* (1964). Also outstanding is Violet Conolly's study of the nationalities question, a field to which she has previously made noteworthy contributions. Harry Willets's analysis of the agrarian question reduces to relative clarity a highly complex topic. Of the more tradition-oriented papers I found Lothar Schulz's study of constitutional law particularly satisfying, in part because of its forthright assault on the influential but highly questionable view, dating back to Max Weber, that the Russian government after the 1905 Revolution was merely a "sham-constitutional" regime.

Strikingly absent from both the German and the American symposia, regrettably, is any recognition of the importance for this period of Russian achievements in the visual arts, music, ballet, and the theater—a glaring revelation of the predominantly word- and book-oriented viewpoint of many academic specialists. In both symposia the name of Sergei Diaghilev, the great impresario, makes only a single appearance, as sponsor of the literary-artistic journal, *Mir Iskusstva*. The great names of early twentieth-century Russian music—Rachmaninoff, Prokofiev, Scriabin, Stravinsky, and others—are completely lacking, as are those of such

outstanding painters as Kandinski, Vrubel, Repin, V. A. Serov, Roerich, Bakst, and Benois. Yet one of the basic problems posed by the Russian Revolution has not even been formulated, let alone solved, if one fails to realize that it took place at a time when Russian society, through its creative intelligentsia, was giving proof of unparalleled vitality.

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IVAN S. LUBACHKO. *Belorussia under Soviet Rule, 1917-1957*. [Lexington:] University Press of Kentucky. 1972. Pp. 219. \$10.00.

The paucity of English-language works on Belorussia should make this volume welcome. In thirteen brief chapters and a conclusion the author discusses Belorussia's pre-Soviet past and the growth of the national movement, the proclamation of national independence, the formation of the Belorussian Soviet Republic (after two Soviet governments had failed), the partition of the country between Poland and the Soviet Union from 1920 to 1939, and the course of developments in Soviet Belorussia during the 1920s—a period of relative freedom—as well as Stalin's repressive policies in the thirties and forties. The fate of Western Belorussia under Polish rule is discussed along with the reunion of Eastern and Western Belorussia in 1939. Separate chapters are devoted to the cruel German occupation of 1941-44 and to the wartime diplomacy that led to Belorussia's inclusion as a charter member of the United Nations.

Although Lubachko frequently cites the late Nicholas Vakar's *Belorussia: The Making of a Nation* (1956) he has also utilized a wide variety of Soviet Belorussian and Russian sources, emigré publications as well as some interview data. Yet there are certain lacunae. There is little biographical information on individual leaders, whether Communist or nationalist, and they remain names rather than personalities. There is little discussion of the origins of the abortive Soviet effort in 1919 to merge Belorussia and Lithuania in the so-called Litbel. The 1945-57 period is given relatively little attention. Considerable emphasis is given to general aspects of Soviet policy such as the origins of Stalin's nationality policy, collectivization, industrialization, and the purges, but it is pre-

sented in the context of the Belorussian specific. Unfortunately, there remains a lack of Western studies on Soviet Belorussia comparable to the detailed studies of the Ukrainian Soviet Republic by Yaroslav Bilinsky and Hryhory Kostiuk.

Within the limits set for himself, however, Lubachko has written a very useful work. One of its attributes is that it is written from a Belorussian point of view, which simply means that the author documents the costs exacted by Moscow in political and cultural terms. Lubachko's work merits a place among the growing number of specialized studies dealing with the Soviet nationality problem.

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#### NEAR EAST

*The Near East in Modern Times*. Volume 1, *The Ottoman Empire and the Balkan States to 1900*, by GEORGE G. ARNAKIS; volume 2, *Forty Crucial Years, 1900-1940*, by GEORGE G. ARNAKIS and WAYNE S. VUCINICH. Austin: Pemberton Press. 1969; 1972. Pp. xv, 452; 356. \$12.50 each.

What makes these volumes different from other textbooks and general surveys on the Balkans or the Middle East is the authors' belief that these two areas should not be considered autonomous fields of study but rather should be conceived as a unity. In his preface to volume 1 Professor Arnakis tells us that one of the fundamental premises upon which he bases his study is the concept of a unified approach to what, in traditional terminology, has been referred to as "the Near East." The Near East he defines as the land formed by the converging extremities of the three continents of the Old World—Europe, Asia, and Africa—an area that can be said to fall into six divisions geographically: the Balkan Peninsula, Asia Minor, the Fertile Crescent, the Nile-land, the Arabian Peninsula, and the Iranian Plateau. While Arnakis's geography may not be acceptable to everyone his decision to move away from specialization and survey the larger area seems not only easily defensible but also clearly a move in the right direction. For despite their complexity and diversity the Middle East and the Balkans have often, in the past, formed a historical unit. This was certainly the case to a

greater or lesser degree with the Persian, Alexandrine, Roman, and Byzantine Empires in ancient and medieval times.

But, most recently, it was the Ottoman Turks who held dominion over the whole area for centuries. It is, therefore, on the Ottoman Empire that Arnakis focuses after a brief survey of the Near Eastern peoples, their history, and their conflicting religious heritages. (In this latter connection he sees the contest between Byzantines and Arabs as the "greatest and most protracted culture conflict in recorded time" [p. 47].) Nevertheless, in telling the story of the origins, expansion, institutions, and decline of the Ottomans from the fourteenth to the late eighteenth centuries Arnakis does not seem to have made much use of the latest research of both Western and Turkish historians of the Ottoman Empire. Thus, the "Lybyer thesis," now generally considered outdated and an oversimplification of the facts, is still relied upon to explain the dynamics of the Ottoman ruling elite (p. 68). Similarly the judgment that Bajazet II (1481-1512) was "unequal to the task of keeping the Ottoman Empire together" (p. 63) does not find acceptance today. As for the causes of decline it is no longer sufficient to attribute them primarily to excesses of militarism, favoritism, and corruption without reference to long-range causes such as population growth, inflation, and geographic and logistical limitations to the deployment of Ottoman military power.

Still, most of the first volume—three-fourths of its contents—deals with the emerging nationalisms in the Balkans where the Ottoman Empire first began to disintegrate. And this theme, the basic one of the study, is treated thoroughly. Arnakis provides us with competent accounts of the development of the Greek, Romanian, South Slavic, and Bulgarian national movements in the late eighteenth and early nineteenth centuries that are skillfully interwoven with the complex strands of Ottoman reform and great power diplomacy on the Eastern Question. On the other hand, with the exception of a chapter on Mohammed Ali of Egypt and the European powers, only passing reference is made to conditions in the Arab provinces of the Empire during this time.

In this respect volume 2 is much more soundly balanced. After a detailed examination of the

cataclysmic events leading to the dissolution of the Ottoman Empire and the birth of the Republic of Turkey, developments in Iran, Syria, Lebanon, Iraq, Transjordan, the Arabian Peninsula, Egypt, the Sudan, and Palestine are given as careful attention as those in the Balkans. (The chapters on the Balkan states, with the exception of Greece, are contributed by Professor Vucinich.) There is also a satisfying balance between internal political events, international rivalries, and economic and social conditions. Convenient summaries of cultural, artistic, and intellectual accomplishments in each country are also included.

What emerges from a study of the 1900-40 period as a whole is that the ideology of nationalism, while decisive in bringing about the downfall of the Ottoman Empire, failed to provide answers to the tough economic and social problems faced by the newly independent or mandated states of the Near East—problems aggravated by the Great Depression. In fact, as Arnakis points out, it became fashionable for economists to deplore the collapse of the huge multinational Ottoman and Habsburg Empires that were, in their eyes, much more viable geopolitical entities. Middle-class leadership proved largely ineffective in the Balkans, except for brief periods of beneficial rule in Greece and Romania in the second and third decades of this century. In the Middle East its energies were absorbed by the struggle to shake off British and French control. In both areas dictatorships became the general rule. By the late 1930s this was the case "all the way from the Danube to the Nile and from the Adriatic Sea to the Indian Ocean" (p. 295). The most successful of all these Near Eastern dictators, according to Arnakis, was Mustafa Kemal of Turkey who labored systematically and effectively for the creation of a Westernized, progressive bourgeoisie in his country. Ataturk's accomplishments constitute "the greatest bloodless revolution in the annals of Islam" (p. 295).

Like most general surveys these volumes are not models of readability, although the style is clear enough. And, like other such efforts, it has its errors. Some are seriously misleading—for example, the Persians and the majority of the inhabitants of Iraq did not choose the Shi'a form of Islam after the assassination of Ali (vol. 1, p. 9), but centuries later—while others

are mere slips: the Kurds did not rebel against the Turkish authorities in the summer of 1929 but in 1930 (vol. 2, p. 81).

The usefulness of both volumes is enhanced by such aids as a glossary of terms, a chronology, dynastic tables, and a select bibliography of works available in English. A third volume, *Second World War and after 1940-1960*, has not yet appeared.

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M. K. ZULALIAI. *Armenia v pervoi polovine XVI v.* [Armenia in the First Half of the 16th Century]. (Akademiia Nauk SSSR, Institut Vostokovedeniia; Akademiia Nauk Armianskoi SSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 124.

This is a brief analysis of western Armenia, or the eastern provinces of the Ottoman Empire, in the first half of the sixteenth century. The author first describes the social and economic structure of these provinces and then discusses the effects of the Ottoman-Safavid conflicts on the Armenian regions. In the first and more important of the sections the author bases his account mainly on Armenian and other contemporary chronicles, although he uses those published primary Ottoman sources available to Soviet scholars. *Kanun-names*, edited by Omer Barkan, chronicles, and the travelogues of Evliya Çelebi add greatly to the author's evidence. Nevertheless the picture given of the different classes and their interrelationships, especially between Muslim and Armenian, is a contradictory one. Armenian chronicles speak of Muslim tyranny; foreign travelers' accounts and that of Evliya Çelebi point out that the lucrative East-West trade, which had not yet diminished by 1550, was in the hands of "wealthy Armenian merchants," while the Ottoman legal documents fill in the intentions, if not the practice, of the ruling classes as manifested in tax laws, privileges, and the political structure. But what was social and economic life really like in these provinces? Which evidence should we accept? The author does not provide the answers, and with good reason, because until the Ottoman archives themselves are studied, along with records of local officials, the judiciary, and religious establishments, our knowledge of this or any other Ottoman province

will remain incomplete. However, this book is the best treatment to date of these Ottoman Armenian provinces in the sixteenth century.

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CARL MAX KORTEPETER. *Ottoman Imperialism during the Reformation: Europe and the Caucasus.* (New York University Studies in Near Eastern Civilization, number 5.) New York: New York University Press. 1972. Pp. xix, 278. \$14.50.

Organized on the scale of a Mongol campaign, this book has as its subject the history of the northeastern and eastern Ottoman frontiers from the end of Suleiman the Lawgiver's reign (1566) to the death of the Crimean khan, Gāzī Girāy (1608).

From his study of political affairs in the long frontier zone running from Hungary through the Crimean steppe to Azerbaijan, Kortepeter extracted a number of themes by which he seeks to explain the history of Ottoman imperialism. Three of these arguments are important. First, the establishment of Ottoman influence in most of Hungary owed its success, in part, to the Hungarian reaction to Habsburg and papal attempts to impose their control over local populations. Second, the semi-independent status of the Crimean khanate masked an imbalance of power on the steppe frontier resulting from the technological inferiority of Tatar arms. Finally, the urge toward the Black Sea of the Muscovites combined with the rise of Safavid power in Persia to threaten Ottoman connections with Central Asia.

Kortepeter, however, offers meager evidence on how these trends affected Ottoman policy. In his history of the Danubian frontier, for example, the campaigns of the Tatars receive more attention than does the study of why contrasting imperial policies brought most of Hungary under Ottoman rule. Granted that the Tatars did not adopt firearms with alacrity, still the record of their military effectiveness through the early eighteenth century makes one wonder whether or not technological inferiority was a major factor in the steppe politics of the late sixteenth century. More intriguing, however, is the unanswered question concerning the status of the khanate. Why did the Ottomans refrain from imposing direct con-

trol over the Tatars? Equally exciting is the geostrategic issue raised by the Russo-Safavid convergence on the Caucasus. But if this event represented a real danger to the Empire, then should not the 1577 decision to war against the Safavids be judged from a viewpoint other than that represented by court factions opposed to the campaign?

The undue concentration on Crimean affairs in this work draws attention away from the central event underlying imperial history in the age of Ġāzī Girāy (1588–1608)—the cessation of Ottoman expansion. Surely, the ending of approximately three centuries of Ottoman growth is connected with the reasons why the Ottomans were not able to impose direct administrative control over the turbulent border regions of Hungary, the Crimea, and the Caucasus.

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RICHARD W. BULLIET. *The Patricians of Nishapur: A Study in Medieval Islamic Social History*. (Harvard Middle Eastern Studies, 16.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xvi, 288. \$12.50.

This short, stimulating, and highly intelligent analysis of social life in Nishapur from the tenth to the twelfth centuries A.D. is an outstanding contribution to Iranian urban history. The first eighty pages define the composition, social motivation, education, and political influence of the city's ruling elite while the remainder of the book is devoted to a meticulously documented reconstruction of the leading families within that elite, their genealogies, marriages, occupations, and sources of income.

At the heart of Bulliet's study—and it is this that gives it so much of its importance—is his rigorous and, to my mind, wholly convincing definition of the ruling elite of Nishapur as a patriciate (pp. 20–27). This patriciate was recruited from three separate groups, with the *ulama* occupying pride of place but deriving additional economic support and political leverage from marriage alliances with the families of the wealthier merchants and of local landowners belonging to the former *dihqan* class. The *ulama* never constituted a closely knit, coherent group—there were subtle distinctions that it is now difficult to perceive clearly—but Bulliet

makes a persuasive case for regarding the right to teach (as opposed to the right to learn) as a measuring rod of social pre-eminence. "The point of control in the system was the determination of who was to get to teach. The certifying apparatus . . . was the very heart of Islamic education of that time" (p. 54). Since successive rulers were willing to allow such cities as Nishapur a considerable degree of autonomy the patriciate had little difficulty in maintaining its grip over local affairs with minimal interference from without or protest from within. Much of this is confirmed by what we know of other cities in the area—Heart, in particular—but it is not the least of Bulliet's virtues that he insists that what he is saying relates specifically to Nishapur and not to the cities of Khurasan as a whole, while nowhere does he offer his readers a "model" of the traditional Islamic city.

Like most scholars who have investigated the social structure of the Iranian city in the early medieval (i.e., pre-Mongol) period Bulliet is clearly baffled by the role of the *madhhab* in the furious faction-fighting that ultimately destroyed the city in 1162; it is indeed difficult to know what to make of these violent and seemingly irrelevant confrontations. Probably similar studies of other major centers such as Tus, Ray, or Isfahan will assist in elucidating the problem as well as confirming *The Patricians of Nishapur* as a scholarly contribution of truly seminal influence.

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## AFRICA

JAMIL M. ABUN-NASR. *A History of the Maghrib*. New York: Cambridge University Press. 1971. Pp. x, 416. \$14.50.

This general history is an impressive survey of the Maghrib between the time of the Carthaginian commercial empire and the independence of Libya, Morocco, Tunisia, and Algeria in the mid-twentieth century. Directed toward a university audience and based upon the secondary literature in French, English, and Arabic, with an occasional dipping into Arabic sources to verify fact and interpretation, the book is in several respects unique.

Professor Abun-Nasr possesses special insight



into Islam, as well as what it means to be Arab or Berber. His view of motive and action is therefore subtly different from the general works of Western scholars such as Julien, Barbour, Hahn, or Le Tourneau, and adds a new dimension. He also valiantly incorporates Libya into the history of the Maghrib without losing sight of the long-standing connection with Spain. From the Carthaginian through the Ottoman periods the focus is basically political. Abun-Nasr, however, makes an effort to mention major economic and cultural questions within the religio-political framework, but a four-hundred-page survey inevitably imposes limiting decisions. As a result it is reasonably certain that his audience of university students will in some cases lack the staying power to plow through the ever-changing fortunes of multifarious tribal leaders who come and go with the wind. This kind of experience can be of value, as any serious reader of Merovingian history knows, if the end result is intelligent generalization based upon analysis. Usually this quality is present.

For the modern historian the book's pace accelerates two-thirds along the way. By the nineteenth century the sources are much deeper. Further, the monographic work is far richer, and in this context the author skillfully uses the recent work of that small but strong group of American scholars—Ashford, Brown, Gordon, Halstead, Ling, Moore, Charles Stewart, not to omit Graham Stewart. This output, plus the special and general studies of the French and British researchers, gives rise to the thought that along with European occupation of the Maghrib—with its dynamism, ethnocentrism, and destruction of tradition—came an impressive amount of work that today furthers our understanding of the area. This book would suffer without it.

One refreshing characteristic deserving mention is that Abun-Nasr in his final two chapters, which are done in a historian's shorthand, draws his own original conclusions. Thus he considers indigenous nationalism well launched before World War II, and, as a result, he is unwilling to credit the expansion of that conflict to North Africa with any important contribution to independence. I find this view not entirely persuasive. Maghribian nationalism surely drove forward before 1939, but it is

true that the wartime preoccupations of the European (and Asian) powers catalyzed African nationalism. The emerging superpowers, the United States and the Soviet Union, stood on record against colonialism and for national self-determination. This body of opinion no doubt contained much cant, but it aided Maghribian nationalism to the extent that the idea became familiar. The defeat of France in 1940, also, cannot have been without importance, as was the defeat of Italy, for those people reaching toward independence. If Professor Abun-Nasr will look as deeply into recent European history as he has into the history of North Africa, possibly he might temper his judgment. His point is nevertheless well taken. It is consistent with the general message one might draw from the book: Maghribian history is long and complex. Determinists, therefore, should exercise great restraint in pushing their dogma.

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CHARLOTTE A. QUINN. *Mandingo Kingdoms of the Senegambia: Traditionalism, Islam, and European Expansion*. Evanston: Northwestern University Press. 1972. Pp. xxiii, 211. \$10.00.

Based upon a thorough illumination of relevant British and French archives and upon the judicious use of oral data collected in the Gambia this study provides a first highly readable and substantial synthesis of the structure and evolution of Gambian society in the nineteenth century. A description of the pluralistic society of Senegambia precedes histories of the north and south bank states that carefully document their internal structure and the tensions that evolved until open conflict occurred in the 1860s. A chapter on European settlements traces the development of trade interlocking the European and African communities in Bathurst while pointing out that neither the Colonial Office nor local officials understood the realities of the Gambian situation.

Political authority in the Gambia, if greatly decentralized, was the exclusive preserve of the Soninke aristocracy. Civil and military authority were divided; *jawara* generals were elected only for the duration of specific conflicts. As the author shows, frustrations were

keenly felt by a new class of people who in the nineteenth century had achieved wealth outside the traditional clan structure through trade and cultivation and export of groundnuts. Members of this new class were often from the Marabout communities, where people had settled because of occupational and religious interests rather than because of clan ties. Represented in central councils at a lower rank than were men of comparable Soninke title, the Marabout leaders formed an "Islamic shadow state" whose economic power far outweighed its access to political authority.

The jihad inspired and led by Maba Diakhrou Islamized the population of the Senegambia and shattered the power of the Mandingo aristocracy. In the second half of the book the author emphasizes the "social revolutionary aspects of the jihad" and studies in further detail the "sharp conflicts which emerged as clan strata began to grow into broader social classes." The "secularization" of the jihad and intense European rivalries—commercial at mid-century but by the 1890s exploding into political and military competition between British and French that resulted in the imposition of colonial rule—were important factors in preventing the existence of an enduring Islamic state.

The author delineates similarities between the Gambian jihad and other nineteenth-century Islamic reform movements, and the discussion of the "secularization" of the jihad raises some interesting questions about differences in the dynamics of post-jihad periods. Dr. Quinn explains the "secularization" by a variety of reasons: the early loss of Maba's religious example; ambivalence toward leadership in Sufi thought and in Maba's own reticence to occupy himself with statebuilding; the traditional Marabout distrust of secular authority; the Mandingo resistance to Wolof expansion of which Maba seemed an example because of his alliance with Lat Dior and other northern contacts; and the European opposition that precluded efforts to consolidate military gains. In Hausaland the potential threat of a Hausa backlash against Fulani expansion did not occur nor did the early withdrawal from political life of Shehu Uthman dan Fodio lead to the collapse of the Sokoto Empire. He and his chief lieutenants, Mohammed Bello and

Abdullahi, had long occupied themselves with the proper form an Islamic government should take. Maba did not and his successors less so on the basis of present evidence. Further investigation of Maba's ideas and the Arabic literature and correspondence of the jihad might clarify why the only man who seriously attempted to unify the African powers of the Senegambia seemed to withdraw from these problems. The hierarchical and centralized political and administrative institutions of pre-jihad Hausaland, which enabled the Fulani to assume control without utterly destroying the political fabric of society, were absent in the Gambia. Despite the "Islamic shadow state" and their real economic preponderance (which the Fulani in Nigeria did not have on their side) the Marabouts were unable to overcome the centrifugal tendencies of Gambian political institutions. The new entrepreneurial class, whose emergence during the first half of the century was an important theme in Gambian developments, disappears in the post-jihad era. A future examination of the relationship between it and the Soninke and Marabout leaders during the last thirty years of the century will be useful.

These themes of religious reform, secularization, and the emergence of social classes are only three examples among others which illustrate that this book will be welcomed both for its sensitive groundbreaking study of the nineteenth-century Senegambia and for its contributions to comparative historical work on economic and social as well as political change.

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T. N. TAMUNO. *The Evolution of the Nigerian State: The Southern Phase, 1898-1914*. (Ibadan History Series.) [New York:] Humanities Press. 1972. Pp. xvi, 422. \$12.50.

KENNETH W. J. POST and GEORGE D. JENKINS. *The Price of Liberty: Personality and Politics in Colonial Nigeria*. (African Series Studies, number 7.) New York: Cambridge University Press. 1973. Pp. xii, 494. \$27.50.

These two volumes are curiously complementary. Each deals with a discrete period of Nigerian history, each is concerned with aspects of the creation of the modern Nigerian state,

and, in their different ways, each recounts in detail events that led to crucial stages in Nigerian political development.

Mr. Tamuno takes a fresh look at the rise of the Nigerian state between 1898 and 1914. The book is an administrative history covering in detail the relationship between the colonial administration and the indigenous authorities as British rule was firmly established in southern Nigeria. The author's careful research into the records of the period serves to illumine many aspects of colonial policy, and, in particular, the events leading up to amalgamation.

The main outlines of the history of this period are, of course, well known, but the unique quality of Mr. Tamuno's work is that the narrative of events during the period is seen not from the vantage point of a British defender or attacker of imperial policy, but through African eyes. The role of the leading African political figures of the time is delineated more clearly than in previous histories, and even the prominent personalities of the colonial administrators emerge in somewhat different perspective. The author's comment that "between 1909 and 1913 the British authorities lost a valuable opportunity of making, and gaining from, detailed investigations into the institutions of the people whom they hardly understood and who scarcely understood them" points up a fundamental misconception of the years of indirect rule that followed World War I. Many administrative officers who felt certain that they understood "their Africans" in fact comprehended and appreciated little of the culture of the people over whom they held authority. Mr. Tamuno's work adds a new dimension to historical interpretation of the period he describes, and it is to be hoped that he will see fit to extend his research to the period between the wars.

The Post and Jenkins volume treats much more contemporary Nigerian history. It is a biography of one of the more fascinating figures among the nationalists of the preindependence years, Adegoke Adelabu. The story of Adelabu's rise to political influence and power in Western Nigeria is not only of interest because of the personality of the man, but because the perplexities and frustrations he suffered were, in a real sense, those of many in his time and, more particularly, those of his contemporaries

who entered the political arena at the point of violent eruption of party contention in the Western Region. I can attest from personal experience to the fact that Adelabu was indeed the complex figure portrayed by the authors. He sought power and wealth, and he did not hesitate to use his charisma for his own ends. Yet, at the same time, he was one of the relatively few politicians of the period who retained, in the face of the multiple countercurrents of shifting political loyalties, a vision of the destiny of a united Nigeria. His sudden and tragic death in a motor accident on the treacherous Lagos-Ibadan road robbed Western Nigeria of a figure whose hold on the masses of Ibadan might well have been extended, had the time been given him, to Nigeria as a whole.

This book is not one for the beginner in Nigerian politics; the references are often obscure and unexplained. The detailed account of events is often unnecessary to the main theme and of interest primarily to those who knew Nigeria at this period. The labor of research that went into the writing is obviously prodigious, and, indeed, as the authors imply in their preface, the book is to some extent a labor of love. But these caveats notwithstanding, to trace Adelabu's career is to trace the political history not only of a man but of a community during the feverish years of party activity. Adelabu was not always an attractive figure. His idealism was offset by a ruthless pursuit of ego satisfaction; the successful politician did not signal the emergence of the statesman. But in this he differed little from those with whom he did battle. What set him apart was his energy, his ambition, and his political skills combined with a strain of idealism. The authors have succeeded in creating a vivid portrait of him in the context of the brief time he occupied the political stage. For this they are to be congratulated.

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A. H. M. KIRK-GREENE. *Crisis and Conflict in Nigeria: A Documentary Sourcebook, 1966-1969*. Volume 1, January 1966-July 1967; volume 2, July 1967-January 1970. New York: Oxford University Press. 1971. Pp. xiv, 459; vi, 528. \$13.75; \$17.00.

Of only a few works can one genuinely say that they are truly indispensable for the study of their subject, that they are the place to begin. This is one, and it is a remarkable achievement on many grounds. It shows not only an extraordinary amount of careful work, but work under difficult conditions. Kirk-Greene had to deal detachedly with crisis and civil war in a country where people and events mattered to him a lot, for he had spent some twenty years as an administrator and academic in northern Nigeria. It was not work that could be put off to some other time, partly because much material could simply disappear—some into the airwaves, some into the decay that humidity brings, some as casualties of the war itself.

The primary part of the work is more than a collection of 227 documents of the usual type. These are not simply standard items. There are speeches where different versions of texts had to be sorted out, transcripts of elusive radio broadcasts, and statements collected from many locales on three continents and culled sometimes from nearly inaccessible newspaper accounts. There are even facsimiles of ephemeral leaflets and posters that circulated only briefly but could in crisis matter more than any formal speech.

Not that the crucial official documents are omitted; they are all there, and some, most strikingly the Special Branch report on the army coup of January 15, 1966, are particularly important and available nowhere else, even to the avid researcher. Indeed, Kirk-Greene's material on that coup, including Major Nzeogwu's radio broadcast and television interview right after it, are especially valuable, since evidence for the major's views and plans is sparse and likely to remain so.

These volumes make a further contribution. Explicitly designed as a "history towards a history," a guide to fuller research as well as a source book for that research, they present the first full narrative account of that history in long introductions of over one hundred pages per volume. In these essays the author makes no attempt to conceal his point of view. His years in the north particularly affect his presentation of events during 1966 and 1967, especially his assessments of the military coups and their aftermaths. His approach to the

conflict itself can be described, if too simply, as profederal. Two points are salient for him: he believed that the unity of Nigeria should be preserved; he did not believe that the federal government practiced—or intended—genocide.

Fortunately the overelaborate use of metaphor and alliteration with which his presentation begins gives way in the second volume to an unencumbered narrative, more even-handed in pointing out in footnotes the inconsistencies and ambiguities in both Nigerian and Biafran government positions. These introductions serve also as a way for the reader actually to use the documents themselves, for the volumes are, alas, without an index. More irritating is the absence of a list of documents in volume 1; the reader must apparently have both volumes in hand at all times. The end of volume 2 also contains the extensive annotated bibliography, so important for further researches.

Kirk-Greene's subject has now begun to fall into some perspective. The charges and counter-charges have receded. There was no genocide, and the postwar reconstruction and reconciliation have been profoundly impressive, even for those whose faith in Nigeria and Nigerians (not excluding Ibo) was greatest. Historians must resist the temptation to read backward here as elsewhere, of course, and for that reason, among others, the contemporaneity of Kirk-Greene's work adds to its value.

We have not had the expected spate of post-war accounts, but the most important of them (John de St. Jorre, *The Brothers' War* [1972]) acknowledges a debt to Kirk-Greene that others will share. One of the first analyses based on experience inside the Biafran enclave (Arthus Nwankwo, *Nigeria: The Challenge of Biafra* [1972]) offers illuminating insights, the more so when the reader complements them with the documentary context of Kirk-Greene's volumes.

Though most Nigerians—Ibo and non-Ibo alike—have put the war firmly behind them, many of the challenges remain. An African leader talked in 1969 of "the immense significance for the rest of Africa of the Nigerian experiment. . . . Had Nigeria succeeded (and Nigeria still can succeed . . .) . . . we would be able to say: 'Within Nigeria there are several peoples, each conscious of itself and

conscious of its ability to be a nation on its own. If they have nevertheless succeeded in submerging their natural unity into a larger artificial unity, for the greater benefit of them all, then the rest of Africa can submerge its smaller artificial units into that greater artificiality (indeed that more natural unit of all Africa) which holds greater promise for all the peoples of Africa" (vol. 2, p. 436).

It is paradoxical that "Nigeria's real significance to Africa" should have been so clearly stated by President Nyerere of Tanzania, in explaining his country's decision to recognize Biafra. Kirk-Greene's work will help many make sense out of such paradoxes.

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#### ASIA AND THE EAST

RASHĪD AL-DĪN. *The Successors of Genghis Khan*. Translated from the Persian by JOHN ANDREW BOYLE. (Persian Heritage Series.) New York: Columbia University Press. 1971. Pp. xi, 372. \$12.50.

This is a primary source for Mongol history, long known to Orientalists and Central Asian specialists and now available in English to world historians. "Our chief authority on the origins of the Mongols and the rise of the Mongol World Empire" (p. 8), this work constitutes the second volume of the *Ta'rikh-i Ghāzānī*, according to the "convenient division into three separate volumes, first proposed by E. G. Browne in 1908, . . . adopted by the Russians in their recent editions and translations of the Persian text" (p. 9). The first volume deals with the different Turkish and Mongol tribes (pt. 1) and Genghis Khan and his ancestors (pt. 2), while the third volume covers the Il-khans of Persia, down to the Il-khan Ghazan.

This *Ta'rikh-i Ghāzānī* is itself volume 1 of Rashīd al-Dīn's magnum opus, the *Jāmi' al-Tawārikh*, volume 2 of which, commissioned by Ghazan's successor, Öljeitü, is the first of the universal histories, dealing with all Eurasian peoples with whom the Mongols had come in contact—the Persians, Arabs, Turks, Jews, Franks, and Indians.

This work on Genghis Khan and his suc-

cessors is therefore but a small part of Rashīd al-Dīn's total contribution to Eurasian history, howbeit very important. Covering all those successors from the Great Khan Ögedei (1229–41), third son of Genghis Khan, down to Qubilai's grandson, Temür Öljeitü (1294–1307), Rashīd al-Dīn discusses each ruler under three categories: his wives, sons, and descendants; the details of his life and reign; and anecdotes illustrating his character, sayings, and miscellaneous relevant information.

Professor Boyle, the translator, with the passing of Vladimir Minorsky, is in the forefront of English-speaking authorities on Mongol history, but particularly on its major Persian sources. High standards of editing and translation would be expected from his publication, in two volumes, of Juvaini's *History of the World-Conqueror* (1958). Here his documentary apparatus and interpretive footnotes throughout the translation are evidence of his thorough research and illumination of the historiography of the Mongol period. All historians are indebted to him for this painstaking and perceptive rendition of one of the most important contemporary sources for Mongol and Central Asian history.

It remains to note that the volume contains useful appendixes: a good selective bibliography, four tables of Mongol dynasts, with dates and family relationships, and a fine detailed index.

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HUNG-MAO TIEN. *Government and Politics in Kuomintang China, 1927–1937*. Stanford: Stanford University Press. 1972. Pp. xii, 226. \$8.95.

Professor Tien's study examines the effectiveness of the Nationalist (Kuomintang) party-government in unifying and ruling China during the decade 1927–37. It is not so much a political history of this period as an analysis of institutional development directed to answering the question of the extent to which Kuomintang (KMT) party and government institutions penetrated Chinese society. He concludes that the factional politics characteristic of the Kuomintang, which permitted the personal dominance of Chiang Kai-shek, were an obstacle to gaining genuine political power be-

cause efficiency and administrative ability were often incompatible with loyalty to the party-government leader. Chiang firmly controlled the Kuomintang, but the KMT had only a narrow power base and was compelled to compromise with regional (militarist) interests and conservative social groups. The central administrative bureaucracy was weak and unable to subordinate the provincial administrative machinery to the goal of national development. Such partial control as the center achieved over the provinces was obtained largely by military means. It was, as a consequence, a fragile and superficial unity not founded on the elaboration of viable political institutions. The first decade of Kuomintang rule saw little real nation-building and an almost complete neglect of social reform. By the time war with Japan came in 1937, the eventual disastrous fate of the Kuomintang was already sealed.

To make his case the author describes in turn the failure to develop a new administrative bureaucracy at the center, the hollowness of the formal KMT party organization, the increasing dominance of military interests, competing political factions in the KMT party-government, the pattern of revenues and expenditures of both the central and provincial governments, and the general inability of the civilian party and government to penetrate and control the provincial administration in contrast to the very large role played by Chiang's military power.

I have no quarrel with Professor Tien's conclusions; in fact, most students of China in the 1930s would be in agreement. But our acceptance of this viewpoint, I must candidly state, has to date been based less on scholarly research on the decade 1927-37 than on our knowledge of how the next installments in the serial story of the Kuomintang turned out. Unfortunately, Professor Tien's study only marginally contributes to filling the scholarly gap. He states, and I acknowledge, the unavailability of the Kuomintang archives, but he has barely touched the rich contemporary periodical and newspaper sources. The two chapters on central and provincial revenue and expenditure contain little new factual information and are analytically quite primitive. I was at a loss at times to comprehend the relevance of the extensive tables in the long chapter on "the

social composition and turnover of provincial elites." This is, in sum, a flawed effort that, however, asks the right questions.

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HOLMES WELCH. *Buddhism under Mao*. (Harvard East Asian Series 69.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xviii, 666. \$16.00.

With the publication of this volume, Professor Holmes Welch, research associate in East Asian studies at Harvard, has completed a series of three books on Buddhism in modern China. The author admits candidly that it was more difficult to write this volume than the two previous ones, *The Practice of Buddhism, 1900-1950* (1967) and *The Buddhist Revival in China* (1968), because sources of information have been scant and have grown steadily scarcer and any statement concerning Communist China is bound to be controversial. Inasmuch as the history of Chinese Buddhism since 1949 involves millions of people diffused over a vast area with many regional differences, it requires unusual effort for any outside researcher to ascertain which, and to what degree, Buddhist institutions and practices have changed. In this respect the author should be commended for having combed through a staggering amount of relevant data, including those that were based on his personal interviews. The author says cogently that "ultimately a book like this has to depend on using odds and ends of inadequate data to make judgments, rather intuitively, about what *probably* happened" (p. viii). While we are in no position to judge the adequacy of Welch's perception, we learn a great deal about contemporary Chinese Buddhism from his lucid and insightful accounts.

Welch tells his narrative under ten major headings. In chapter 1, "A Policy Emerges," he traces the development of the official policy of the Chinese Communist party regarding Buddhism by sorting out the ambiguities and inconsistencies involved in the attitudes of the Religious Affairs Bureau toward the journal *Modern Buddhism* (*Hsien-tai fo-hsüeh*) and toward the Chinese Buddhist Association. He

finds that the official policy, at least until the Cultural Revolution, was to "protect Buddhism, while at the same time keeping it under control and utilizing it in foreign policy." How this policy was implemented is told in chapter 2, "The Decimation of the Sangha," chapter 3, "Making Monks into Good Citizens," and chapter 4, "The Reform of Monastic Life." The overall effect of this policy is succinctly characterized as follows: "The trend towards reorganization was offset by the trend for the number of resident monks to decrease. At many places there were simply too few people left to run anything but a museum. In any case, regardless of how monasteries were organized—whether they were headed by an abbot or a prior or a committee—real control had passed out of the sangha to the government, at first to the civil affairs bureau, later to the religious affairs division. Monasteries had lost their traditional autonomy" (p. 139).

The author makes a similar observation in chapter 5, "Preserving Buddhist Culture." After discussing the regime's claim for the conservation of Buddhist art and architecture, he states that "the art historian will be pleased that so much restoration was done, but the historian of religion may be less impressed. What interests him is not how many temples were restored but the degree to which they remained in religious use" (p. 168). Chapter 6, "Buddhism in Foreign Relations," shows how the Peking regime attempted from 1952 to 1966 to use Buddhism in order to influence public opinion in the neighboring Buddhist nations in Asia. That this policy was abruptly reversed on the eve of the Cultural Revolution is attributed by the author to "a feeling by Mao and others that the use of Buddhism in foreign policy had been more trouble than it was worth" (p. 228).

In the following chapters—"Suppressing Buddhist Opposition," "Interpreting Buddhist Doctrine," "The Laity," and "The Individual Buddhist"—the author portrays a bewildering picture of crisscrossing influences between government programs and the residual strength of Buddhist beliefs and practices on the part of the laity and the clerics. Of special importance is "the resourcefulness of Buddhists in trying to survive" (p. 266), even in the midst of almost insurmountable odds. Accord-

ing to Welch's account in chapter 11, "The Cultural Revolution and After," no news items were printed in the Mainland press after August 1966 concerning the Chinese Buddhist Association or the activities of monks and lay devotees. The author concludes the last chapter, "The Future of Buddhism in China," with the mildly optimistic opinion that elements of Buddhist belief and practice will remain a part of the "spontaneous religiosity" of the Chinese people in the years to come.

Readers will no doubt take issue with the author's interpretation of various events and the motivations of heroes and villains, both in the government and in Buddhist circles, mentioned in this volume. Nevertheless, all students of Buddhism and Sinology are greatly indebted to Professor Welch's careful and thorough analyses of the dramatic pages of Buddhism in contemporary China.

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JOYCE C. LEBRA. *Jungle Alliance: Japan and the Indian National Army*. (Asia Pacific World Library.) Singapore: Donald Moore for Asia Pacific Press. 1971. Pp. xiv, 255.

*Jungle Alliance* is a welcome addition to our understanding of the significance of the Greater East Asia War, which, according to an orthodox interpretation, was simply a Japanese version of the Western capitalist pattern of imperialism. Lebra has challenged such a notion.

The book is a product of painstaking research spanning ten years during which Lebra traveled to India, Japan, and Southeast Asia. The result is an authoritative account of "the interaction between Japan and the Indian independence movement in Southeast Asia." She narrates the story with objectivity.

The uniqueness of this study is that out of the narrative emerges a young Japanese major, Fujiwara, of the legendary *F Kikan*, who played a dominant role in the creation of the Indian National Army (INA). Lebra skillfully narrates the importance of Fujiwara in the making of South Asian history and provides a much-needed study of a remarkably individualistic policy entrepreneur and of the organization (*kikan*) he headed. Equally unique is the fact that India remained peripheral in Japanese military strategy, which was never

devised to push the boundaries much beyond Burma. Yet, this marginal interest in India produced the most charismatic revolutionary of all wartime Asian nationalists, Subhas Chandra Bose, who led the INA against British troops. Lebra describes well the impact of Bose upon such Japanese political and military leaders as Premier Tojo, army chief of staff Sugiyama, and the Burma area commanding general Kawabe.

Bose, as depicted by Lebra, was a single-minded revolutionary for whom the alliance with Japan was only a means for achieving the goal of Indian freedom. Nothing deterred him from this objective. This, together with his tragic death en route to Russia three days after the end of the war, is probably the reason he became a legend that still lives on in India.

*Jungle Alliance* supplements and complements K. K. Ghosh's *The Indian National Army*, published in India in 1969. Together they may well remain the definitive study of the INA. This is a fascinating study from which students of Japanese and Indian history may gain great satisfaction.

I should note the absence in the bibliography of volumes 8 and 9 of Yomiuri Shimbunsha's *Showashi no Tenno* (*The Emperor in Showa History*) (1969), which give detailed eyewitness accounts of Bose's submarine trip to Sumatra from Germany, of his fighting in the front during the Imphal Operations, and of his death in an airplane crash.

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H. PAUL VARLEY. *Imperial Restoration in Medieval Japan*. (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1971. Pp. xi, 222. \$8.00.

This is a study of the Kemmu Restoration (1333-36), first, as a crucial development in the shift of power from civil aristocrats to soldiers and, second, as a topic of historical reflection from that day to the present. Professor Varley has utilized the findings of Japanese historians, read extensively in contemporary chronicles, and supplied new information. His treatment of institutional changes made during the Kemmu Restoration and his discussion of the *Jinnō Shōtō Ki* (*Chronicle of*

*the True Line of Sacred Emperors*)—written between 1339 and 1343—are of special interest. But as valuable as these contributions are, they do not do much to help us understand the Restoration phenomenon or to see patterns of change in Japanese historical thought.

Professor Varley appears to think of the Kemmu Restoration as no more than a "final bid by the court nobility to assert its claim to the rulership of the land." Conflict between the old and new elites was certainly there, but surely it was the deep-seated, highly institutionalized beliefs concerning the sacred emperor that led each contender to fight for imperial sanction. A rigorous investigation of the throne's symbolic significance might indicate that Emperor Go-Daigo failed to gain control of the state because he did not make a sharp distinction between his sacerdotal functions and the handling of administrative affairs. Under the influence of Chinese political thought, and of Japanese precedents established when Chinese models were being followed, he may have strayed too far from the imperial way when he tried to be prime minister and shogun as well as emperor.

In comparing the *Jinnō Shōtō Ki* with the *Gukanshō*, which was probably written in 1220, Professor Varley does not move much beyond the observation that the former rejected the *mappō*-based pessimism of the latter and concentrated on "the true line of sacred Emperors." By delving into the intellectual and religious content of such terms as *seiri* and *dōri*—terms that represented different convictions about the sacred dynamics of history—he might have helped us to understand better the different positions of these two classics in the evolution of historical consciousness.

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ROGER F. HACKETT. *Yamagata Aritomo in the Rise of Modern Japan, 1838-1922*. (Harvard East Asian Series 60.) Cambridge, Mass.: Harvard University Press. 1971. Pp. ix, 377. \$12.00.

This book undertakes the formidable task of a "life-and-times" style biography of one of the most influential figures in modern Japanese political history. As a designer of the Meiji political order, Yamagata is known primarily



for his leadership in organizing a universal conscription system, forming a modern army, and establishing a semiautonomous position for the military within the government. He was particularly influential, too, in his role as builder of local government institutions, using the Home Ministry, which controlled local administration, as a powerful instrument for popular mobilization and for integration of the citizenry into the national political processes. With personal power thus rooted in the military and civil bureaucracies, he was a force to be reckoned with in national politics for nearly half a century.

His biographer, therefore, must cover a good deal of territory. As if so large an assignment were not challenging enough, he also must confront the peculiar difficulties of writing Japanese biography. Japanese leaders often appear flat and colorless as a result of paucity of biographical material, the murkiness of decision-making, and our uncertainty over how to analyze Japanese personality. Professor Hackett, for example, is handicapped by a lack of information about Yamagata's first thirty years and can find little to relate beyond the bare outlines of his early life. We get little feeling therefore of how Yamagata's experiences during these formative years conditioned his attitudes during the remainder of his career. Once beyond the Restoration, the author has more biographical material and the narrative picks up steam. With admirable clarity he surveys his subject's role in the political history of the next fifty years, making imaginative use of Yamagata's poetry to enliven the account.

Yet, as able as Hackett's narrative is, the book ultimately disappoints—only in part owing to difficulties inherent in Japanese biography. The greater disappointment is that the scholarship of the 1960s is not digested into the substance of this work. Professor Hackett does not speak directly to the major historiographical issues that have been hotly debated in the decade preceding the publication of his book. These issues involve interpretation of the Restoration, the making of the constitution, and the rise of political parties, in all of which Yamagata was prominently involved. The reader can guess what the author's views on some of these issues may be,

but he will wish that Professor Hackett had brought his understanding of political history to bear directly on these debates.

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F. G. NOTEHELPER. *Kōtoku Shūsui: Portrait of a Japanese Radical*. New York: Cambridge University Press. 1971. Pp. x, 227. \$16.00.

Recent biographers of Japanese figures have turned as often as not to radical intellectuals—dissidents who advocate direct action—rather than to members of the ruling elite. Doubtless the attraction of the radicals derives from their colorful careers and their penchant for committing their innermost thoughts to paper, both of which facilitate the writing process. Moreover, the vogue for these romantics, especially the martyred among them, in Japan today gives them a contemporary relevance. Professor Notehelfer's distinguished and well-documented study of Kōtoku Shūsui is a worthy addition to this genre. The Notehelfer biography is particularly notable for its delineation of the striking personality of his tormented subject. In these pages Kōtoku lives and breathes as a whole man in his Meiji environment while making his intellectual odyssey from *shishi* loyalism—by way of liberalism, socialism, and anarchism—to advocacy of imperial assassination.

Kōtoku's commitment to revolutionary action had its roots in traditional values, the author asserts, and he makes his case. It was political violence born of admiration for the swashbuckling *shishi* of the era before the Meiji Restoration. Because Kōtoku regarded them as successors to the *shishi*, assassins of the later Meiji era regularly won praise from him, sometimes fulsome praise in ode form. A late infusion of support for direct action from Western radicalism, from the Wobblies, and from others, merely reinforced Kōtoku's violent tendencies but pointed to the head of state of an exploitative society as the prime target.

This is fundamental—that the Meiji emperor should be considered a "murderer" deserving assassination by one whose early inspiration came from the loyalists in the Restoration of this same Meiji emperor. It seems to me, if I read the text correctly, that

the author does not altogether make his case that Kōtoku's thought—apart from his tactics—developed out of a “restructuring of traditional values” (p. 3). Notehelfer's own evidence suggests a massive injection of Western radical thought to bring Kōtoku to advocacy of regicide. The reservation is minor. The book, in fact, provides a splendid, incisive record of the constant revision of his political thought by Kōtoku, a practicing journalist, as ideas flowed to him from foreign publications and through residence in the United States. His march toward ever more radical programs for social justice coordinated with his deepening political and personal frustrations.

Kōtoku is here—warts and all. He wrote on behalf of social justice, yet dressed as a dandy, kept his distance from steerage passengers when aboard ship, and generally shunned the illiterate masses whom he found distasteful. He expressed compassion for people in the abstract but was notably cruel to those, particularly women, closest to him. Finally, he spent a career in opposition to the Meiji leaders, yet at the end allowed himself to be bought off by the home ministry to cease radical activity and study away from Tokyo.

For his link with the high treason case, an alleged plot to assassinate the Emperor Meiji, Notehelfer treats Kōtoku as neither “first martyr” nor “arch traitor.” In this dispassionate volume, Kōtoku comes through as one who rallied conspirators to the cause of imperial assassination with inflammatory words, only to back away from the plot as it matured. He returned, nevertheless, to die in style, unflinchingly, denying the accusations not at all, after a secret and controversial trial. The author suspects that the transcript of this trial may still exist, unreleased. Unless this document comes to light, the present biography will continue to be the definitive English-language study of Kōtoku.

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KATHARINE SANSOM. *Sir George Sansom and Japan: A Memoir*. Tallahassee, Fla.: Diplomatic Press. 1972. Pp. xvi, 183. \$15.00.

First, it should be said that this is a very stylish book, definitely a credit to George Lensen, the

Diplomatic Press, and the Irish printers who handled the production. And it is charmingly written. The cultivated style for which the late George Sansom was justly famous shines through, not only in the excerpts from his own writings, but in the general narrative as presented by his widow, the author. Despite her disclaimer, it is entirely worthy of Sir George.

The content is fascinating. Japan specialists of our present era will be enthralled by the story of the career of this great amateur, who in a very real sense provided in his *Japan: A Short Cultural History* (1931) the basic text from which most of these specialists got their start. It is clear that almost from the moment of his arrival in Japan in 1904, at the age of twenty, as a neophyte in the British consular service, Sansom began to soak up the culture that went into his later books. The encouragement he received from Basil Hall Chamberlain, Sir Charles Eliot, Langdon Warner, and other Western scholars is acknowledged, as is his indebtedness to many Japanese scholars with whom he spent hours discussing the fine points of his studies. The course of those studies, which resulted in the production of *An Historical Grammar of the Japanese Language* (1928), the aforementioned *Cultural History*, *The Western World and Japan* (1950), and *History of Japan* (1958–61), as well as several short pieces, show him persisting through all manner of distractions, consular reports, diplomatic assignments, the Pacific War, financial worries, and stomach ulcers to complete them. The story will warm the heart of any scholar.

But this is not merely a testimonial to scholarship. Historical events that Sansom lived through, but never wrote about in his books, the events of the 1930s and forties that found Japan on the road to war, into it, and through defeat and occupation are seen here through his eyes for the first time. His observations contain information useful to a variety of historical specialties. His recollection of the conversation between George Bernard Shaw and General Araki in Tokyo in 1933 is the first of a number of insightful commentaries on the Japanese militarists. His private discussion with Shigeru Yoshida (1940) provides an excellent basis for understanding Yoshida's role as postwar Japanese premier; Sansom's brief comments from Singapore (1942) explain for military history the

gamble on the weather taken with the *Repulse* and *Prince of Wales*, a gamble lost.

Regarding the occupation of Japan, which concerned him as a member of the Far Eastern Commission, Sansom's views were definitely conservative. He admired General MacArthur, was "not sure that the Americans realize what they are doing in their enthusiasm for freedom," worried about the "Communitistic leanings" of "many of the young officers at G.H.Q.," and thought there was not "a shred of evidence" that the Zaibatsu had "deliberately and willingly collaborated with the military." Though he felt that "the present liberals" (like Shidehara) were "too broken by past events" to be effective, he distrusted Japan's intellectuals as "critical of occupation policy" and "fundamentally anti-white." Certainly he was glad that his prewar friend Yoshida Shigeru emerged to lead Japan toward recovery while maintaining conservative moorings (chapter 12).

Since Sir George singles out Professor Ayusawa Iwao as an example of the "anti-white" Japanese intellectual (p. 152), it seems important for me, who knew Ayusawa fairly well, to point out that he was in fact a true internationalist, a "christian," in the broadest and best sense of the word, who devoted his life to international causes, including in his later years the establishment and development of Japan's great International Christian University at Mitaka. That Sansom could not appreciate Ayusawa is an indication of a flaw in himself rather than in Ayusawa. Sansom, for all his appreciation of the intercultural world, was to some extent patrician, impressed with titles, "class," and Anglo-American leadership in international affairs, which Ayusawa realized was too narrow a base for a world system.

Hence we may conclude with the observation that George Sansom did brilliantly what he did, which was cultural history, but it is probably just as well that he confined his writings on international politics to his diary and private papers, for his judgment on these was by no means infallible.

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ROBERT A. SCALAPINO and CHONG-SIK LEE. *Communism in Korea*. Part 1, *The Movement*; part 2, *The Society*. (Published under the auspices of the Center for Japanese and Korean Studies,

University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xxi, 685; xi, 688-1533. \$25.00; \$30.00.

The authors of this definitive study believe it extremely doubtful that a resurrected Karl Marx would view the Democratic People's Republic of Korea as a state drawn from his inspiration or as a true socialist society. Even Lenin might find it difficult to accept the creed of communism in North Korea and the structure of Kim Il-sŏng's polity, although Lenin would be more understanding than Marx.

Indeed, the present North Korean system was by no means inevitable, or even natural, for the society. Once it was implanted by Soviet power, however, and then evolved on its own, the system became a crucial variable and caused an ever-widening divergence between North and South. A few years ago Professor Glenn Paige wrote an essay for a conference on modernization held in South Korea and similarly predicted that the political variable would be of vital importance to such emerging societies. As the Scalapino-Lee study of the two Koreas illustrates so graphically, "that variable is not mechanistically determined by the particular stage of socioeconomic development in which the society finds itself, Marxian theory notwithstanding" (p. 1316).

Such reasoning practically guarantees that this monumental two-volume work will be dismissed out of hand by some scanners of "new leftist" persuasion, by those "new monolithists" who expect only similarities throughout the so-called third world, and by those with an uncritical predilection for any "communist" regime. There are other reasons. For example, the authors carried through painstaking research in prewar Japanese documents, early journals, rare leaflets, obscure newspapers, and elusive mimeographed items; they also relied heavily (for part 2) on in-depth interviews with thirty-four former North Koreans (that is, defectors) conducted in the 1967-70 period. These were utilized only after the authors conscientiously checked them against biographies, Comintern documents, official publications, and English-language material issued by the Foreign Language Press in Pyongyang. Nonetheless, the importance and validity of such data will be challenged by some readers.

The forthright stance, rather than the credentials, of the authors will be questioned

by others. Robert A. Scalapino is professor of political science at the University of California, Berkeley, and author of *The Japanese Communist Movement, 1920-1966* (1967) and numerous other Asian studies. Chong-sik Lee is associate professor of political science at the University of Pennsylvania and author of *The Politics of Korean Nationalism* (1963). They use an objective, balance-sheet approach. Nonetheless, they present devastating profiles—of a movement plagued by factionalism and failures; of a society proclaiming dedication to internationalism and, in fact, inspired by an ultra-nationalist, exclusivist, and vaguely racist principle (*chuch'e*, "the principle of making indigenous need and experience the essential criteria, of implanting *self-sufficiency* as the central theme of the Korean revolution"); of a reconditioned guerrilla-hero leader who administers a twentieth-century monarchy with a Stalin-like cult of personality, existing (incongruously) alongside an elaborate institutional structure and refined creed of collectivism; of a modern party elite who, if they are not drawn from the *yangban* (gentry), neither are they from the ranks of the urban proletariat; and of a state dedicated to the creation of a new socialist human being and to the salvation of man, but in which concern about men steadily diminishes.

Beyond controversy, this is a pioneering work in the field of comparative communism, now in its infancy. It makes frequent comparisons with other communist systems, particularly in Russia and China, with which North Korea has been closely associated. The authors make a case for a more accurate typology of communist systems, including delineation of their common and particular features. The Democratic People's Republic of (North) Korea has been and remains by self-definition "communist." What does this mean?

The study demonstrates that Korean "communism" has been indelibly stamped by traditional (Korean-Confucian) political culture, warped by its peculiar (Japanese) colonial experience and evolution in (Manchurian) exile, nurtured under alien (Russian) domination and salvaged by alien (Chinese) military aid, and tested in (Korean civil war) emergency. "In the final analysis, it is the dynamic interaction among Communism, emergence, and tradition that provides the fullest context in which the

politics of the society operate" (p. 1306). In the present order of priorities and in the means used, the authors conclude that the Korean Communists reveal themselves to be leaders of an emerging society first, Marxists second. Kim and his cohorts have not seen Marxism as the culmination of the modernization process, but rather have sought to make of it an instrument of modernization and in this effort have basically altered Marxism.

To the specialist on East Asia and to the depressingly few experts on Korea, the two-volume work represents a staggering challenge. In sheer volume of data sifted and presented, as well as in comparative insights, it is difficult to imagine that this study will soon or easily be matched. The first eight chapters (part 1) present a historical, developmental treatment, with emphasis on the origin and relationships with other Asian communist movements. The final seven chapters (part 2) turn to topical, social-science oriented analysis, with emphasis on the evolving character of the political elite and most especially on the emergence of Kim Il-sŏng to a position of absolute power and evolution into an institution. Attention is paid to organization, the role of ideology (my favorite chapter), the functions of special groups (for example, the cadres), the status of the military, the economic structure, and performance. Most enlightening are the descents from the abstract to microlevel vignettes of the actual life-styles of the intellectual, artist, student, party cadre, soldier, peasant, and worker.

To the most critical and persistent expert, the two volumes offer rich rewards in massive documentation. There are detailed maps, numbered (twenty-nine in part 1, seventy-two in part 2) and unnumbered tables, and appendixes. These include the constitution, party rules, lists of Central Committee members (1948-70) ordered by rank, cabinets (1948-70), charts of selected commissions, ministries, and local governments, as well as statistics on prices, wages, and monthly budgets. The authors present their interview schedule and a complete bibliography (subdivided into Korean, Japanese, English, Russian, and Chinese sources). The index itself is a detailed forty-one-page pamphlet inserted between parts 1 and 2.

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HAZEL KING. *Richard Bourke*. New York: Oxford University Press. 1971. Pp. xv, 312. \$15.50.

Sir Richard Bourke, governor of New South Wales from 1831 to 1837, was probably the most attractive and popular governor in Australia before 1850. Dr. King has written an outstanding biography that is a model of compression. She exploited the fine collection of Bourke papers in the Mitchell Library, Sydney, and ferreted out private collections in Australia, England, and Ireland. Her skillful use of quotations gives us a vivid sense of the man and his family. Newspaper files and official government documents were exhaustively examined, and the issues of the time have been clearly presented and woven into the thread of the narrative. She is cautious, fair, and balanced in her judgments. The result is a full-length biography with fresh information about Bourke's early life, education, military career, home life in Ireland, and his governorship of the Cape of Good Hope, 1826-28. Less than half the book covers his administration in Australia.

It is now apparent that Bourke might have risen high in the British Army. Between 1798 and 1806 he showed great promise. He fought with Wellesley in the Peninsular campaign, but put his family before his career and dropped out of public life between 1808 and 1812 to guard his wife's health and to help the treatment of his oldest son's hydrocephalus. Returning to Spain, he served in a quasi-diplomatic role, then retired to Ireland from 1815 to 1825 to live the life of a country gentleman.

Coping with the imperial problems of the Cape of Good Hope prepared Bourke for handling similar ones in Australia: disputes over the press, the bickerings in the executive and legislative councils, native policy, taxation, the consistent parsimony of the treasury, the interference of the Colonial Office, and others. Dr. King rates Bourke a success and deserving of the assignment to Australia.

Arriving in Sydney, Bourke's affiliation and his dependence on the new Whig government in England are stressed by Dr. King. We also learn much about Bourke's Australian friendships, especially his closeness to the Chief Justice, Francis Forbes. Dr. King brings out the serious thought given by the Colonial Office to the immediate abolition of penal transportation. The chapter on the aborigines is too short,

though the point is clearly made that Bourke considered the problem insoluble. He did not understand the natives, a surprise as he had been much more successful with them in South Africa. More could have been written on his success with religious toleration and his failure to introduce public education. Dr. King accentuates the weak support Bourke received from London, which eventually caused his resignation. Both his church and education policies were hamstrung by Glenelg's indecision. However, Bourke received support for his idea of permitting pastoral expansion by granting licenses, renewable each year, to squatters on Crown lands outside the boundaries of location. Indeed, his six years in Australia were ones of economic prosperity, particularly in the wool trade. His immigration policy of free settlers was also successful.

After Bourke retired to Ireland he refused other posts, such as the governorship of Jamaica. Dr. King's thorough study has illuminated both a fascinating human being and his times.

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J. A. LA NAUZE. *The Making of the Australian Constitution*. (Studies in Australian Federation.) [Carlton:] Melbourne University Press; distrib. by International Scholarly Book Services, Portland, Ore. 1972. Pp. xi, 369. \$20.00.

To the student of government it is obvious that there have been very few unions of states or provinces reached by popular assent that have endured. The United States is the oldest and Switzerland the only example outside the English-speaking world. Historically the great states of the ancient and modern world have grown by military conquest, and their strength and endurance have depended on the strength of the central government of the conquering power. Another universal truth seems to be that such federations come into existence only in the face of considerable popular opposition and have usually been achieved under pressure from the economic rivalry or military threat of outside powers. In the case of the United States and Canada the strongest opposition came from the smaller units—Rhode Island and Prince Edward Island are examples—but in Australia, New South Wales, the largest and oldest prov-

ince, was the most difficult to convince. Politicians in Sydney, wishing, no doubt, to claim leadership in the commonwealth that they knew was coming, worked hard as framers of the new constitution, but they never wholly converted their constituents.

La Nauze does not explain this divergence from what might be called the norm of federal movements, but he makes clear that the movement as a whole set a record for the length of time it took Australians to make up their minds that federation was a necessary step in their political evolution. Fifty years went by—in which many of the wisest statesmen recognized the step as inevitable—before the Commonwealth of Australia was proclaimed in 1901. Australia, the “quiet continent,” did not suffer from the pressures that had harassed the United States and Canada.

La Nauze's volume is a meticulous and fully documented account of the conventions at Melbourne, Adelaide, and Sydney, and also of the discussion at the Colonial Office in England where the Colonial Secretary, Joseph Chamberlain, was chiefly concerned lest the right of appeal to the judicial committee of the Privy Council in England from the High Court in Australia be abridged in cases involving the British investors of capital. The book will be invaluable as a reference work for Americans teaching British imperial history, but it cannot be recommended for light reading. Fortunately the last chapter, which sums up some of the conclusions reached earlier, is good reading and of interest especially to us because of the pages dealing with the influence of the American Constitution and American constitutional government as the Australians understood it. The chief source for its study was James Bryce, *The American Commonwealth* (1888), which “lay on the Table” for all to consult during the meetings in 1897 and 1898. The document sent to England, which is one of many useful appendixes, does not very closely resemble the American Constitution as we know it, being much more diffuse in wording and much more flexible in the powers given to the Australian Parliament to make changes in some fundamental provisions. Even so, La Nauze ends with the criticism that the constitution should have provided for total reconsideration and revision at intervals. Apparently he feels that if it were

rewritten today, a good deal of it could be omitted.

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## UNITED STATES

DAVID M. POTTER. *History and American Society: Essays*. Edited by DON E. FEHRENBACHER. New York: Oxford University Press. 1973. Pp. x, 422. \$10.00.

A quotation from the first of these essays, “Explicit Data and Implicit Assumptions in Historical Study,” could stand as a fitting epigraph to the book: “The evaluation of significance may be a matter of sagacity and applied experience which cannot be taught as method. When we encounter this sagacity in politics, we call it statesmanship, and we do not for a moment suppose that students can be trained in school to be statesmen. When we encounter it in historical studies we are likely to call it ‘an awareness of the historical process.’”

The rest of the book exemplifies this awareness, along with Potter's conviction that “the tasks of the historian must change as our conception of the problems and issues of society changes, and we must be prepared for the fact that the historian's work will not lie in those areas where his methods make him feel most at home but in those where society's need for an understanding of its past is most acute” (“Tasks of Research in American History”).

This is an invaluable and thought-provoking book, one from which the general reader as well as the historian can profit. The title, it should be understood, has reference to the structure as well as the subject matter of the book: nine essays on history and seven on American society. Of the sixteen essays eleven were previously published, but some of them in relatively obscure journals. The most familiar is probably “The Historian's Use of Nationalism and Vice Versa,” in which Potter used the experience of the Civil War as the focus for a wide-ranging exploration of the relative importance of cultural differences, historical process, and conflicting interests to the phenomenon of nationalism. The essays on history include, in addition to those mentioned above, one in which Potter, while paying his respects to social science, argues that its methodology

cannot meet the historian's need to form judgments, to deal with ideas and influences, and to consider heterogeneous elements. A short piece on the problem of large-scale community formation serves as a kind of preface to the familiar essay on nationalism, and the first part of the book is rounded out by a critique of the Turner thesis, reprinted from *People of Plenty*, and essays evaluating each of three historians: C. Vann Woodward, Richard Hofstadter, and Roy F. Nichols.

The essays on American society turn largely on the question of the American character and in nearly every case advert to the conflict between individualism and equality, the latter being "conducive to conformity rather than to freedom, since it places the stigma of arrogance upon any man who ventures to set his personal judgment against the judgment of a majority of his equals." The previously unpublished essays include one that is probably the most important and easily the most arresting in the entire book: "Rejection of the Prevailing American Society," a lengthy yet all too brief preview of a major historical study of alienation in American life that Potter had projected before his death in 1971.

Dissent, Potter argued, had come to find outlet in confrontation and disruption because of the decline of sanctions against such behavior. Down to early modern times the main sanction had been simply coercion, and aggressive impulses had to find objects other than the institutions of society. After the American Revolution, however, coercion had been replaced by the impulse to conformity, which meant conformity mainly to WASPish values in society. But by the 1920s and 1930s those values had suffered crippling blows, most conspicuously in the failure of Prohibition and the social trauma of the Great Depression. But perhaps most crippling of all was the rise of a new class, the pedagogues of academia who became intellectuals and "set busily about laying their axes to the mythic underpinnings of the American identity." By the 1960s the defenders of society were guiltily aware of its failure to fulfill its own ideas and disarmed also by "broad, absolutist, and somewhat indiscriminate ideas of the right of dissent." Society even accorded a kind of sanction to disruption.

By way of summary, Potter's argument goes

something like this: all societies have a quantum of discontent, some of it rationally derived, but not necessarily commensurate with either the degree of injustice or the degree of social protest, since discontent in the past has found other outlets, like revival meetings. Finally, the private and individual sources of much discontent have traditionally been ignored by historians, although they may be widely prevalent and may indeed have arisen from historical changes. The last part of the essay, therefore, focuses on problems of individual identity and roles, more specifically on the male sex role.

The growing difficulty of male identification with the father, as Potter noted, has been widely recognized. Less widely recognized has been the decline in the role of the play group of prepubescent males, often celebrated in such classics as *The Adventures of Tom Sawyer* or, in its more cruel and tribal aspects, *Lord of the Flies*, but less often appreciated as a functional institution in the development of masculine identity. Somewhere along the way the group, too, has lost its authority, leaving large numbers of males confused about their identity and role in society and filled with a dislike for authority, whether exercised by themselves or others, and turning elsewhere for the fulfillment of personal needs—to identification with symbols of rejection, to the drug culture, and the hippie culture.

This essay, along with pieces on "The Roots of American Alienation" and "Social Cohesion and the Crisis of Law," offers tantalizing glimpses of what the larger study might have been. The focus on male roles in "Rejection of the Prevailing American Society" is matched by a focus on female roles in "American Women and the American Character" (first published in 1962), which has achieved the status of a minor classic in its field. Taking off from the point that "our social generalization is mostly in masculine terms," Potter noted that many such generalizations—about the effect of the frontier, regimentation of labor, specialization, sedentary occupations, the growth of other-directedness—have little relevance to the lives of women, whose experience in some cases has been opposite to that of men. If women have gained in a labor system in which biceps are no longer at a premium, society has remained unable to

"make her career aspirations and her family aspirations fit together as they do for a man." And those who have become housewives have encountered new frustrations—frustrations in being the only workers who do not get paid, in their new economic role as consumers rather than processors, and in their new social role of keeping up with community activities.

The book is so full of substance that no review can possibly touch upon everything of value that it contains. It reflects, moreover, the author's constant dedication to rational judgment, to live issues, and to a simple felicity of style happily free from polemics and jargon. Among other things, Potter was no mean epigrammatist and the editors of the next *Bartlett* will do well to dip in here before they go to press. A few pertinent samples: "An adversary need not be a mortal enemy." "If hypocrisy is the tribute that vice pays to virtue, permissiveness is, to some extent, the tribute that authority pays to the principle of equality." "The power to disrupt is the power to discredit. And in public affairs, the power to discredit is the power to destroy." "The spirit of the commune is . . . an unstructured diffuse sense of 'love' toward everyone in general and no one in particular." "The myth is to the society almost what a sense of personal identity is to the individual." "But history often mocks logic."

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SYDNEY E. AHLSTROM. *A Religious History of the American People*. New Haven: Yale University Press. 1972. Pp. xvi, 1158. \$19.50.

This book is a monument not only to its subject and to the ample support of the Yale University Press but also to the prodigious enterprise, the scope of imagination, and the range of interest and empathy of its author. It is an attempt to write the history of American religious life in the four-hundred-year period from the time when the Reformation was more or less established until the 1960s, with a brief but poignant coda suggesting another spiritual era sloughing toward Bethlehem to be born, as Yeats had foretold. At times one may be

deluded, by Ahlstrom's sedulous attention to the detailed transition from decade to decade, into assuming that this book is one more, perhaps a more grandiose, celebration of the triumph of religion. But Ahlstrom's schema, though informed by his grand conception of an epoch of religious life that was born, developed, and then imperceptibly weakened, is not obtrusive. In manner, he is closer to Neander than to Hegel. In spirit, he is much too aware of the cunning twists that history takes to allow his general view to obtrude upon his close reading of the ironies and ambiguities of cultural development. To employ an impious comparison, for an American, he looks at history more in the manner that Lincoln regarded the Civil War than in the modes of Horace Greeley, Wendell Phillips, or Bishop Lynch of Charleston, South Carolina. It is characteristic of his perception of past, present, and future that he can conclude a chapter on Roger Williams's Rhode Island by saying that that experiment "seems to illustrate in an almost tragic way the political corollary of a dictum often voiced by historians of science, that premature discoveries are uninfluential."

But he does take his periodization seriously. He does believe that the 1960s mark the inauguration of a new period. Not only has the Roman Catholic Church largely abandoned the triumphalism that since Trent dominated its thought, but Judaism is back in the wilderness, and Protestantism is increasingly polarized between Jesus-freaks and God-squads and increasingly tempted by an intellectual dosage of speed and tranquilizers in its holy place and holy hour.

In writing of an epoch in (primarily) Western religious experience Ahlstrom refuses to be parochial. Though he scants the Reformers more than I should have liked he wisely gives more space to Kant than to Bushnell: and he acknowledges that the end of Victorianism marked an end of certainty for almost everyone in the West. Nor is he seduced into writing church or denominational history. Where the church or the denomination is a real force he discusses them, but he sees no need to devote unusual time to organizational setting-up exercises; intelligently he is willing to make exceptions, as with his treatment of Christian Science, which he finds more notable for its



organizational brilliance than for the character of its ideology.

Somewhat more problematically, Ahlstrom defines quite narrowly what he means by "a religious history of the American people." He gives little attention to the way in which economic, political, cultural, and social life in this country has been shaped by religious presuppositions, if not dicta. And though in the introductory sections of each of his more than sixty chapters he sketches shrewdly and succinctly the milieu of the religious life he will be concentrating on, these are obviously not at the center of his attention; it would be, accordingly, foolish to nit-pick at some of these opening paragraphs or pages. His stake, and the reader's, is in what follows. And what follows is very, very good. Another principle of Ahlstrom's book is that he unhesitatingly recognizes that no more than an approximate chronological order can be adhered to. Individual readers might want to read about Hasidism in the context of Kant and the romantic movement; and others may be surprised to find Mary Baker Eddy so many pages removed from discussions of the "transcendental strain." But it is exceedingly unwise—as I found in preparing this review—to conclude that Ahlstrom is leaving something out. One may quarrel with his architectonics but not with his close attention to the many mansions in American religious life. One's own blueprint might be different, but be assured that Ahlstrom has provided a room for virtually every view that can be conceived of.

A consequence of this intellectual ecumenicism is that even in a volume of more than one thousand pages the author must abbreviate. Aware as most of us are of Ahlstrom's deep knowledge of New England theology, or of Mercersburg, or of the strands of Lutheran thought we may feel at times that he is compressing his knowledge into the constrictions of a new *Hastings Encyclopedia*—in one volume! But one wants more on a subject for the best of all reasons: that Ahlstrom has above all been judgmental, discriminating, and persuasive in what he does have room to say. At the same time he is empathetic with what foreign observers have sometimes regarded as a zoo of religious curiosities. Take, for example, his discussion of neo-orthodoxy in American Protes-

tantism. After some incisive remarks about the Niebuhrs, he notes the breadth of the attitude, which embraced orthodox Presbyterians and Pentecostals; all joined in "an assault on both the great romantic doctrine that the religious and/or moral consciousness provides the proper starting point of theology. . . . The genteel tradition must go; metaphysics cannot do duty for the revealed Word of God." "A situational-contextual 'love-ethic' became the positive part of a widespread critique of legalism and code morality"; "because neo-orthodoxy did not rest its ultimate faith in human arrangements, it could bear—or even advocate—the shaking of cultural foundations." Neo-orthodoxy, in contrast to fundamentalism, showed great respect for the "scientific, scholarly, and artistic achievements of men." It also put great stress upon "the Church," and especially "the prophetic church that would recognize its continuity with the New Testament community, and, therefore, its distinctness from the world in which it proclaims the Word and to which it ministers." The early Fathers, the councils, and, above all, the Reformation theologians "also gained new currency. . . . They were taken seriously rather than dismissed in the liberal manner as outmoded stages in the evolution of pure religion."

No one will find everything in this volume that the title and the richness of the text leads one to hope for. I, for example, would have liked a bit less on anti-Catholicism and more on the inner tensions and resolutions, however inchoate, of American Catholics. And, curiously, Ahlstrom, while profoundly sympathetic with what James might have called the religious "impulse," takes its existence for granted and does not probe deeply into the changing psychology of its provenance. But let me repeat: he is no muckraker; in a book much compressed he takes time and space for a moving letter from a woman to whom what he calls "harmonialism" is deeply important.

Ahlstrom saves one of his most important, one of his most general, insights for his last page. "The American experience," he writes, "does not explain itself." He who would look into the future, "whether as amateur or professional . . . will be a pioneer on the frontiers of post-modern civilization." He may be wrong in concluding that any such pioneer may find strength in the "idealism" of the country's

religious past; there are many other elements that Ahlstrom has himself in this book acutely illuminated. But no one writing or thinking hereafter about America's past will be able to ignore Ahlstrom's magisterial account of the religious element.

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RICHARD AUBREY MCLEMORE, editor. *A History of Mississippi*. In two volumes. Hattiesburg: University and College Press of Mississippi. 1973. Pp. xiii, 703; xi, 651. \$25.00 the set.

These encyclopedic volumes supersede all previous general histories of Mississippi. Forty-one authors, mostly natives of Mississippi or teachers at Mississippi colleges and universities, wrote the forty-three chapters. Most of the writers are professional historians, but the list of contributors includes a geographer, an anthropologist, an agronomist, and a sociologist, as well as a former governor and the present lieutenant-governor of the state. These highly factual essays begin with geography and prehistory and end with events of the 1970s. They offer most readers all the information they ever need to know about Mississippi.

These volumes suggest the healthy state of historical scholarship in Mississippi. Most of the essays rest upon extensive original research. In some chapters authors have summarized and updated their earlier writings. Thus John K. Bettersworth's judicious essay on "The Home Front, 1861-1865" is a condensation of his standard monograph on *Confederate Mississippi* (1943). Other contributors have expanded or synthesized their previous, more specialized studies. The elaborate documentation (together with the valuable bibliography compiled by Willie D. Halsell) indicates how much important work has been done in Mississippi history and is particularly useful as a guide to numerous unpublished theses and dissertations.

Despite the fact that these volumes were partially financed by the state of Mississippi most of the essays are remarkably objective. In only a few instances are Mississippians told that they "can take pride" in certain developments or informed that one of their politicians deserves to rank "as one of the country's greatest statesmen." If there is a certain amount of

pointing to Mississippi "firsts"—for example, to the fact that Mississippi established the first state-supported college for women—there is no attempt to conceal that Mississippi has all too often been last among the states, as in its rate of literacy and its per capita income.

In a refreshing break with tradition the editor, Richard Aubrey McLemore, who is also director of the Mississippi Department of Archives and History, has chosen 1890 as the dividing date to separate the two volumes. This arrangement gives implicit support to the growing conviction among historians that the Civil War-Reconstruction era marked much less of a break in Southern history than earlier scholars believed, and, of course, it gives much more space for extensive treatment of history of the past eighty years. Whether 1890 is more than a convenient dividing point in Mississippi history remains doubtful. Cumulatively the essays in these volumes show how astonishingly little the state changed up until about 1930 and suggest that the Great Depression and World War II brought about a striking transformation of Mississippi agriculture, industry, race relations, and, eventually, politics.

In a cooperative work of this scale there is inevitably overlapping, and even contradiction, among the essays, and McLemore has made no attempt to smooth these out. Indeed, except in the two final chapters, there are not even cross-reference footnotes to indicate that one contributor was aware that another had also dealt with his topic. Since nineteen of the chapters deal with broad subjects like geography, education, and labor, there is a considerable amount of repetition, and only the careful indexes bring together scattered discussions of the same topics.

McLemore's light-reined editing has also allowed enormous variation in the quality of the chapters. In general the first volume is sounder and more informative than the second. Especially valuable are Porter L. Fortune's analytical discussion of "The Formative Period," John E. Gonzales's sprightly chapter on "Flush Times, Depression, War, and Compromise," and William K. Scarborough's thoughtful account of Mississippi slavery. Except for Neil R. McMillen's admirable account of the "Development of Civil Rights, 1956-1970," the chapters on politics in the second volume are inferior in

quality. James P. Coleman's chapter on the Mississippi constitutional convention of 1890, which disfranchised blacks, shows how little a former governor and a present judge of the United States Circuit Court of Appeals understands either about his state or about history. William D. McCain's chapter on the Bilbo era is grossly partisan in its praise of an administration characterized by a contemporary as "a four-year orgy of graft, corruption, extravagance, political trickery, demagoguery and scandal in high places." Some of the topical chapters that make up the bulk of the second volume, such as Thomas D. Clark's superb essay on "Changes in Transportation," are highly instructive, but others dribble off into long lists of schools, colleges, judges, lawyers, hospitals, and the like.

Just because this impressive *History of Mississippi* is going to be, at least for a generation, a standard work it is necessary to mention its limitations. First, this is essentially a history of white Mississippians. Unlike some earlier histories of the state this is not a racist work. There are no slurs upon Negroes. On the contrary, the quality of black leadership in Mississippi during the Reconstruction period is described as "higher than that found in most southern states," and Mississippi Negroes are exonerated not merely of excessive political corruption but even of any special desire to hold public office. But references to the black half of the state's population are, throughout, few. As a result there is also little discussion of interaction between the races. William Lincoln Giles's judgment that in rural Mississippi there developed between blacks and whites "a mutual confidence, respect, and interdependence resulting in splendid race relations" is one of the few offhand remarks on that subject. Readers may be more impressed by the fact that "lynching" does not appear in the index of either volume.

Second, by focusing on the complex story of internal developments in Mississippi this history necessarily neglects the connections between the state and the rest of the nation. Occasionally one of the more thoughtful authors will offer a judgment like Glover Moore's conclusion that "in the 1850s Mississippians had more influence in Washington's ruling circles than ever before or since"; but there is no

systematic discussion of federal-state relations. Most United States senators and congressmen get little more than a listing in these pages, while every governor of whatever caliber rates a full-length discussion. This emphasis does not seriously distort the history of Mississippi in the nineteenth century, but since at least the 1930s the role of the federal government in agriculture, in industry, and in race relations has often been more important than the part played by local politicians.

The third problem is one posed by any state history: does a state really have a distinctive history? There is little in the useful chapters on economic, social, and cultural life in these volumes to indicate that Mississippi was much different from other states of the Deep South. Politically and administratively, of course, a state does have a history, but it is hard to find a central theme that will elevate state politics above antiquarianism. That task was made the more difficult in the present instance, where many writers worked quite independently of each other. Thus Fortune and Gonzales suggest that the early political history of the state revolves "around the challenge of the expanding areas of the state to the dominant position of the Natchez district"; William F. Winter sees politics of the more recent period as a perennial struggle of "Delta v. Hills"; and James G. Revels agrees with V. O. Key's analysis that the Negro is "the beginning and the end of Mississippi politics." But none of these somewhat contradictory suggestions is developed in a sufficiently systematic way to give unity to Mississippi's political history.

There is, of course, another kind of unity that the history of some states does have: the shared sentiments and beliefs of its inhabitants. There is, unfortunately, in McLemore's massive volumes too little discussion of what has made Mississippi such a distinctive and peculiar place, with a tenacious hold upon its natives even after they have long left the state. "Mississippi to me," wrote Tennessee Williams, who is one such expatriate, "is the beauty spot of creation, a dark wide spacious land that you can breathe in." Neither that spaciousness nor that darkness comes through in this work, which, after all, was not intended to be an interpretive history but a reference tool. An invaluable one McLemore's *History* is, too, and perhaps it will

assist some future W. J. Cash in writing "The Mind of Mississippi."

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ANDREW OLIVER, editor. *The Journal of Samuel Curwen, Loyalist*. In two volumes. Cambridge, Mass.: Harvard University Press, for the Essex Institute. 1972. Pp. xxxiv, 516; xi, 517-1083. \$30.00 the set.

Samuel Curwen has long been one of the best-known American loyalists, not because of his eminence within the loyalist community but rather because his fascinating journal was first published more than one hundred years ago, albeit in what now appears to have been an inaccurate and incomplete edition. As Andrew Oliver, the editor of this new version, points out in his introduction, George A. Ward, his predecessor (like too many other nineteenth-century editors), changed words and phrases, omitted some entries either in whole or in part, and combined others at random.

Curwen, a Salem merchant before the war, fled to England in 1775. Throughout nine years of exile he faithfully kept this journal, which is filled with incisive comments on his life in London and in various provincial towns like Exeter and Bristol. Unfortunately for those historians who have relied heavily upon him, however, Curwen was not truly representative of his fellows in at least one important respect, his attitude toward America. For example, in December 1776 he angrily wished for a rebel victory to convince "these conceited islanders" that America too had its "brave soldiers." Although his pro-American sentiment is often used as evidence of a change of heart common to other exiles, Curwen's position was unique, and therefore to rely solely upon him is misleading.

Further, a comparison of Ward's version with Oliver's shows how the former's faulty editing has led historians astray for over a century. To cite only one instance: on February 1, 1776, Curwen attended a meeting of the New England Club, an exile organization, and recorded its membership. Ward, it turns out, added three members, altered Thomas Hutchinson's status in the club by omitting a significant phrase, and changed Curwen's "Mr. Waldo" to "Joseph Waldo," whereas the member in question was probably Francis Waldo, a customs

official from Massachusetts (Joseph was an American-born, pro-rebel Bristol merchant, so the difference is far from trivial).

Oliver's edition is, on the whole, accurate and meticulous. At times he neglects to identify Curwen's exile friends adequately: for example, Mr. and Mrs. Henry Barnes of Marlborough, Massachusetts, whom Curwen saw in Bristol, are never identified as Americans. At other times Oliver makes mistaken identifications, the most glaring of which (evidently through a transcription error) confuses the rebel Colonel Francis Marion with Colonel John Murray, a loyalist from Rutland, Massachusetts (p. 171). But such flaws are few and far between. Oliver has done a fine job, and this edition of Curwen's journal is a must for every serious student of the Revolution.

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JACKSON TURNER MAIN. *Political Parties before the Constitution*. Chapel Hill: University of North Carolina Press, for the Institute of Early American History and Culture. 1973. Pp. xx, 481. \$15.95.

Professor Main's immensely thorough inquiry into preconstitutional American political parties exemplifies both the advantages and the risks of parliamentary biography—a method of historical investigation that we owe to Beard and Namier. The result is a systematic study of the votes in state legislatures wherever records exist in the Confederation period, amplified with biographical information going far beyond Beard's crude economic categories to include such matters as education, religion, and "world outlook." We are thus presented with vastly more, and vastly more reliable, information about the members of state legislatures than we have ever had before. Main likes to let his materials do much of the talking, while he assists them with numerous and useful tabulations. It cannot honestly be said that the author's style makes for readability; it often reads like a direct transcription from index card entries and is unfortunately likely to turn too many readers straight to his conclusions, which are not the safest guide to much of the state-by-state detail.

The broad results, however, are clear and impressive. Throughout most of the state legisla-

tures two groups emerge, defining themselves and opposing each other on the principal divisive issues with marked consistency. Main non-committally calls them "Cosmopolitans" and "Localists." These legislative "parties" identified themselves over the treatment of loyalists and their property, various forms of debtor relief, specific local issues such as that of the Bank of North America, and, above all, paper money. Main also finds a high level of continuity from the Cosmopolitans and Localists to the Federalists and Antifederalists and argues that these continuities will be found to survive into the party system of the 1790s.

Yet much will always depend on the questions that are put to the historical data and the categories that are perceived. These and similar issues certainly did mark divisions between great men and small, inhabitants of coastal and inland districts, town and country, and commercial and noncommercial farms in different states, but they did not always divide them along the same lines; there seems inadequate ground to presume that the issues themselves defined the categories either on a nationwide or even on a consistent local basis. On the contrary: Main himself finds that on paper money the Cosmopolitans of the South were thoroughly split (pp. 339-40), and in summarizing "The Issues" he remarks that "large landowners, on the average, divided just about in half and only rarely united" (p. 362). This constitutes a significant reservation to his conclusions precisely because the definitions themselves were reached through the analysis of voting records, to which should be added his own cautionary observation that "the present study tends to exaggerate the differences between blocs" (p. 359 n.11). Yet Main's conclusions have a relentlessly reductionist character, which persistently tends to emphasize the differences.

Hidden preconceptions may be partly responsible for occasional translations of the statistics into a sense they do not bear. Thus, on the restriction of slave importation during South Carolina's period of economic crisis, he writes: "The low-country majority for the restriction did not come from the planters, only half of whom agreed to deprive themselves of more Negroes, but from business and professional men: thus two-thirds of the latter ac-

cepted the amendment" (p. 285). Stated differently, the same figures show that this majority actually came from two-thirds of the businessmen plus no fewer than half of the planters; it is perhaps more remarkable that one-third of the businessmen opposed it. There is much consistency in the voting records that Main has found, but his language exaggerates it; eighty-one per cent becomes "almost always" (p. 290 and table 10.3), but eighty-one per cent can also be called four-fifths, and out of 200 is only 162!

Main is dismissive toward social issues, however important, if they "did not contribute to legislative bloc voting" (p. 354). Why? Because they did not contribute to legislative bloc voting. But the considerable number of neutrals disclosed by his researches seem to have been as consistent as the party faithfuls, while as many as a third of his "members" had inconsistent voting records. This finding is surely just as important for an understanding of politics as are the alignments that do appear on the issues that divide the representatives. "Inconsistency" exists only in relation to perceived consistencies. Party politics were widely deprecated, and many legislators plainly felt that neutrality was preferable to alignment, if not always attainable. These difficulties are involved with Main's basically unsatisfactory treatment of the concept of "party" in eighteenth-century politics, which leaves him groping darkly for an understanding of why the concept itself was so controversial.

He begins with the definition in Dr. Johnson's *Dictionary*; but he ignores Burke's tententious but far more celebrated explanation, which virtually launched the modern idea of party (whatever Namier may have thought!). There need be no mystery about all this, since the idea of party came down from the terrible divisions of English politics in the seventeenth and early eighteenth centuries, with all their implications of disloyalty and even treason. But in this as unfortunately in other respects Main impairs his own position by his neglect of the conceptual and historical problems implicit in his inquiries and his disregard of the considerable literature that they have generated—most notably, Hofstadter's *Idea of a Party System* (1969)—with the result that he fumbles heavily with problems, concepts, and definitions that

have already been illuminated. His own criterion of party is merely mechanical: a two-thirds consistency in voting with one bloc constitutes "membership"; those legislators who voted this way are held to form a party. Whether they conceived themselves as doing so, how policies were concerted, what loyalties were invoked or formed—these questions, which are of critical importance to the idea of party, receive negligible attention.

Main's conclusions also raise unexamined problems of explanation. He treats virtually all strong statistical correlations as self-explanatory—and it should be said that most of his findings will satisfy the common-sense criteria of explanation, when carefully used. But he does not seem quite to appreciate that statistical correlations are not the same thing as explanations. He frankly states, perhaps overstates, the limitations of his methods with the warning that uncertainties and probable errors in the data give the statistics "an impression of accuracy that is quite false" (p. 43). This warning, however, does not touch the deeper problem of the connection between statistical correlation and historical causation. If these comments are critical, they are not meant to be ungrateful; they are made because of the obvious importance and usefulness of Professor Main's research.

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RICHARD WALSH, editor. *Journal and Correspondence of the Council of Maryland*. Volume 10, *Journal of the Council, 1789-1793*. (Archives of Maryland, volume 72.) Baltimore: Maryland Historical Society. 1972. Pp. xiii, 395.

This is the second volume in a series devoted to the journals and letters of the executive council of Maryland, 1784-1821. About eight more volumes—six of them devoted to the journals—should be needed to complete the set. The full cost of the venture will run close to \$200,000.

The journal consists almost exclusively of routine business: the issuing of commissions to local justices; the administration of bonds from civil officials; the consideration of applications for remission of fines and forfeited recognizances incurred at county courts; orders on the treas-

urers to pay accounts; or the routine execution of an act of assembly allowing indulgence on taxes. The lack of detail is discouraging. Although, for example, the editor claims that "the volume is filled with descriptions of the plight of many of the incriminated poor through their appeals for relief," there is next to nothing in the petitions for remissions of fines that would add more to the record in county court journals than the size of petitioners' families or the occasional names of victims. Nor do the very summary grants for tax indulgence "shed further light on Maryland's Confiscation Act." Although the journal would be essential to such supplementary work as the compilation of the names of officeholders in the period, it is hard to see how the bulk of the journal can be seen as a widely usable historical source in its own right. And I find it impossible to see how it illuminates the massive themes outlined in the editor's introduction (a "resurgence of the democratic ideals born out of the earlier phases of the Revolution"; "renewed agitation for manhood suffrage"; "the anomaly of slavery"; or "the struggle for mass education").

I have a prejudice in favor of public records. They have escaped the process of selection that sometimes makes private collections better guides to historiography than history. Their routine quality gives them an objectivity to which personal papers cannot pretend. And their massive comprehensiveness allows them to be used for the sort of sophisticated quantification that is so badly needed. But I think, too, that it is right to ask whether a record as limited in its research value as this really calls for the broad distribution of a letterpress edition. Were it the one surviving journal of a peculiar body, one among a few governmental sources for a period in which the remaining record was scant, or one weak spot in a series of much wider general interest there would be a good case for publication. As it stands, one wonders whether there is.

This is essentially a *document inédit*—a perfectly legitimate choice for an editor to make. But the few editorial revisions are cumbersome and inconsistent. To take one example, the introduction devotes a sentence to the spelling "goal" and adds that "other misspellings have been indicated by *sic*." Why it should be neces-

sary ever to add a "sic" to a spelling like "Relegion" I do not know. But to do so only erratically is downright irritating. More important, it is never legitimate for an editor, whatever his style, to look at a manuscript quite this uncritically. When, as on page 21, the same councillors are listed both in favor of a motion and opposed to it, a note correcting the text is imperative. And it requires little judgment to see that an abbreviation as obscure as "fund. tax" should be spelled out, especially when one as common as "tcs." has been awarded a bracketed "[tierces]." One can also demand an accurate and comprehensive index: one that includes references to the occupations (shoemaker, distiller) that are among the few useful details in the applications for remission of fines; that is not carelessly alphabetized (Black, Bladensburgh, Blacklock); that does not omit all the references to the armorer of Frederick Town on pages 182-83; that has adequate subject cross-references; and that does not confine the entry under "Accounts"—in a journal largely devoted to them—to two isolated pages. If the editors are not willing to engage in the elementary clerical pedantry that makes for a good edition, then that is one more reason for surrendering these valuable, but very restricted, records to the microfilm camera and letting them be.

R. NICHOLAS OLSBERG  
*Colonial and State Records  
of South Carolina*

L. MARX RENZULLI, JR. *Maryland: The Federalist Years*. Rutherford, N.J.: Fairleigh Dickinson University Press. 1972. Pp. 354. \$15.00.

Politics were complex and colorful during the Federalist years in Maryland. Drawing from a wealth of correspondence, pamphlets, and newspapers Renzulli probes the motives and activities of political leaders for this period. His best chapters describe the intricate division over the ratification of the Constitution, with special attention to the speculation of leaders in script and confiscated property, and the evolution of a political party from the constitutional debate. The Federalist party, ostensibly led by an agrarian oligarchy, persisted from 1789 to the early 1820s despite internal factionalization—the result of regional and ideological stress—

and the growth of a popularly based opposition.

The wealth of contemporary political opinion, however, obscures Renzulli's critical vision. Since he construed party and leaders as synonomous he rarely looks beyond the actions of the elite. Terms like oligarchy or High Federalist, patterns of elite recruitment or constituent voting behavior, questions about decision making within the party, or dynamic concepts such as the meaning of an urban-rural conflict in this period are used imprecisely and remain unclarified. Indeed, he treats the Republican party entirely from a national and a Federalist perspective and explains little about its origins and activities on the state level. While he asserts that the Republicans were better organized than the Federalists, the infrastructure of both parties remains unexamined. Closer scrutiny would probably show that the Republican party was also led by an agrarian oligarchy.

As the definitive study of Maryland politics from 1787 to 1818 this book is informative but incomplete. Renzulli's handling of the material before 1800 is excellent, but such intensive interpretation does not endure for the remainder of the volume. Furthermore, no study of politics for this period is satisfactory without a detailed examination of the Republican party. Since Renzulli has explicated the behavior of the national-state political elite using traditional methods, now researchers should devote greater attention to the influence of regional competition within a state party system, to basic questions concerning elite recruitment and party decision making, to a more systematic analysis of voter support (as derived from pollbooks), and to the essential dimensions of primary concepts like oligarchy. Then, perhaps, we can better understand the impact of federal patronage on state-level politics or settle the anomaly of why areas of radical discontent during the Revolution became the backbone of the Federalist party.

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JOHN K. MAHON. *The War of 1812*. Gainesville: University of Florida Press. 1972. Pp. xii, 476. \$12.50.

American historians seem endlessly fascinated by the War of 1812, writing book after book

and article after article on some aspect of it. To them it is an important experience in the building of a nation, as well as a convenient model for testing historical interpretations. To English historians the war appears a minor episode. Occasionally they may recall, as one of them did, that "a few fir-built frigates, manned by a handful of bastards and outlaws," had injured the pride of the Mistress of the Seas.

John K. Mahon of the University of Florida is the latest scholar to turn his attention to the war. His study covers ground recently tilled by Reginald Horsman in *The War of 1812* (1969). But Horsman wrote a concise military history, whereas Mahon intended his work to be as nearly as possible definitive—a full-bodied account of encounters on land and water. Mahon believes that the past, particularly that which deals with warfare, can live only when presented in detail. So he explains in depth how and why Americans held their own and why the British failed to conquer. He analyzes tactics at the lowest level as well as high-level strategy. His theme, implied rather than stated explicitly, is that of blunder and futility; his story sketches a model of how not to use war as an instrument of national policy.

Although Mahon claims to have placed military operations "precisely within the politics and culture of the time" he has in fact written a straightforward military narrative that has in it little room for theory or broad analysis. He does suggest that American dissent over the war, and other developments outside the battle areas, affected diplomatic and military decisions. He does not, however, explore the causes of the war, mainly because it has been done often and hardly needs retelling.

Using both manuscript and secondary sources Mahon displays an impressive command of his specialized subject. He understands military affairs and is usually able to present technical data in terms meaningful to the nonspecialist. In general his prose is clear and uncomplicated. Some of his battle accounts, such as those relating the duel between the frigates *Constitution* and *Java* off the coast of Brazil, and the better-known naval encounter on Lake Erie, are especially well done. Also noteworthy are occasional bits of social history dealing with blacks and Indians. Even while fighting each

other, he reveals, Americans and Englishmen clung to their racist heritage. For example, despite the military advantages they might have gained, and the willingness of American blacks to switch to their side, British commanders discouraged the inciting of slave rebellions.

Regardless of Mahon's competence in military narrative the detailed descriptions of equipment used, of fortifications, and of battle maneuvers contribute little to large understanding. Yet the study has lasting value, for anyone interested in military affairs as well as for the scholar, as an authoritative reference work. Making good use of Admiralty Records and Canadian archives Mahon gives fuller coverage to the British side than have previous military historians. Anyone who desires a precise, connected account of a particular military operation will find it in this book. As much as any other feature this complete coverage makes the whole undertaking worthwhile. Rather than interpretation or new information, it is the book's main contribution to the literature of the subject.

ALEXANDER DECONDE  
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*Santa Barbara*

JOHN M. MCFaul. *The Politics of Jacksonian Finance*. Ithaca: Cornell University Press. 1972. Pp. xv, 230. \$9.75.

The vagaries of Jacksonian finance have baffled and divided historians and economists from Jackson's day to our own. John McFaul has enriched the literature and has clarified some of the issues, but he has not arrived at a final answer. He takes issue with Bray Hammond's widely accepted thesis that the Jacksonians were not opposed to banks or business per se; they were only against the Bank of the United States and its relatively limited circle of beneficiaries. They were themselves enterprising businessmen who simply wanted to share in the distribution of capital. If this had been the case, McFaul argues, then the state banks on which the Jacksonians relied would have joined the opposition to the BUS. His evidence that the state banks tended rather to support that institution is weakened by the number of state legislatures attempting to tax branches of the bank out of existence. Once the government



deposits were shifted to the pet banks the money certainly was used to finance business enterprise, no different for being Democratic instead of Whig.

McFaul is on surer ground when he argues that the system of pet banks under Secretary Levi Woodbury was actually moving toward central banking, with the Treasury itself fulfilling the central role and the pets as branches. This trend was abruptly halted by the distribution of the surplus revenue, which left the system without operating capital. The final blow was the specie circular, which convinced the ordinary citizen that the government did not trust the banks. He hastened to get his money out while he could, and the crash of 1837 followed. Jacksonian finance, in a word, was probably just as confused as it seems. All shades of opinion were represented within the inner circle of the party, which made a constructive policy impossible for another decade.

CHARLES M. WILTSE  
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SYDNEY NATHANS. *Daniel Webster and Jacksonian Democracy*. (The Johns Hopkins University Studies in Historical and Political Science, ninety-first series [1973], 1.) Baltimore: Johns Hopkins University Press. 1973. Pp. xii, 249. \$10.50.

ROBERT F. DALZELL, JR. *Daniel Webster and the Trial of American Nationalism, 1843-1852*. Boston: Houghton Mifflin Company. 1973. Pp. xv, 363. \$8.95.

Chronologically these two studies of Daniel Webster fit together rather neatly, the one concentrating on the years from 1828 to 1844 and the other on those from 1843 to 1852. But the two authors take somewhat different approaches.

Sydney Nathans, of Duke University, looks at Webster as an elitist politician who disliked parties and preferred "government by independent men" but had to respond to the rise of "voter-oriented partisan politics" in the Jackson period (p. 3). Nathans "analyzes his efforts to survive, comprehend, and manipulate the new politics" (p. 4). He finds that "Webster's adjustment to the issues and to the style of Jacksonian politics left him fit to cope with the voters by the end of the 1830s," but he was "less able to deal with the demands of his party comrades" (p. 6). Nathans concludes:

"Ironically, by 1844, Daniel Webster had once again become what he had been in 1828: merely a Massachusetts man" (p. 225).

Robert F. Dalzell, Jr., of Williams College, explains that he originally contemplated a "detailed analysis of the rhetoric of Daniel Webster's nationalism" but found that his rhetoric could not well be "treated separately from the rest of his career" (p. ix). Hence Dalzell has undertaken a restudy of Webster's politics along with his nationalism, choosing the period when nationalism "faced its severest challenge in America" (p. x). Dalzell sees Webster as a man "fighting simultaneously to save his political career and to defend something in which he believed most deeply" (p. xi). Believing that a "viable balance of sections, interests, and rights" was the "very essence of the American nation" Webster "devoted himself to articulating the principles of that balance" (p. xii). But in the end the "great vision" was "shattered on the paired reefs of sectionalism and democratic politics" (p. xv).

Despite their difference in approach the two books are similar in being essentially studies of Webster's role in presidential politics. The Nathans volume sticks somewhat more closely to this theme, tracing in considerable detail the man's opposition to Andrew Jackson in 1828 and 1832, his dream of a Webster-Jackson party after the nullification crisis, his unsuccessful Whig candidacy in 1836, and the frustration of his hopes for the nomination in 1840 and 1844. The Dalzell volume begins with an analysis of Webster's rhetoric in the second Bunker Hill address but, noting that by 1843 his "stock as a presidential candidate had fallen to zero" (p. 29), soon takes up the story of his renewed ambitions and continuing disappointments, with special attention to the "politics of compromise" in 1850 and to the final, bitter defeat in 1852.

Together the two books form an excellent and quite full account of Webster's political career during the quarter century they cover. Neither of the two alters the basic outlines of the familiar story, but both of them offer fresh insights and new, illuminating details. Both are based on extensive use of Webster and other manuscript collections as well as printed primary sources. The authors completed their

research before the preparation of *The Microfilm Edition of the Papers of Daniel Webster* (1971), a continuing project, under the general editorship of Charles M. Wiltse, that will lighten the work of Webster scholars in the future.

Certain views of each of the authors might be questioned. For example, Nathans emphasizes as distinctive of Webster his "dependence on patriotism as the way to secure the blessings of the traditional politics" (p. 7). But did not Jackson and the Jacksonians also appeal to patriotism in defense of the new politics? Again, Nathans implies that Webster favored a "Webster-Jackson party of patriots" because he wanted to "check the politics of conflict" (p. 73). But was Webster really more interested in that objective than in the more personal one of getting himself elected president? As Dalzell remarks in another connection, "party loyalty always mattered less to Webster than securing the object of his ambitions" (p. 55). Dalzell, for his part, makes the astonishing statement that "internal improvements could vitally concern only those producers, of whatever, who produced more than they could use themselves or sell locally—a relatively small group in America even as late as 1860" (p. 33). Were the wheat growers of the West and the cotton growers of the South producing only for local consumption? And did they constitute a "relatively small group"?

Both authors take Webster's political philosophy—his rhetorical nationalism, his concept of social harmony—pretty much as given. Neither author relates it effectively to the sectional economic interests that it served, though a close relationship would seem to be suggested by the fact that Webster had abandoned his earlier low tariff and states' rights ideas only when his constituency ceased to depend mainly on overseas commerce. Nor does either writer point out that Webster looked upon social harmony not as automatic (or as a product of individual virtue) but as contingent upon government policy and economic development, though he had made this clear in his earlier phase when he warned that protective tariffs, by fomenting manufactures, might give rise to a dangerous proletariat. Before 1828 he proposed *laissez faire* as the harmonizer, the means of avoiding class conflict; after 1828 he discovered the

proper means to be government aid to business enterprise.

To be sure, neither Nathans nor Dalzell pretends to be dealing with the prenationalist Webster, though each includes a summary of the earlier career. There is need for a new study of that earlier phase, a study conceived and executed with standards as high as those of these two books.

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ROBERT H. RUBY and JOHN A. BROWN. *The Cayuse Indians: Imperial Tribesmen of Old Oregon*. Foreword by CLIFFORD M. DRURY. (The Civilization of the American Indian Series, volume 120.) Norman: University of Oklahoma Press. 1972. Pp. xix, 345. \$8.95.

*The Cayuse Indians* is a carefully documented yet most readable history written by a surgeon-historian team that has authored other works on the Indians of the Oregon Territory. In this book Ruby and Brown focus on the events surrounding the tragic occurrences of November 29, 1847, when a group of Cayuses entered the Methodist mission established eleven years earlier by Marcus Whitman and slew him, his wife, and twelve others.

During the late eighteenth century the Cayuse Indians, a plateau people who lived by hunting and by gathering wild vegetable foods, turned to horse raising and trading. Though a small tribe they acquired considerable prestige. Then the trickle of northwest explorers and traders gave way to a steady incursion of immigrant farmers who passed along the Oregon Trail through Cayuse territory seeking land and bringing dread new diseases. What had been sporadic clashes between Indians and newcomers took on a more serious note; Whitman himself felt something was bound to happen by the time of the massacre.

The events that followed the Whitman Massacre repeat a familiar pattern. The Cayuse lost the war that ensued and became divided among themselves as to whether to shelter or to give up the leaders of the attack against Whitman. Meanwhile, the massacre was used as official justification for a statement that, by their conduct, Cayuse lands were "forfeited by them, and justly subject to be occupied and held by

American citizens." The trial and execution of five leaders in the Whitman affair were followed by a decade of scattered fighting. The Cayuse unsuccessfully resisted the loss of their lands and were finally moved onto a reservation. Some later joined in the Nez Percé war of 1877, and some sought the revival of past independence through the Dreamer religious movement. In the end the Cayuse scattered and intermarried with other Indian groups. They no longer exist as a people; it is instead the sturdy horse they bred that bears the name by which they were known.

The authors tell the story straightforwardly, apparently committed to reconstructing the history of an Indian people without taking a partisan stand. Their position, however, seems strained to me. "We mention little about white responsibility for the massacre because it concerns the Cayuse Indians only indirectly," they write, yet they refer to the more militant Indians as "wily," "hostiles," "troublemakers," or "hotbloods," thereby implying the judgment they do not overtly make. It would have been better to engage in frank speculation about the role they see the various personalities in their account as perhaps playing in relation to various segments of Cayuse opinion. As it is, the Indians are categorized simply as either "friendly" or "hostile," and they do not sufficiently emerge as a diverse people actively attempting to deal with a difficult situation.

In a way, *The Cayuse Indians* is a guide for the more complete social and political history that remains to be written. Not that this is too serious a criticism, for it is a very good guide on a little-documented subject.

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C. THOMAS DIENES. *Law, Politics, and Birth Control*. Urbana: University of Illinois Press. 1972. Pp. vii, 374. \$15.00.

The spreading practice of birth control in the United States throughout the past century has challenged orthodox morality, traditional medical practices, and, not least of all, a host of federal and state anticontraceptive laws. C. Thomas Dienes, a student of the law, has therefore chosen birth control as a case study

of the relation of legal institutions to social change. His investigation makes it readily apparent that compared with the legislatures the courts have constituted a more accessible forum for those people, such as Margaret Sanger, who have promoted the practice of artificial contraception. Consequently Dienes emphasizes the case-law history of the birth control issue in what is assuredly the most exhaustive study to date of the legal aspects of this important subject.

The author, clearly sympathetic with the birth control movement, even more clearly endorses the principle of activist jurisprudence, "an explicit policy approach to constitutional decision-making" (p. 169). Great judges, in Dienes's view, are those who exploit opportunities to effect maximum changes in the law, consistent with minimum standards of continuity and order. Thus, for example, he praises the majority and concurring opinions in *Griswold v. Connecticut* (381 U.S. 479 [1965]), despite his recognition that "*Griswold* may cause some consternation to those who seek a craftsmanlike development of principled constitutional decision-making" (p. 177).

Unfortunately, birth control is not an encouraging example for advocates of such a theory. With very few exceptions the courts have accommodated only slowly and narrowly to the changes associated with the diffusion of contraceptive practice. Moreover, it could be argued that the law has not been particularly relevant to the modern history of birth control. Most of the "landmark" cases Dienes cites merely legitimized behavior that was already widespread and accepted. With regard to birth control, in other words, legal change has lagged far behind social practice—as distinguished, for example, from the area of civil rights where activist courts have been a significant force for social change.

This book is at its best when it critically discusses individual cases, especially *Griswold* and *Commonwealth v. Baird* (355 Mass. 746 [1969]). As history it is less valuable, often relying on dated and incomplete sources for the history of the birth control movement. In fact, though the author sets out to explore the connections between the law and society, he concentrates heavily on the law, especially on the relation between courts and legislatures,

while social history, in the end, gets rather short shrift. It should also be mentioned that, throughout the book, haste is hot, change nearly always comes in tides, people get "literally raked over the coals" (p. 288), and at least one development takes place, believe it or not, "back at the ranch" (p. 275). Prose such as this is as unscholarly as it is unjudicious.

DAVID M. KENNEDY  
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DAVID F. MUSTO. *The American Disease: Origins of Narcotic Control*. New Haven: Yale University Press. 1973. Pp. xiii, 354. \$10.95.

There is gentle irony in David Musto's title for his study of narcotics in the United States, *The American Disease*, since Americans, at least for the past eighty years, have equated drug addiction with foreigners and suspect minority groups. Dr. Musto points out that the use of opium and its derivatives rose sharply in America during the second half of the nineteenth century and reached a peak in the 1890s, a decade when cocaine, too, had achieved great popularity as an all-purpose therapeutic and a favorite ingredient of medicine, soft drinks, and wines. The medical profession and proprietary drug makers share a good deal of responsibility for this rise of narcotic addiction, but, as Musto points out, the use of drugs grew far more rapidly in the United States than in any other Western country.

Many factors led to attempts to regulate the distribution of narcotics by 1900. The medical and pharmaceutical associations were seeking respectability, muckrakers were drawing attention to a wide range of social evils, and opium was associated with the Chinese minority and cocaine with Negro dope fiends. The failure of state and local laws led to federal action. The Harrison Act was finally passed in December 1914 because Southern opposition to strengthening the federal government was neutralized by an irrational fear of cocaine-crazed blacks.

During this period the question whether drug addiction was a disease or a moral problem was hotly debated, but both sides believed it was curable. By the end of World War I the moralists won out and addiction was ac-

cepted as a social evil that had to be stamped out. Addict maintenance programs were banned and the emphasis was placed upon rigorous punishment. This same climate of opinion prevailed until well after World War II when methadone programs revived the idea of maintenance. Public opinion today is once again swinging toward the view that addiction is a disease, but we are no closer to a cure than we were fifty years ago.

Musto's book is the first serious study of narcotic addiction in America, and it is a first-rate one. He shows that our policy toward narcotics has been an irrational, political one, conditioned largely by prejudices and social tensions. He clearly demonstrates that the amount of addiction is invariably exaggerated, that it is not a minority problem, that the relationship between crime and addiction has been largely one of society's making, and that addiction is a peculiar American problem. Our efforts to control the distribution of drugs on the international market simply reflect our delusion that addiction is un-American. I only hope this important book gains the widespread distribution it deserves.

JOHN DUFFY  
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College Park

HOWARD B. SCHONBERGER. *Transportation to the Seaboard: The "Communication Revolution" and American Foreign Policy, 1860-1900*. (Contributions in American History, number 8.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xix, 265. \$10.50.

Like a good many historical works, even of a specialized kind, this book presents very little that is absolutely new. Rather, it focuses upon a basic relationship that should have been emphasized long since but which somehow has not been. This is the underlying community of interest of American agrarians, American merchants, and American transportation enterprise in the development of effective routes of international trade. Likewise involved are questions of American foreign policy and even, in its broadest sense, the factor called "imperialism."

Professor Schonberger suggests that the traditional late nineteenth-century collisions between farmer and railroad baron have been

considerably exaggerated and that the story by no means should be restricted to a semimoral struggle between the deserving husbandman and the selfish Eastern capitalist. To be sure, there existed an inland jealousy of the older entrepôts—of New York, Boston, and Philadelphia—and of the already well developed trunk lines that led thereto. But the grain and cotton producer was by no means opposed to rail carriers as such, and the author takes pains to emphasize that a large part of the effort to reduce the mercantile New Yorkers to size involved the encouragement of alternative, but very similar, establishments at points like New Orleans, Galveston, and Seattle.

An important factor in the story is the astounding post-Civil War expansion of the American output of raw commodities for export, in particular grain, to such an extent as to create a chronic condition of glut in the markets of Europe. This naturally inspired a search for alternatives, notably in Latin America and the Far East, a search that produced demands for further railroad and port facilities and set off intensified discussions as to the possibility of a transisthmian canal and the advantages of a modernized navy. Made very clear is the fact that the American place in the structure of international commerce was, even so late as the turn of this century, essentially that of a producer of unfinished products. In fact, the world trade position of the United States was still of the "colonial" variety, even as the republic was assuming a clearly "imperial" posture.

This is a well-organized book: the author begins by stating his thesis; he then elaborates upon it and concludes with a comfortable summary. The scholarly apparatus is more than adequate. The errors noted are few and of the smallest consequence: Samoa is an archipelago, not an island; the Ocmulga River is usually rendered as "Ocmulgee"; and the Canadian Pacific Railway was not completed in 1886 (it was placed in full operation). This is, in short, a useful contribution to American economic history. If Professor Schonberger has perhaps pointed to the obvious, no one else seems to have had the perception to do so in quite the same way.

ROBERT C. BLACK III  
Colorado Women's College

LOREN P. BETH. *The Development of the American Constitution, 1877-1917*. (The New American Nation Series.) New York: Harper and Row. 1971. Pp. xxvi, 280. \$7.95.

There has long been a need for a fresh overview of late nineteenth- and early twentieth-century American constitutional history. The political history of the period has been subject to stimulating reinterpretation; but the character of governmental and legal thought and institutions has not yet undergone a comparable re-examination.

So one might reasonably expect to look for fresh and interesting analysis in the New American Nation series volume on the constitutional history of the 1877-1917 period. But it cannot be said that Professor Beth's work fulfills that expectation. The problem, I think, lies in his apparent inability to decide whether he is writing a work of history or of political science. In subject matter it is clearly the former; in approach it is more so the latter; in execution it falls somewhere in between.

Professor Beth's first four chapters—half of the text—examine the major political and governmental institutions of the time: the presidency, Congress, the bureaucracy, the political parties, the elective system, and the courts. His analytical mode is that of the first generation of political scientists—Woodrow Wilson, W. W. Willoughby, Frank J. Goodnow, Henry Jones Ford—who defined the nation's Constitution as the sum of its public institutions. This might have been a useful approach if it had reflected the insights of more recent political scientists such as David Truman and David Easton. But there is little of this; and space limitations prevent the author from offering more than a hurried survey of these topics and concluding that governmental power on every level was becoming more active and more concentrated.

The remainder of the book is given over to the activities of the nation's courts. Professor Beth retells the familiar story of the Supreme Court's use of substantive due process to limit state business regulation, and its failure to use that concept to protect the civil rights of Negroes. Again, his conclusions are unexceptionable—and unexceptional: that the Court was not wholeheartedly pro- or anti-business

and that its decisions had the effect of increasing its own determinative power.

The most original portion of the study examines the work of the state courts (chapter 8). Professor Beth usefully reviews a wide range of cases dealing with economic regulation and individual rights. But that he can draw from this survey only an "impression of overwhelming confusion" (p. 229) perhaps speaks more to the limitations of his approach than to the realities of the time.

Professor Beth rarely attempts to describe the evolution of judicial doctrine from 1877 to 1917 as he does with government and politics during that time. The progressive impulse of the early twentieth century had an evident impact on the presidency and on domestic and foreign policy; was there no concomitant development in the courts? There is no way to tell; the author does not ask this question, let alone try to answer it. He tells us that great constitutional change occurred between 1877 and 1917. But he has not really shown us the character of that change so far as the courts are concerned. To conclude that by the latter date "American constitutionalism remained invincibly middle class" (p. 252) begs the question whether the character of middle-class perceptions of law and government in 1917 were what they had been fifty years before. It is hard to believe that this was the case.

MORTON KELLER  
Brandeis University

ALEXANDER B. ADAMS. *Geronimo: A Biography*. New York: G. P. Putnam's Sons. 1971. Pp. 381. \$8.95.

*Geronimo*, purportedly a biography of the Apache war leader, is more so a history of the Apache nation and its relations with the United States and Mexican governments during the nineteenth century, and of the bloody contest between Apaches and Anglo-Americans for control of southern New Mexico and Arizona. The author includes sketches of Geronimo's antecedents—Mangas Coloradas, Cochise, Nana, and Eskimizin—Apache patriots who defended their homeland and throttled the American advance into the Southwest's heartland during most of the nineteenth century. Some attention

is given to the Apaches' simple life-style, their martial tradition, and their propensity for raiding the Spanish-American settlements as a source of captives, plunder, and glory. *Geronimo* is a literary tragedy, a chronicle of admirable resistance to American intrusion, an epic of ethnic destruction. The seriousness of the Apache threat was confirmed by the national government maintaining an army of 5,000 troops and 500 Indian scouts in an extended Southwestern campaign to search out and destroy Geronimo and his band of nineteen warriors. Upon his capitulation in 1886, Geronimo and his warriors with their families were sent to Fort Pickens, Florida, as prisoners of war. The captives were relocated to Mount Vernon Barracks, Alabama, in 1887 and five years later were moved to Fort Sill, Indian Territory.

The life story of the Apache leader is based largely on secondary sources and contains little that is new except the author's field work, a comprehensive study of geography of the Apache homeland. Thus on the subject of the Apache milieu he can write with certainty and authority. The descriptive passages depicting Apache haunts in northern Mexico, southern Arizona, and New Mexico rate as the most valuable part of the author's effort. Likewise, he has compiled an extensive list of printed sources on Geronimo not hitherto available. *Geronimo's* faults are extensive, the principal one being viewpoint. Occasionally it is presented from Geronimo's stance, but more often in the broader Apache nation context, shifting from this to the United States Army viewpoint with such frequency that the reader's continuing question must be "Where is Geronimo?" This maze of shifting viewpoint obscures the principal figure and occasionally loses him completely. *Geronimo* lacks essential continuity in content and, all too often, the chronology is vague. The publisher touts *Geronimo* as "unequaled in depth and scope," adding the unwarranted claim that "here is the definitive biography of Geronimo," which clearly it is not.

ARRELL M. GIBSON  
University of Oklahoma

PETER KOLCHIN. *First Freedom: The Responses of Alabama's Blacks to Emancipation and*

*Reconstruction.* (Contributions in American History, number 20.) Westport, Conn.: Greenwood Press, 1972. Pp. xxi, 215. \$10.00.

This instructive case study of Alabama blacks during the first half decade following the Civil War centers not on the actions of whites toward the freedmen but on the ways in which the Negroes themselves responded to emancipation. Consisting of a series of chapters on migration, the black family, education, churches, class structure, and politics the book presents data demonstrating that the freedmen displayed a vigorous independence and played a vital role in shaping the character of the Reconstruction experience and of the postwar black community. Thus, for example, sharecropping developed largely because the former slaves preferred it to a wage system, and despite the demoralizing effects of slavery black family life assumed a stable patriarchal character. The best chapter is the one on politics, where the author demonstrates in a striking manner that blacks operated as a largely autonomous bloc and were anything but subservient to the white scalawags and carpetbaggers in the Republican party.

The author's conclusions buttress those of scholars like A. A. Taylor, Vernon Wharton, and Joel Williamson in regard to education and the church. His discussion of agricultural labor supplements that of Wharton, Williamson, Martin Abbott, and William McFeely, though his interpretation of the Freedmen's Bureau in this connection is more favorable than other recent research. And overall the author goes far along the lines of inquiry suggested in the discussion of the black experience during Reconstruction in Harold M. Hyman, ed., *New Frontiers of the American Reconstruction* (1966).

Although neither the approach nor the conclusions are as original as Kolchin's introduction would lead the reader to believe; although the section on class structure suffers from inadequate conceptualization because the author in effect equates economic class with social class; and although the author often writes with unwarranted certitude on the basis of limited or ambiguous evidence, *First Freedom* is a welcome addition to the growing number of studies approaching Reconstruction in the

South from the perspective of the black experience.

AUGUST MEIER

Kent State University

I. A. NEWBY. *Black Carolinians: A History of Blacks in South Carolina from 1895 to 1968.* (Tricentennial Studies, number 6.) Columbia: University of South Carolina Press, for the South Carolina Tricentennial Commission, 1973. Pp. xiii, 388. \$9.95.

Malcolm X used to say that black Americans did not land on Plymouth Rock, rather Plymouth Rock landed on them. Professor Newby might have borrowed this image to introduce the central theme of his book—namely, that white supremacy crushed the life out of black Carolinians in the dismal period between the disfranchisement campaign of 1895 and the Orangeburg Massacre of 1968. "White Reconstruction" betrayed black Reconstruction as blacks in twentieth-century South Carolina succumbed to "white codes" in a "white state"—a "closed society" governed by a "gross racism." Lynching, peonage, disease, illiteracy, poverty, and powerlessness are the familiar code words that spell out a history of oppression and squalor.

The author portrays a half-century of dishonor, but he does not give us a full picture of black Carolina. He rationalizes that he could not produce a study of the black community, because, except as a "geographic expression," there was no such community. Indeed, he comes close to saying that there existed a black anti-community, bereft of leaders, plagued by parasitic institutions, and inhabited by stunted individuals whom white Carolina had "programmed" as "good darkeys"—New South Sambos.

In short, the offspring of white supremacy is black pathology. There is truth here, of course, but it is partial truth. Newby looks on black religion as an opiate that "engaged Satan in the fiercest kind of combat" but found "the white man too menacing to challenge." A more subtle analysis might have perceived that within black religion Satan was the white man. Newby describes the 1903 Columbia streetcar boycott as a failure, which it was, but he makes no effort to analyze the move-

ment and its legacy for twentieth-century black protest. Similarly we learn too little about the place of Booker T. Washington in the history of black Carolina; the same can be said for the NAACP; Marcus Garvey is absent even from the index.

Admittedly the Garvey movement was not to Charleston what it was to Harlem, but Newby's failure to mention the greatest grass-roots movement in Afro-American history is symptomatic of his larger failure to penetrate the substrata of black Carolina. His advice that black Carolinians, like white Populists, should have "raised less corn and more hell" seems a gratuitous non sequitur following his presentation of the masses as the broken victims of racism. He depends entirely on published materials, mostly the public documents and white newspapers of South Carolina. Collections like the Booker T. Washington and NAACP Papers, as well as oral and folk sources, would have enriched his study. Even a section of good photographs would have warmed the clinical tone of the book.

These misgivings ought not to detract from the considerable virtues of Newby's work. In venturing beyond the patented years, 1865-1900, he has launched a second generation of state studies on Southern Negroes. There is a Myrdalian completeness to his statistical profile of black Carolinians, and his unsparing account of racial injustice is vital to these Tricentennial Studies. One can appreciate the book as it is while wishing for the book that it might have been. Professor Newby has shown us the black Carolina that suffered, which is important; but not the black Carolina that endured, which is more important.

WALTER B. WEARE  
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Milwaukee

RICHARD D. CHALLENGER. *Admirals, Generals, and American Foreign Policy, 1898-1914*. Princeton: Princeton University Press. 1973. Pp. viii, 433. \$16.00.

This long-awaited volume by a pioneer in the field of civil-military relations analyzes the ways in which the officer corps viewed international affairs and the manner in which the diplomats utilized armed force to bolster foreign

policy. It makes no pretense of being a full-fledged study of national security procedures, such as one might attempt for the period after 1945. It concentrates on the reciprocal relationship between the presidents and secretaries of state on the one hand and the uniformed services on the other. It says little about Congress and the committees that deal with the army and navy. Challenger finds that most officers sought not a larger share in policy-making but rather clearer guidelines within which to plan for war and peace. In an era of imperialism, military and civilian leaders shared many assumptions about the world in which they lived, the power alignments in Europe and Asia, the use of force, and the need for stability in the so-called backward areas. Concern for the nation's security often led to exaggerated fears among generals and admirals, but a similar lack of realism could be found in the White House and State Department as well as in legations and consulates abroad. The author concludes that interservice collaboration before 1914 far exceeded that between the civilian and the military.

Arguing that personalities, not institutions, shaped policy, Challenger tells his story by presidential administrations. Of the four long chapters that comprise three-fourths of the text two are devoted to McKinley and Roosevelt and one each to Taft and Wilson. Besides a full introduction and conclusion there is an initial chapter that explores the world view of the officer corps, the extent of interservice cooperation, communication channels between civilian and military, and contemporary practices in Europe. The volume rests largely on unpublished records in the State, War, and Navy Departments and on a wide array of personal manuscripts. Particularly rewarding are the papers of the Joint Army-Navy Board and the Navy's General Board, the subject and area files in the Naval Records and Library Collection, and the reports reaching the secretary of state from commanders maintaining a naval watch in East Asia and the Caribbean. The omission of these last reports in the *Foreign Relations* volumes has led historians to overlook one military contribution to foreign policy. Thus, Rear Admiral Joseph B. Murdock, reporting from Shanghai in 1911-12, offered a different—and often more realistic—



view of the Chinese revolution than did Minister William J. Calhoun in Peking. Similarly, Secretary of State Philander C. Knox came to pay more attention to the dispatches of Rear Admiral William W. Kimball from his squadron in Nicaraguan waters in 1909-10 than to those of the often hysterical or self-serving departmental representatives in that strife-torn country.

To diplomatic historians, who usually ignore the records of the armed forces, this volume should add to their knowledge of many events—the Boxer revolt, the Venezuelan blockade, the uprising in Panama, the turmoil in Central America, the tensions with Japan, the downfall of the Manchus, and the intervention in Mexico. To those who are not familiar with recent work in military history Challenger provides the best single source of information on such continuing problems as the defense of the insular outposts, the quest for naval bases, the location of the battle fleet, the evolution of war planning, and the use of gunboat diplomacy. Because the book has been long in preparation some of its findings have been anticipated by William R. Braisted, Arthur S. Link, Charles E. Neu, and others, while its main outlines are already known to the small but growing band of specialists in national security policies. Nevertheless, in the questions it poses and the sources it taps this work can serve as a starting point for similar but much more needed studies of the periods 1921 to 1939 and—it is hoped, before too long—1939 to 1950.

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C. ROLAND MARCHAND, *The American Peace Movement and Social Reform, 1898-1918*. [Princeton:] Princeton University Press. 1972. Pp. xix, 441. \$16.00.

A proper understanding of this book requires a full appreciation of the relationship between peace and domestic reform movements suggested in the title. Anyone familiar with the overlapping leadership and similar tactics of the peace and civil rights movements of the 1960s is aware of the potential rewards from careful study of such relationships; Marchand has utilized this approach on a broad scale

for the so-called Progressive era. In separate chapters he describes the involvement of international lawyers, businessmen, "scientific" educators, women suffragists, social workers, labor leaders, and churchmen in the American peace movement. The author skillfully analyzes a hodgepodge of more than one hundred individuals and various groups. The result is a study of extraordinarily diverse approaches to foreign policy questions. Marchand emphasizes that the foreign policy views of the peace spokesmen derived from their professional and occupational goals as well as from their prior notions of domestic society and social reform. He explores this dimension so exhaustively and persuasively that it should never have to be done again.

Marchand's approach is interesting and plausible, but it also creates methodological and interpretive problems. His focus on the relationship between peace and various domestic reform concerns inevitably highlights minor figures in the peace movement who were deeply involved in other causes while slighting many of its central actors who almost exclusively promoted international reform. We learn much more about Carrie Chapman Catt, for instance, whose priority was suffrage than about a dedicated pacifist like Emily Greene Balch. He also exaggerates the monolithic, establishment characteristics of the peace movement for the 1898-1914 years. Some historians of anti-imperialism might object to his emphasis on the similar viewpoints between anti-imperialist peace advocates and those imperialists who joined the peace movement after about 1900. His interpretation is not surprising, however, because he perceives little difference between the anti-imperialists' promotion of the expansion of American ideals abroad through the use of righteous example and the imperialists' willingness to extend them by force. A convincing evaluation of the peace movement at the end of the century will require more research on what the peace leaders actually said and did during the debates over war and imperialism. Marchand also neglects divisions over military preparedness before 1914 and probably overstates the similarities between the World Peace Foundation and the Carnegie Endowment for International Peace.

One can readily agree with Marchand's con-

clusion that the peace movement was a "second-class" reform if the standard of judgment is absolute pacifism. But it is an open question whether the quest of the political internationalists for a League of Nations constituted a second-rate movement. Marchand's preoccupation with social relationships unfortunately inhibits any extensive discussion of the political impact of that or any other "peace" campaign on foreign policy issues. Finally, it may be that the weakness of the peace cause stemmed less from competing domestic loyalties than from more fundamental forces in American society—in particular, the self-confident nationalism of the era and the absence of any prolonged military crisis that might have prompted widespread public revulsion against the sanctity of the nation-state.

Many of these queries are admittedly debatable, and scattered remarks in the book indicate that Marchand is at least aware of some of them. It is the great merit of this book that it provokes so many questions on the study of peace movements. Fully researched, boldly conceived, lucidly written, and forcefully argued, Marchand's volume is an important contribution to the growing literature on American peace movements.

DAVID S. PATTERSON  
Rice University

WALTER JOHNSON, editor. CAROL EVANS, assistant editor. *The Papers of Adlai E. Stevenson*. Volume 1, *Beginnings of Education, 1900-1941*; volume 2, *Washington to Springfield, 1941-1948*. Boston: Little, Brown and Company. 1972; 1973. Pp. xxi, 586; xiii, 620. \$15.00 each.

The career of Adlai E. Stevenson well exemplifies what persistent vitality the principle of hereditary, one might almost say territorial, aristocracy retains in American politics. The son of a secretary of state for Illinois, the grandson of a vice-president of the United States, the great-grandson of Jesse Fell, Adlai Stevenson could trace—and did trace, with a good deal of modest oratorical reiteration—his family's prominence in the public life of Illinois over virtually the whole of the state's political history. Nothing could be less indicative of such an inheritance than Adlai's accident of birth—in Los Angeles, of all places. But this was swiftly corrected by his return,

at the age of six, to the family heartland at Bloomington, where the *Daily Pantagraph* was long established as a Stevensonian journalistic fief. His Middle Western upbringing was a prelude to Eastern schooling: at Choate and Princeton, with both of which there were family connections, then to the Harvard Law School, and so back, nearly full circle, to the La Salle law firm in Chicago in 1927.

It is not the least virtue of the first volume of this impressively comprehensive collection of *Papers* that it provides full material for the reconstruction of this background to Stevenson's later career. Its importance for him, in endowing him with an easy assurance of springing from the central stock of his region's, indeed his country's, political and moral leadership, becomes abundantly evident. The letters of childhood and youth, as graphically as snapshots in a family album, set Stevenson as the favored (but not spoiled) son of a devoted family, which in turn occupies an assured (but also earned) place in the life of a basically stable community. Their very ordinariness, their straightforward, predictable reactions to the challenges and pleasures that American life offered to a young man of average attainments in above average circumstances make—not indeed for very lively reading—but for a proper comprehension of the roots of Stevenson's liberalism. For this liberalism, from which a decade of anxious and harried citizens, from the urban intelligentsia to the rural poor, drew sustenance and courage, was not nurtured in youthful rebellion or frustration. It was a natural expression of an inherited tradition that Stevenson sought not to flout but to fulfill. When his turn came he, too, married into an old Chicago family, his children went East for their education (and, when in London, to Harrow), and he accepted rather than solicited the burdens and opportunities of public life.

The familiarity of this Whig tradition and setting (one is reminded repeatedly of the correspondence and career of the youthful Franklin Roosevelt) necessarily robs the greater part of the first volume of these *Papers* of any element of novelty or surprise. Indeed as one item of misspelled *juvenilia* follows another it is tempting at moments to inquire whether they fully earn their keep in these admirably

printed, spacious pages. The editors must often have had to wrestle with such a question themselves. On balance the reader, even as he exercises his basic human right to skip, must applaud their decision to go for completeness, especially since it is not apparently being bought at the price of excision elsewhere. (The decision to print only a selection of the letters from Stevenson to his first love, Miss Birge, was presumably dictated by other considerations.) One thing that does emerge very clearly from these letters is the close affection between son and mother and its unmistakable tinge of possessiveness. (Mrs. Stevenson's coming to live in Princeton during his second year is strongly reminiscent of Sara Delano Roosevelt's similar infliction on her poor son at Harvard in the 1900s.) The correspondence also demonstrates the peculiarly intimate rapport between Adlai and his sister; it is clear that no adequate life of Adlai can be undertaken without an understanding of the remarkable personality of Mrs. Ives.

In 1928 Adlai was writing to Miss Birge, "I've become one of the standardized earnest young men whom I used to despise so whole-somely" (vol. 1, p. 201). It is tempting to credit the challenge of the New Deal for breaking him out of his corporation lawyer's shell. Unfortunately the letters surviving from his year at the Agricultural Adjustment Administration are too few to substantiate this theory, though they do make it evident that his New Deal, well represented by the George Peck faction at the AAA, was far from a revolutionary experience. By July 1934 he was ready to "go back to Chicago and finish what I started—make my place etc" (vol. 1, p. 258). Nor does the ensuing quinquennium as a "Chicago civic leader" give much indication of what lay ahead; the reader will quickly weary of the succession of charming, light-weight, carefully uncontroversial chairman's introductions at meetings of the Chicago Council on Foreign Relations.

It is in fact the challenge from abroad, not at home, that seems to have made Stevenson "come out" and grow up. The events of 1940 and 1941 and his work with the William A. White Committee elicit a new kind of commitment and an altogether sharper response. When volume 2 opens, six months before Pearl

Harbor, he is ripe for the unquestionably formative experience of being assistant to Frank Knox, secretary of the navy. There is excellent reading in the papers of these years, ranging from some unusually penetrating strategic and logistical assessments to the lively diaries that Stevenson kept while accompanying the secretary on service missions and subsequently on his Foreign Economic Administration mission to Italy.

The failure (on which the *Papers* are very thin) on Knox's death to secure control of the *Chicago Daily News*, or even to protect it from the emasculation that quickly succeeded its sale, meant that the way into public life via journalism was substantially closed. Yet it was still true, as Stevenson had written in 1941, that he never "fancied" himself "as a combatant politico" (vol. 2, p. 26). In these circumstances it was fortunate that Washington never ceased to coax him back to service in the executive branch, and that despite the intermittent obligations of his law practice he never failed to respond.

So began the period of Stevenson's service with the State Department and the infant United Nations, admirably documented here with letters, diary extracts, speeches, and official statements, which cumulatively build up a picture of these formative years that no historian of the UN can ignore. Only the San Francisco Conference itself is passed over in virtual silence; it was an unhappy meeting for any American P.R.O. But on a host of other topics—Chicago's bid for the UN site, the Russian role in the Preparatory Committee—the *Papers* are highly illuminating.

Despite it all, Stevenson's forty-seventh birthday found him "restless" and "dissatisfied." "How can I reconcile life in Chicago as lawyer with consuming interest in foreign affairs?" (vol. 2, p. 365). The answer came, oddly and in the *Papers* not wholly comprehensibly, via the governorship of Illinois. But it is clear that something ignited him "as a combatant politico." One senses here a new joy in combat and a new release.

The intrinsic interest of these *Papers* has left all too little space for comment on their editing. But this is in fact the highest tribute. So careful and unobtrusively helpful is the editors' discharge of their obligations that the

reader's attention is kept focused where the editors would clearly wish—on the *Papers* themselves. These two handsome volumes are a monument not only to Adlai Stevenson but also to Professor Johnson and Miss Evans and constitute a triumphant inception of an admirably planned and executed undertaking.

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Oxford

RAY ALLEN BILLINGTON. *Frederick Jackson Turner: Historian, Scholar, Teacher*. New York: Oxford University Press. 1973. Pp. x, 599. \$17.50.

As each age, in the words of Frederick Jackson Turner, "writes the history of the past anew with reference to the conditions uppermost in its own time," so what others write of Turner describes them and their times as well as him. Since virtually all American historians—even Walter Prescott Webb, who said that he had not read Turner when he developed his own interpretation of American history and published his first books—are in some degree Turner's intellectual heirs, what they see in him has varied widely. Thus we have had views of Turner as a romantic and as an analytical historian, as monist and as pluralist, as apostle of democracy and of imperialism. Ray Billington says (p. vi) that he chose to write Turner's biography as the biography of a college professor and that he derived this book from a draft of two thousand pages that is available for fuller reference at the Huntington Library. In the ten years since Billington left Northwestern University to go to the Huntington as senior research associate, he has also produced three other books about Turner, *America's Frontier Heritage* (1966), an appraisal of Turner's interpretation of the frontier and of the views of his critics; *"Dear Lady": The Letters of Frederick Jackson Turner and Alice Forbes Perkins Hooper, 1910-1932* (1970); and *The Genesis of the Frontier Thesis: A Study in Historical Creativity* (1971). The biography and its predecessors are not so different from other books as they might have been if other writers on Turner (such as Richard Hofstadter, in *The Progressive Historians* [1968]; Wilbur R. Jacobs, in *The Historical World of Frederick Jackson Turner*

[1968] and other works; and Howard R. Lamar, in *Pastmasters* [1969]) had not drawn on Billington's work, especially on his articles. But readers who have heard Billington read papers on Turner will recognize distinctive interpretations, emphases, and style. While Billington apparently has used all published accounts of Turner and many collections of manuscripts (the author describes forty-three of "the most important" apart from four collections of Turner's papers, which must constitute a record in the genre of biographies of college professors), the book, like its predecessors, is much a product of this time, of the vast resources of the Huntington Library, and, above all, of a historian committed to the significance of the frontier in American history as Turner described it. It will not take the place of the many published recollections of Turner by his students or of the book that Fulmer Mood or another student might have written, analogous perhaps to Ralph Barton Perry's *Thought and Character of William James* (1948), but it is difficult to imagine anyone else now retracing life and labors in such detail and with the enthusiasm for the task that makes this long book engrossing reading.

Thirty-five years ago Charles Beard, writing on *The Frontier in American History* in a series of essays that the *New Republic* (92 [1939]: 359-62) published on books that changed their readers' minds, disagreed with Turner's interpretation of the role of the frontier but paid tribute to him as prophet of new directions and dimensions in historical research and as a selfless and inspiring teacher. Beard wrote that "personally Turner was one of the most modest and diffident scholars ever produced in America." Billington does not quite try to turn that estimate around but goes to considerable pains to justify Turner's theories and interpretations by standards of later generations: "his presentations [in diplomatic history] were so correct, that they were immediately absorbed into textbooks as part of the acknowledged pattern of history" (p. 170); he "came to a surprisingly correct conclusion concerning the frontier process" (p. 456). The one area of scholarship where Billington seems to me to be overly severe is historical statistics (pp. 468-69); here he draws on some of Turner's own criticisms of the maps that he used in

writing on the United States of 1830-50. Criticism is natural enough in an age when social scientists have excelled—in part, perhaps, because of pushes that Turner gave to them—in the mechanics of measurement, in what the late Pitirim Sorokin called quantophrenia. But do not present-day cliometricians still look back with respect to Turner and his students, from Orin Grant Libby ("A Plea for the Study of Votes in Congress," *AHA Annual Report* [1897]) to Merle Curti? Turner's enthusiasm for making correlations by precincts and wards in the Wisconsin Domesday Book as Joseph Schafer proposed it (1920) seems statistically more sound than the compilation of demographic and electoral data by counties at the Inter-University Consortium for Political Research.

At the same time, Billington, whose years of early retirement from teaching have been so different from Turner's, cannot forgive Turner, as Beard did, for not writing more books, and Turner's dispersion of his efforts is almost the main theme of the book. Even in his declining years at San Marino, "it would have been far better if he had had the will-power to refuse . . . invitations to write and speak and consult" (p. 406). Beard's view of Turner's output ("That was in truth enough for one man to do in a lifetime.") was close to that of Carl Becker, who said that "history, as Turner [conceived] it, [was] not well adapted to quantity production" (Becker in Howard W. Odum, ed., *American Masters of Social Science* [1927]: 312, 314, 317). Further, Billington refutes Beard's picture of modesty, telling at length of Turner taking private pleasure in laudatory book reviews while evading his commitments. Perhaps such problems are insoluble, especially if, as Billington suggests, Turner's uncompleted last books lacked the originality of his early essays and if he lost touch with the social sciences, from which he had drawn hypotheses and method in his youth. Turner may have been, far from Tolstoy's historian as a deaf man, like a man with very good hearing who in his best years spent much time listening to questions that many others were asking; the answers that he gave, short as they were, have had more influence than those of colleagues who efficiently insulated themselves from interruption.

Turner's twenty-nine years as a student and teacher at Wisconsin account for most of the narrative part of the book (pp. 18-57 and pp. 82-307, as against pp. 309-415 for his twenty-two years at Harvard and after), as they do of most memoirs of him. We learn much of Turner as citizen and leader of the academic community in president-making, in departmental affairs, in extension teaching. When he left Wisconsin, it was to help the university by showing the regents the consequences of their attacks on it. "Turner would have sold his soul to stay in the Madison that he loved" (p. 302); he was not dedicated to Harvard and was not at home at Cambridge (p. 385). Billington apparently is not impressed by Hofstadter's suggestion that Turner went to Harvard hoping that a change of scene would help him to write more, and that Cambridge offered a livelier atmosphere, a new intellectual frontier. (According to William B. Hesseltine, Mrs. Turner told him that her husband never regretted the move.)

Telling much of Turner's personal affairs, Billington does not give so clear a picture of either university or of Turner's associates, including students whose ideas, like Carl Becker's, are not easily encapsulated. Broad strokes sometimes oversimplify background, and peripheral details falter. Wisconsin in the 1890s was "a backwater college . . . overshadowed by the great eastern universities" (p. 142), though when Woodrow Wilson, described as a member of the department of history at Princeton, tried to persuade Turner to leave it, he had to apologize for Princeton's library (p. 152). That President Van Hise encouraged the faculty at Wisconsin to favor humanistic above practical research (p. 292) seems an overstatement of his defense of the humanities and social studies. The effect of exemplary research is slightly marred by typographical errors and slips in names and identification of minor characters. But perhaps Turner's students and Turner's place with them in the changing world of scholarship call for still another book. As a whole, this one is a major event in the history of higher education and of American historiography.

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ROBERT D. CUFF. *The War Industries Board: Business-Government Relations during World War I*. Baltimore: Johns Hopkins University Press. 1973. Pp. x, 304. \$13.50.

Although industrial mobilization was one of the most significant aspects of America's World War I experience historians largely neglected it for over half a century. This impressive monograph deals with an important aspect of that experience by examining the role of the War Industries Board (WIB) in coordinating the war efforts of government and industry.

Cuff's well-researched study does much to illuminate the development, operation, and dissolution of the WIB. He also provides new information about its predecessors, the Council of National Defense and the Munitions Standards Board, as well as the war service committees composed of businessmen who represented individual industries. Most of the book is devoted to an intensive examination of the WIB's operation, especially the formulation of industrial policies, priorities, and price fixing. Throughout his discussion Cuff emphasizes that Bernard Baruch and the WIB did not dictate to big business. Rather, they were subjected to a variety of pressure groups and ideologies, including those of businessmen who hoped to utilize the war crisis to attain centralized industrial stabilization. Although the author is sympathetic to Gabriel Kolko's views concerning business-government relations he concludes that businessmen in World War I did not achieve the centralized institutional order that they sought and that other interest groups acted as countervailing powers.

Cuff has dug deeply in primary sources. He has examined the records of the WIB in the National Archives and dozens of manuscript collections relating to important political leaders and administrators. These materials he supplements with scores of newspapers and trade periodicals. Without doubt Cuff has produced the most authoritative volume available about the WIB and has filled a void of many years.

And yet even appreciative readers will not find this a wholly satisfying study. In part, it may be a matter of *Weltanschauung*. Cuff is so intent on demonstrating what was wrong with the WIB that he tells his readers very little about what was right. After all, despite many weaknesses, industrial mobilization in

the United States during World War I did achieve unprecedented goals. Its speed and magnitude completely confounded the predictions of the German General Staff, which had not expected effective American mobilization before 1920. Moreover, while Cuff does well in delineating some of the ideological considerations that governed war mobilization in the United States he hardly touches on the influence of technology in shaping government-business relations. The impact of ideology may be lessened when it is remembered that technological imperatives were operating not only in the United States but also in other industrial societies as diverse as England, France, Germany, and Japan, producing similar bureaucratic tendencies. The American experience must be measured by a worldwide rather than a purely national context. But if this is not a definitive work about the WIB it is the most informative yet to appear, and it can be read with profit.

GERALD D. NASH

*University of New Mexico*

ROBERT SOBEL. *The Age of Giant Corporations: A Microeconomic History of American Business, 1914-1970*. (Contributions in Economics and Economic History, number 7.) Westport, Conn.: Greenwood Press. 1972. Pp. xiii, 257. \$9.00.

General business history, as distinct from the study of corporate administration or accounts of single companies, is rapidly taking shape as a subdiscipline of American history. In the last two years the first comprehensive business text, two analyses of nineteenth- and twentieth-century structure and management, and Robert Sobel's "microeconomic" history from 1914 to 1970 have appeared. The latter is devoted to a rather detailed study of the external history of giant corporations, their adjustments to booms and depressions, their relations with government, and their mergers and conglomerations. For such information it is the best factual reference work that has appeared. Since the author has expressly avoided theory and large generalizations, the book is devoted chiefly to narration of events.

But no one can write history without generalizing, and the vulnerability of some of his conclusions illustrate the relatively primitive state of business historiography in which little

has been settled and there is no great store of established knowledge. A few examples may suffice. His interpretation of business in relation to the National Industrial Recovery Administration and General Hugh Johnson differs markedly from that of Louis Galambos in *Competition and Cooperation*. Sobel avoids the problems of the average American business, which is very small, but his generalization that "the message was clear: become large, merge or go out of business" (p. 210) is not borne out by the statistics showing rapidly increasing numbers of units right up to 1970. In part, his aphorism may come from the fact that he devotes much more space to manufacturing, which employs less than thirty per cent of Americans and is the chief area of concentration of control, than to any other type of business. The book also has a more optimistic quality than it might have had if written a couple of years later, a problem no author can guard against. The poor performance of the United States relative to the other leading industrial nations in both increase in income per capita and new consumer goods technology has suddenly become much more evident.

No author of a general book in a new field can avoid debatable interpretations, however, and they scarcely detract from the value and utility of Professor Sobel's book. It is a pioneer contribution to synthesizing the history of large companies since the First World War.

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GIUSEPPE FIORI. *Antonio Gramsci: Life of a Revolutionary*. New York: E. P. Dutton and Company. 1971. Pp. 304. Cloth \$8.95, paper \$3.45.

Antonio Gramsci (1891-1937), an impoverished, hunchbacked Sardinian intellectual, was a co-founder of the Italian Communist party and one of the principal victims of Fascist repression. He died in 1937 of tuberculosis and other complications aggravated by his ten years of imprisonment. Posthumously he has come to be regarded by the New Left as probably the most original interpreter of Marxism to emerge in Western Europe during the twentieth century.

The time is propitious, therefore, for this fine translation of Giuseppe Fiori's excellent

biography of Gramsci. The original Italian edition was brought out in 1966 by Laterza of Bari. Gramsci has found a well-qualified biographer. Fiori was born in Sardinia in 1923 and took a law degree at the University of Cagliari. Later he worked as a radio journalist and as a cinema critic for the chief Cagliari daily. He also traveled widely in Europe, writing for such Italian periodicals of radical orientation as *L'Espresso*, *Il Mondo*, and *Il Ponte*. He has also written two documentary novels about life in Sardinia and a study of contemporary Sardinian banditry, *La Società del Malesere* (1968). Fiori was in a much better position than any non-Sardinian to probe into and interpret Gramsci's island background. In addition to the interviews, Fiori consulted most of the published documentary sources relevant to Gramsci's career. While these are carefully footnoted in the Italian edition, for some reason they have been omitted in the English version, though it contains an updated portion of the original bibliographical essay. The index is confined to names of persons mentioned. Tom Nairn, the translator, is an Englishman. He has wisely recast complicated Italian prose into straightforward English, and he deserves particular praise for his adeptness in clarifying the often abstruse dialectical arguments and vocabulary employed by Gramsci—a mode of philosophical discourse that sometimes seems almost unintelligible to the English-speaking world.

The biography gets off to a somewhat slow start in Sardinia but rapidly becomes more absorbing as the author sympathetically describes Gramsci's cultural development at the University of Turin and his conversion to socialism during World War I. Fiori devotes careful attention to Gramsci's founding of the Turin newspaper *Ordine Nuovo* in May 1919 and his advocacy of the new "shop-steward committees" (*commissioni interni*) as the Italian revolutionary counterpart to the Russian soviets.

In May 1922 Gramsci was named Italian representative to the Comintern executive committee in Moscow. While there, he met and married a very neurotic young woman, Julia Schucht, who was to bear him two sons. His family life was to be awkward in the extreme.

On his return to Italy in 1924 Gramsci took over leadership of the Communist party from Amadeo Bordiga. The political differences be-

tween Gramsci and Bordiga, as well as with numerous other communist leaders, are very clearly delineated by Fiori. Although he had been elected to parliament, Gramsci was arrested by the Fascists in November 1926 and condemned to more than twenty years' imprisonment. During his years of incarceration Gramsci found himself increasingly at odds with the official policy proclaimed by the Comintern in 1928 (and backed by Palmiro Togliatti) that forbade any communist collaboration with Social Democrats in the revolutionary struggle against fascism. Gramsci was politically isolated; from prison he could not prevent his party from blindly obeying Stalin's orders. Meanwhile, he was also cut off ever more from his family. What sustained him through these tragic years was his reading and writing, carried out under the most trying conditions. The preservation and publication of his influential prison notebooks more than a decade after his death was to be a remarkable example of the triumph of mind over matter.

CHARLES F. DELZELL  
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CHARLES P. LARROWE, *Harry Bridges: The Rise and Fall of Radical Labor in the United States*. New York: Lawrence Hill and Company. 1972. Pp. xi, 404. Cloth \$8.95, paper \$3.95.

This book is the product of a decade of inquiry into oral and written sources from Hyde Park, New York, to Hawaii. Bridges refused to be interviewed (although making available his union's records), criticized the book in manuscript, and blasted it after publication in a letter to the *New York Times Book Review*. Here, one would suppose, must be a study both thorough and critical of the most prominent labor radical of the past generation. I think not. To take small things first, the reader is put off by an abundance of typographical errors and by two different versions of an important quotation from Bridges (pp. 126, 351).

These minutiae reflect a more substantive carelessness. On those rare occasions when Larrowe takes note of what was happening in the labor movement nationwide he is misleading and superficial, as when he characterizes the 1930s as a decade "without precedent" for its violence in American labor history (p. 32) or

feels obliged to apologize for antilabor actions by President Roosevelt (pp. 25, 106). Above all, *Harry Bridges* fails to come to grips with the problems posed by its subtitle: what kind of radical was Bridges, and why did the radicalism fade?

One can understand that Larrowe would not wish to be the instrument for Bridges's deportation after the United States government had failed so many times. Yet Larrowe is coy to the point of evasiveness about Bridges's relation with the Communist party. Let us suppose (although Larrowe never says so in so many words) that Bridges did not belong to the party. Explanation is still required for the fidelity with which the union leader followed each twist and turn of the party line: softening his stand toward the New Deal in the mid-1930s, denouncing it during the "phony war" period from September 1939 to June 1941, and then proposing labor-management cooperation to win World War II. Too often Larrowe is content to dispose of Bridges's politics by quoting his testimony on the witness stand. But it should be the business of the historian to tell the truth more clearly than a defendant in a trial is likely to be able to.

Similarly Larrowe alludes—in a dependent clause within a sentence about something entirely different (p. 130)—to the fact that in 1936 Bridges promised the shipowners that his union would stop "using job action," that is, wildcat strikes. In the history of every CIO union the leadership's promise to repress unauthorized strike action by the rank and file was the single most critical step in the union's retreat toward conservatism. Murray and Reuther took this step in 1937, "radical" Harry Bridges in 1936. Why? If Larrowe could explain this, he might be able to offer something better than old age or Michels's iron law of oligarchy in explaining Bridges's collaboration with the shipowners after World War II.

Perhaps the last word belongs to Bridges. "Collective bargaining is class collaboration," he testified at a deportation hearing. If by this is meant that the "fall" of radical labor is best explained by the conservatizing influence of collective bargaining itself, Bridges's own life exemplifies his words.

STAUGHTON LYND  
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*Complete Presidential Press Conferences of Franklin D. Roosevelt.* Twenty-five volumes in twelve. Introduction by JONATHAN DANIELS. Volumes 1-2, 1933, pp. xvii, 407, 593; volumes 3-4, 1934, pp. 441, 304; volumes 5-6, 1935, pp. 395, 372; volumes 7-8, 1936, pp. 308, 212; volumes 9-10, 1937, pp. 467, 443; volumes 11-12, 1938, pp. 499, 326; volumes 13-14, 1939, pp. 484, 397; volumes 15-16, 1940, pp. 605, 395; volumes 17-18, 1941, pp. 423, 403; volumes 19-20, 1942, pp. 398, 316; volumes 21-22, 1943, pp. 416, 254; volumes 23-25, 1944-1945, pp. 279, 281, 121. [The pagination is that of the original transcripts; index to each volume is not paginated.] New York: Da Capo Press. 1972. \$40.00 each, \$450.00 the set.

The De Capo facsimile edition of twenty-five volumes of the press conferences of President Roosevelt, in twelve bound volumes, presents the raw materials of history in a readily available form to which students seldom have such ready access. Here is a direct copy of the press conference transcriptions, with all the typographical errors and inaccuracies intact, without benefit of editorial correction or notes and devoid of supplement beyond the index in each volume. The introduction by Jonathan Daniels is the only editorial contribution beyond the utterances of FDR.

It is right to call this collection a great mass of the raw material of history, and it is honest to acknowledge that much of it is very raw indeed and a lot of it not very material. But it will serve many a scholar until an annotated version appears, with the guidance that can be given by editorial footnotes and chronologies. No one will write a history of this period without consulting it, as surely as no one will write an adequate history of the period by consulting it alone.

This is a unique document that records a unique institution in American government—the Roosevelt press conference. There was nothing like it before in American government. Since the administration of Harry S. Truman there has not been anything remotely resembling it. It is not likely that there will be anything just like it in the future, because the exact combination of circumstances that existed from 1933 to 1945 are not likely to be repeated. The sheer size of the press gallery began to alter the Roosevelt sessions toward the end. (As Jonathan Daniels notes in the preface there were only 279 accredited members of the press

galleries in the twenties, while at Roosevelt's death there were 695 active members of the gallery, plus a new radio gallery of 102, and 89 accredited photographers.) Once President Eisenhower opened the press conferences to television nothing remained of the FDR format, with its cozy off-the-record and background rules. History, moreover, despite its predilection for repetition, is not likely to repeat the atmosphere of the Roosevelt years, when a president could count on newspapermen as allies, first in a war against depression and then in a war against the Nazis and Fascists.

It is not wise to measure the communicativeness of governments by the number of press conferences held, as some have been tempted to do. There are press conferences and press conferences, and Roosevelt's press conferences were as notable for the skill with which he avoided communication when it was not convenient as they were for the effectiveness with which he communicated when he wished to do so. Roosevelt held 998 press conferences, Truman 322, Eisenhower 193, Kennedy 64, Johnson 126, and Nixon 23 in his first thirty-nine months.

To his contemporaries Roosevelt seemed to present at his press conferences the figure of a public man of very liberal inclinations, bold and venturesome and even indiscreet; but to read these pages now (in the light of what has since transpired) is to gain a rather different image—the image of a man who was essentially conservative, cautious on some occasions even to timidity, and respectful of current opinion as long as it seemed likely to prevail.

These transcripts show how cautiously he moved to disclose his support for a whole string of positions on which he later became articulate: guarantee of bank deposits, Nazi persecution of Jews, lynching, federal aid to education, intervention in Europe, deficit spending, veterans' benefits, the United Nations, the recognition of de Gaulle, the treatment of postwar Germany, and the Italian settlement.

His reputation for candor with the press rests more on the manner with which he managed the press conferences than on the meat in them. He took the nation, step by step, toward a military confrontation with the Axis powers

without once acknowledging that we were drawing closer to war until after Pearl Harbor. He prepared the nation's armed forces for war while events prepared the mind of the country for that eventuality. Both were ready when the time came. The brightest and best brains did not always give him the best advice, as he ruefully acknowledged when the steel crisis arose to threaten military preparations.

He was most adroit, and least candid, in fencing with reporters over the third term and the fourth term. He did not hesitate to brand reporters as dunces for quite legitimately inquiring about his intentions. His evasiveness exhausted the psychological hurdle of a third term before the campaign even started. He skillfully refused to be drawn into "politics" in campaign year press conferences and smothered the press with lectures on statecraft and preparations for the nation's defenses.

He was least adroit in the Supreme Court fight, in which he was so slow to claim a victory that it was put down as a defeat. He used the utmost skill in easing New Deal figures out of government without a fuss, allowing no questions on impending changes to ruffle him.

Even to those who were present at many of these press conferences the written record presents a picture of Roosevelt's comradery with the administration supporters in the gallery and of his hostility toward the press in general that is surprising. He adroitly established a sense of rapport with reporters, identifying himself with their work and burdens. His censure of the press was more severe than anything that has come out of the White House (or the government) since, but it was mostly off the record. He blamed the British press for the failure of the London Economic Conference in 1933, when the real blame was his own. He loved to philosophize about the press and fancied himself a newsman because of his experience on the *Harvard Crimson*. He said (November 7, 1934) that columnists writing once a day had to write "a lot of pure bunk." He thought "newspapers have a lot less influence than they had fifteen or twenty years ago." He accused Ernest Lindley of lying about his intervention in New York City politics in a furious and intemperate press conference on August 9, 1937. He roundly lectured the American Society of Newspaper Editors in April

1938. He said the phrase "newspaper story" was coming into general use. He complained: "There is not a newspaperman that comes into my office that understands the ramifications of the national problems." He said eighty-five per cent of the daily press had been inculcating fear in the people. On the occasion of a story about a bomb sight he asked (February 7, 1939) "is it a patriotic thing, if we have a secret of this kind, to blazon it out for the benefit of foreign governments?" In October 1942 he accused a "minority" of the press of "giving out sententious views—news stories that 'just aint so.'" On December 18, 1942, he asked Earl Godwin to give John O'Donnell an iron cross for a story he had written. On June 29, 1943, he accused reporters of coloring the news and said many of them wrote in accordance with orders from their editors. He said stories about WAAC morals were a "newspaper job" that was "shameful" and "hurt the war effort." On August 31 he said a Drew Pearson column was "detrimental not only to the foreign relations of the United States but to the unity of the United Nations, and therefore the winning of the war." He said Pearson was a "chronic liar" and "not the only one" in the press.

As the war proceeded Roosevelt threw a veil of secrecy over more and more of the government's operations, excluding the press from conferences and consultative gatherings like the food conference. The press conferences became less and less productive of real news. The president became more and more sensitive about stories implying division within the government or between the United States government and its Allies.

One is struck by the shadows of things to come. After the repeal of Prohibition he was asked about violators of the dry laws. He said: "Well they violated the law." Would there be amnesty? "The question of the reasonableness of the sentence is a different thing but they certainly violated the law. There is no getting around that." He warned in 1935 that the country was headed toward control of oil. He warned on January 23 that executive responsibility for foreign affairs was limited only by the power to make treaties and by appropriations. He talked about mass-produced houses in 1936. He talked of presidential powers to "impound" appropriated funds in February

1942 but said he was, at the same time, carrying out the obligations imposed by Congress. In 1937 he quoted Woodrow Wilson as saying "once an appropriation is made or a law is passed, the appropriation should be administered or the law executed by the executive branch." And Roosevelt added his own interpretation of this: "Once an appropriation is made, I have a mandate from the Congress to carry it out. No question there."

In 1940 Roosevelt faced the guns or butter, defense or welfare arguments in an endless press conference with National Youth Administration spokesmen. He met the complaint that he was spending a billion more for arms and a billion less for relief, the youth, and the unemployed. He had a hard time persuading his questioners.

There is hardly a subject of current political or economic interest to which reference cannot be found in these volumes. In the end, though, they tell less of history than of the man who made it in these fateful years. These pages say better than any that have been written that he was a man of great courage, great good humor, and indomitable faith.

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REXFORD G. TUGWELL. *In Search of Roosevelt*. Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 313. \$12.95.

REXFORD G. TUGWELL. *Off Course: From Truman to Nixon*. New York: Praeger Publishers. 1971. Pp. ix, 326. \$7.95.

During the early years of the New Deal, Rexford G. Tugwell was a prime target of Roosevelt's right-wing opponents. He was made out to be an ultra New Dealer, a prototype for the countless caricatures of the professor in Washington who, never having carried a precinct or met a payroll, was bent upon ruining the nation with radical nostrums. Now, forty years and many books later, Tugwell is secure in his reputation as one of those brilliant, innovative social scientists who helped move the nation toward more vigorous interaction between the government and the economy. He has not abandoned his faith in government economic planning, the "collectivism" for which he stood in the 1930s, and in both his two latest books

he vigorously defends it. Further, in his writings he has been one of the most perceptive interpreters of Roosevelt. Although he has published extensively on these themes he still has much to say that is thought-provoking. That is especially true of *In Search of Roosevelt*, a collection of the best of the essays he has written during the years since Roosevelt's death. Most of them appeared in journals not easily available; two appear in print for the first time. The most important are two reprinted from the *Western Political Quarterly*, "The Compromising Roosevelt" and "The Experimental Roosevelt," which are in part a reminiscence and in part an exploration of the fascinating problem of means and ends. Thus, Roosevelt's approach to legislation during the "hundred days": "He often felt that if many panaceas were authorized in a measure, the sponsors of each would at least not object; and it would be accepted by the Congress with a minimum of argument or delay. Once the bill was passed one or another or maybe several of embodied schemes could be tried and discarded if they failed. So far as Roosevelt was concerned, the scheme was of no importance, however much it meant to its doctrinaire authors. He was interested only in results, and grand results at that" (p. 286).

Along with his exploration of basic motives and techniques Tugwell presents Roosevelt as a flesh-and-blood figure. He not only explains the significance of the meeting of the president-elect with President Hoover over the war debts question late in 1932 but also recounts Roosevelt teasingly insisting to Raymond Moley, who was to accompany him to the White House, that Moley must wear striped trousers. Again, in a vivid account of Roosevelt's ventures in Georgia farming, Tugwell sets forth numerous homely anecdotes to arrive at a significant conclusion. Far from operating the farm as a frivolous hobby, as it appeared to be, Roosevelt was demonstrating how to rehabilitate a worn-out cotton growing area into one of tree farming and beef production.

There is less of Roosevelt's human qualities and his fallibility in *Off Course*, which is a tract on the developments of the past quarter century. Roosevelt becomes the model against which to measure the failure of subsequent presidents to move in domestic policy toward

collectivism and in foreign policy away from cold war.

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FRANCIS V. O'CONNOR, editor. *The New Deal Art Projects: An Anthology of Memoirs*. Washington: Smithsonian Institution Press; distrib. by George Braziller, New York. 1972. Pp. ix; 339. \$12.50.

The memoirs in this well-illustrated anthology were commissioned in 1968 as part of a study of the cultural and economic effectiveness of the New Deal Art Projects—a study commissioned by the National Endowment for the Arts and conducted by Francis V. O'Connor. Because he sought persons who could discipline recollections with archival research, O'Connor has produced a volume that should provoke interest among scholars as well as nostalgia among project alumni. Although the three Treasury Department programs, routinely described by Olin Dows, were nationwide in scope, as was the WPA Federal Art Project (WPA/FAP), it is the vibrant, trouble-ridden New York Art Project to which O'Connor has turned for his writers. And well he might for New York provided on a grand scale the triumphs and hazards of this federal foray into subsidized art.

The director of the New York region was Audrey McMahon. Referred to by her assistants as "arsenic and old face," this formidable woman fought for her artists with consummate skill and dedication, revealing in her essay the intermixture of art, relief, bureaucracy, and politics that so characterized all of the WPA cultural projects. The artists themselves describe in greater detail the functioning of the various creative divisions. Edward Laning, in a delightfully informative account of his development as a mural painter, provides extensive information on his Ellis Island murals as well as those in the New York Public Library. Joseph Solman furnishes similar insights into the working of the easel division, which employed such colleagues as Joseph Stella, Marsden Hartley, Milton Avery, Ad Reinhardt, Jackson Pollock, Mark Rothko, Jack Levine, Philip Evergood, Robert Gwathmey, Ben Zion, and Bryon Browne. Emphasizing the

diversity of their art, Solman effectively challenges the stereotype of federal artists as primarily producers of the hackneyed proletarian art so often associated with American scene painters in the thirties and with WPA painters in particular. The point is underscored by Rosalind Bengelsdorf Browne who describes the interrelationship between the American Abstract Artists and the WPA/FAP. When the Museum of Modern Art was still showing American Regionalists the government was also exhibiting pioneer abstractionists, sponsoring welding demonstrations by the sculptor David Smith, and encouraging Ashile Gorky in his now destroyed abstract murals for the Newark Airport. The Graphic Arts Division pioneered in its own way according to Jacob Kainen. By expanding the technical possibilities of the media, especially in the area of lithography, and by enlarging popular interest through its vast allocation of prints to public institutions, the FAP, Kainen argues, effectively bridged the gap between the moribund etching societies of the past and the virtual explosion in the graphic arts occurring in the last two decades. Less innovative but no less noteworthy was the Index of American Design. A part of the rediscovery of America in the thirties, these illustrations of historic American crafts reveal "the existence of an ingenious and highly respectable tradition of genuine spontaneous creativity early in our history." Lincoln Rothchild's claim is not an inflated one as anyone who has examined these meticulously executed plates can attest.

In sum, what O'Connor's contributors have done is to provide an informed and balanced assessment of selected aspects of the federal government's involvement in the visual arts. In the process they make no effort to minimize problems confronting this relief-born enterprise. Yet in a curious way theirs is also a story of partial triumph—triumph, in the main, of artistic freedom; of democratic practices in an elitist profession; of technical and esthetic growth in the midst of economic insecurity; of a vision of "arts for the millions"; of an unprecedented cultural democracy at a time when all democracy was threatened. On each of these themes O'Connor could have elaborated more fully in his introduction. His anthology, nonetheless, is a welcome addition

to the growing literature on the cultural history of the depression decade.

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EDWARD L. and FREDERICK H. SCHAPSMEIER.  
*Prophet in Politics: Henry A. Wallace and the War Years, 1940-1965*. Ames: Iowa State University Press. 1970. Pp. xv, 268. \$8.95.

The tragedy of New Dealer Henry A. Wallace was that while he raised crucially important foreign policy questions during the cold war and spotlighted the neglect of the nation's Negro and the poor, he allowed himself to be co-opted, at a critical period, by Communists and fellow travelers and thus appear as an apologist for Communist tyranny and aggression.

As FDR's third-term vice-president, and head of the Board of Economic Warfare, he fought vigorously for experimental development and stockpiling of scarce resources prior to and during the war years. With a unique rearing in scientific farming and recent mastery of Spanish and Russian he made a significant impact upon governmental officials and farmers during important wartime missions to Latin America, Russia, and China. His religioeconomic interpretation of events—his stress on social justice, the brotherhood of man, and the blessedness of universal peace—attracted the overwhelming support of rank-and-file Democrats nationally. Though Southern leaders and urban Democratic bosses convinced FDR to dump Wallace for the 1944 race, this prophet from Iowa came within a few votes of winning renomination. Might America have played a fundamentally different role had FDR defied his conservative advisers?

As secretary of commerce, Wallace lamented America's bipartisan foreign policy, concluding that we were on a collision course with the Soviet Union. Instead, he suggested a return to a more moderate approach toward Moscow because of his belief that the Soviet government and its leaders had disclaimed aggressive intentions or desire for world dominion.

Truman's offhand endorsement, in advance, of Wallace's controversial foreign policy ad-

dress of September 1946 not only demonstrated a lack of sophistication on the part of FDR's successor but evoked a major cabinet crisis that was finally settled when the chief executive fired him. Gradually, thereafter, Wallace veered away from the liberal, democratic mainstream. During his brief reign as editor of the *New Republic* magazine he became increasingly isolated from the real world by fawning Communists and fellow travelers. Michael Straight, the magazine's publisher, informed me in 1947 that he found it impossible to break through the human barrier surrounding Wallace for private conferences. After Wallace fulfilled a number of speaking engagements in Britain during the spring of 1947, Labourite left-winger Jennie Lee bemoaned his "presentation of world affairs in which Russia is always right and the rest of the world always wrong."

Bearing the mantle of FDR, Henry Wallace was in a position to exert tremendous influence upon millions of Americans who still recalled the New Deal with warmth and affection. Instead, as presidential candidate of the Progressive party during the 1948 race, he threw in his lot with the controlling authoritarian Left and momentarily besmirched his role in American history as an apologist for the Soviet Union.

In this intriguing and scholarly study, the second of their two-volume biography, the authors have filled many gaps and disposed of many myths. Though neglecting, for example, to distinguish clearly the role of the Liberal party in New York, which had resulted from defections from the American Labor party because of the latter's domination by Communists and fellow travelers, the authors, nevertheless, have placed Wallace in the proper perspective. He was a farsighted botanist, philosopher, and muckraking journalist who became not only an outstanding public servant but also a spokesman for discriminated minorities and urban laboring masses. However, he "was a man driven by conscience and conviction, so much so that his concern for political realities was often obscured by the cause itself."

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NORMAN D. MARKOWITZ. *The Rise and Fall of the People's Century: Henry A. Wallace and American Liberalism, 1941-1948*. New York: Free Press. 1973. Pp. xi, 369. \$8.95.

Past studies of Henry A. Wallace have usually stressed the "agrarian years." Wallace as agricultural reformer, they have concluded, was innovative and effective. But in the 1940s, as he became embroiled in urban and "popular-front" politics, his befuddled mysticism and naïveté made him a pitiful and tragic figure.

Markowitz's study attempts both to reassess Wallace and fit him into a larger drama of wartime visions and postwar tragedy. Drawing upon massive research in the Wallace papers and related collections it focuses, in particular, upon how the wartime vice-president became the leading spokesman for a "World New Deal" or "people's century" and how, step by step, the chances of implementing this vision melted away. From the beginning, Markowitz argues, Wallace and other "social liberals" failed to see the contradictions in their conception of a "progressive capitalism," and once wartime compromises and lapses had left them to work within a structure of corporate power, pressure-group politics, personality worship, and misconceptions about Russia the outcome was virtually inevitable. Freed from threats of mass unemployment and fascist aggression, a resurgent conservatism could not be contained. Delivered into the hands of a "bumbling" Harry Truman, the Roosevelt coalition disintegrated. And after 1946, as Truman reacted with a blend of anti-Communist rhetoric, populist poses, and economic imperialism, many "social liberals" lost their "nerve." Sacrificing ideals for "toughness" and invoking images of a "Red-fascism," they became "cold-war liberals," joined in crushing Wallace's courageous defenders of the earlier vision, and helped usher in Acheson's "geopolitical fantasies" and McCarthy's Red-baiting.

As a reinterpretation of the 1940s Markowitz's work is more provocative than convincing. It should stimulate considerable rethinking. But on balance its portrayal of the Truman administration comes too close to caricature, its reading of Russian-American relations rests upon too many questionable premises, and its categories of "social," "corporate," "interest-group," "popular-front," and

"cold-war" liberals often distort as much as they clarify. As a careful reconstruction of Wallace and his activities, however, the book is more successful. Its accounts of the clash with Jesse Jones, the vice-presidential nomination of 1944, and the Madison Square Garden speech of 1946 are the best in print. And while one may question the "realism" of Wallace's dream, even in a "democratic socialist America," the study offers convincing evidence that the dreamer was neither the "visionary crackpot" or "Communist dupe" of orthodox history nor the variant of "Open Door imperialist" seen by some New Leftists. In future accounts of the 1940s it seems likely that he will be depicted as more sophisticated, more courageous, and less foolish than he has been in the past.

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*Foreign Relations of the United States. The Conferences at Washington and Quebec, 1943; The Conference at Quebec, 1944.* (Department of State Publications 8552 and 8627.) Washington: Government Printing Office. 1970; 1972. Pp. xcv, 1382; 1, 527. \$7.00; \$4.75.

*Foreign Relations of the United States* defies reviewing. Given the amount of time one can reasonably spend on such a job it is simply impossible to do the two things that should be done with any documentary collection—that is, fully evaluate the fullness and fairness of the selections and point out what is new and useful information for scholars. Both of those tasks are tantamount to preparing a fully researched monograph on each major subject dealt with in each volume under review, and in this case that would amount to at least eight books. This review is, then, like almost every other of this series I have ever read—vague, impressionistic, and all too uncritical.

The materials in the volume titled *The Conferences at Washington and Quebec, 1943* have been available to scholars since 1970 and have already begun to appear in articles and monographs. The two conferences, probably better known by their code names of TRI-DENT (Washington, May 12-25, 1943) and QUADRANT (Quebec, August 11-24, 1943), mark the point at which postwar political

and economic considerations first began to muddy the waters for the Americans. To be sure, as the large number of military documents indicates and as the editors point out in the introduction, military planning dominated the conferences, particularly the one at Washington in May, although the troublesome issue of Soviet-Polish relations did crop up at the TRIDENT meeting. Three months later, at Quebec, there seemed to be a far deeper concern regarding the structuring of the postwar world. The terms of the Italian surrender, British access to atomic energy with its potential for postwar commercial use, German and Italian territorial questions, and the problem of a postwar international organization all appeared in either the actual discussions or the preconference briefing papers. In the military talks familiar themes appear. As outlined in his memoirs Churchill began at Quebec a fervent campaign to commit the United States to a military effort in the Dodecanese Islands—a campaign that would have eventually forced a re-evaluation of the date for the Normandy invasion set a few months earlier at the TRIDENT conference. As usual, the demands of the European Front prevented any meaningful shift of resources to the Asian and Pacific theaters, in spite of a series of vague and eventually unfulfilled promises.

The volume titled *The Conference at Quebec, 1944*, though substantially shorter than the one just discussed, is somehow more intriguing. The reason is, I suppose, that politics and postwar planning dominated the talks. It was at the Quebec meeting that Treasury Secretary Morgenthau presented his plan for the partitioning and pastoralization of Germany. Clearly recognizing the relationship between reparations, reconstruction, and any re-ordering of the main thrust of the German economy Morgenthau argued that the reconstruction of Germany was both dangerous and politically unpopular in the United States (an appeal well calculated to sway Roosevelt), hence large reparations had to be eliminated. The details of the argument and the supporting documentation are set forth in full, and it appears that a re-evaluation of the Morgenthau plan may well be called for. Other concerns also characterize the OCTAGON

conference. Most striking was the clear and very deep concern over the development of tension between the Western Allies and the Soviet Union. The struggle for power in Europe clearly stimulated such suggestions as an occupation of Norway as well as Poland, and the various briefing papers are replete with indications of the awareness on the part of political advisers that a crisis seemed in the offing.

Both volumes are examples of the best the *Foreign Relations* series can offer. In each case the Historical Office has assiduously searched out relevant materials in other collections, particularly in the Franklin D. Roosevelt Library, and has also provided informative references to various printed memoirs that add additional information. The footnotes invariably answer the identification and technical questions, while the maps and photographs in each volume add a touch of life.

In a sense, however, such extensive research and work outside the Department of State records only demonstrates the shallowness of the conception behind the entire series. Put in its simplest terms, foreign policy is not and never has been the exclusive province of the secretary of state and his department. In order to assemble a reasonable sampling of the documentation behind modern American foreign policy the editors are forced to examine a wide range of non-State Department materials. The role of the military, obviously critical in America since 1941, requires at least the papers of the Joint Chiefs of Staff. Since that organization's attitude toward declassification is far more conservative than that of the State Department (which itself is hardly liberal except by comparison to other governments) one can be assured that what appears is carefully screened; nor can independent scholars examine those papers in order to evaluate the work of the editors of *Foreign Relations*. Other governmental organizations—the Commerce Department, the Treasury Department, the Departments of War, Navy, and later Defense—all play a critical role in various aspects of foreign policy, yet precious few of their documents appear within these or subsequent volumes of *Foreign Relations*. That the staff of the Historical Office cannot cope with such a mass of material is obviously true—but hardly an argument for the completeness of

the series. Even if the editors had the personnel, some invaluable collections are simply not available. The Stettinius papers were closed to those editing the volume on the Quebec Conference of 1944, yet Stettinius was a prime mover in the State Department as of the fall of that year. Later volumes have suffered because of the unavailability of the Truman papers, and we can expect similar problems in the future with other presidential papers. Can you imagine a volume on American relations with China for the Nixon years without full, complete, and open access to the files of the Kissinger office in the White House? The need for full access to presidential records is driven home by an examination of the volumes under review. In each there is a minimum of documentary evidence on the actual proceedings of the conferences between Churchill and Roosevelt. Without the vast number of briefing papers, later memorandums, and other materials gathered by the editors (particularly those in the Franklin Roosevelt Library) we would have almost no real knowledge of what actually transpired. Perhaps the most telling argument along this line is the fact that no monograph is ever considered as adequately researched if it relies solely on *Foreign Relations of the United States* for its official documentation of foreign policy matters. Scholars are always and rightfully expected to examine all the records themselves. If one follows that idea to its logical conclusion, then the series is of use primarily for the training of students, to provide background in nondiplomatic history studies, and as a guide to further research. Does that really justify the time, expense, and effort? This is not the place for a re-evaluation or proposed reshaping of the series, but I am reminded of Louis Morton's call for the establishment of a Historical Office outside and above the Department of State. Certainly one thing seems clear. Our current all-too-restricted view of what constitutes the history of American foreign policy and diplomacy has been, at least in part, caused by the profession's overexuberant praise for the *Foreign Relations* series as a primary source.

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FORREST C. POGUE. *George C. Marshall: Organizer of Victory, 1943-1945*. Foreword by OMAR N. BRADLEY. New York: Viking Press. 1973. Pp. xviii, 683. \$15.00.

In this third volume of his magisterial biography Forrest Pogue reaches his stride as a biographer of the first rank and General Marshall emerges as one of the dominant figures of the Second World War, the worthy antagonist of Churchill and the main support of President Roosevelt—in a real sense “the organizer of victory.” This is biography at its best. With the two preceding volumes and the prospect of two more still to come we have not only the most definitive and authentic but also one of the most comprehensive portraits of any leader of the wartime period, a work comparable in breadth and meticulous attention to detail to Douglas Freeman's biography of Robert E. Lee.

Marshall's public life was plainly exposed to view, and though he repeatedly refused to write his memoirs he did talk at length to Mr. Pogue. But Marshall, always “reticent about his personal life,” submitted reluctantly to Pogue's “probings of his private thoughts and emotions” (p. xiii). As a result Pogue had to depend on Marshall's associates and friends for whatever anecdotes and stories he could pick up to portray the human side of his subject. Despite his heroic efforts, however, there is little in the book about the general's life and thoughts and virtually nothing about his home life or family relations.

On the other hand, Pogue has little difficulty portraying Marshall the soldier and public figure. Drawing on Marshall's correspondence, on a wide variety of contemporary accounts, and on more than three hundred interviews with Marshall's wartime colleagues Pogue documents in full detail the general's career and the qualities that earned him the respect and admiration of all with whom he came into contact. *Time*, which named Marshall “Man of the Year” in 1944, said he was “the closest thing to the indispensable man,” and under Pogue's masterly hand Marshall more than lives up to this characterization. Stimson thought him the finest soldier he had ever known, and Robert Lovett called him a magnificent human being. His integrity and self-denial were remarkable. He denied all pub-



licity for himself unless it contributed to the war effort and refused to use his influence for personal gain or friendship. He had an unerring judgment of men, and his ability to select the best man for the job, wrote General Bradley, was almost uncanny. Fiercely independent, invariably truthful, and remarkably free of ambition he exercised enormous influence with Congress and the American people. His grasp of complex subjects, his mastery of detail, the clarity and eloquence of his presentation, and his commanding presence were enormously impressive. Invariably he was compared to George Washington. "His integrity," wrote Douglas Freeman, quoting Jefferson, "was most pure, his justice the most inflexible I have ever known, no motives of interest or consanguinity of friendship or hatred being able to bias his decisions" (p. 358).

Despite Pogue's disclaimer, this volume is more than biography; it is a first-rate history of the two climactic years of the war viewed from the perspective of one who participated in all the major Allied decisions and who, possibly more than any other, shaped the course of the conflict. It is as broad and complete a view of the Allied war effort as one could hope for, since Marshall's responsibilities encompassed virtually every aspect of the war: strategy, manpower, procurement, production, organization, and even morale. The forces that he had built, the weapons he had assembled, and the plans he had made since 1939 reached fruition during these two years.

Appropriately, the volume opens with the Casablanca Conference in January 1943 as the initiative was slipping away from the Axis. The decisions made then set the stage for the measures that led ultimately to victory in Europe—the combined bomber offensive, the drive up the Italian boot, the cross-channel attack, and the landings in southern France. As Pogue recounts the story of these years, they were not years of unbroken triumphs and resounding success for Marshall. There were setbacks on the field of battle; differences over strategy and command with the British, with the Navy, and with MacArthur; personal tragedy in the loss of a stepson for whom he felt a father's love; and professional disappointments. Of these, the greatest was the failure to secure the coveted command of the Allied

invasions of Europe, the culmination of all his plans and hopes. Pogue tells the story of this decision, the greatest drama of Marshall's life, more completely and in more detail than it has been told before.

Inevitably, this volume will be read in the light of the charges of treason made during the McCarthy era concerning the agreements reached at the Yalta Conference and the decision to leave Berlin and Prague to the Russians. Pogue deals with both charges and finds no merit in them—at least so far as Marshall is concerned. The Army Chief of Staff, as well as the other U.S. chiefs, he says, played a comparatively minor role in the agreements reached with Stalin, restricting themselves to the judgment that in the event it proved necessary to invade Japan it would be desirable to have Soviet assistance. Marshall understood politics very well indeed, but he was extremely careful, Pogue points out, always to confine his advice to the president to military matters. Speaking of the military, he once wrote, "We have a great asset and that is that our people, our countrymen, do not distrust us and do not fear us. . . . We are completely devoted, we are a member of a priesthood really, the sole purpose of which is to defend the republic" (p. 458).

In a sense this volume is incomplete. It ends with the defeat of Germany in May 1945, omitting the story of the closing months of the war. Pogue's decision to omit the story of Japan's defeat was made on the ground that these events were needed as background for Marshall's mission to China, to be covered in a later volume. There is room for disagreement here, but there is no doubt that the result of this decision is to weaken the present volume, probably the keystone volume in the series, and to diminish its value to the student of World War II.

Despite this reservation the third volume of the Marshall biography stands out as one of the best works yet written about the war. It is global in scope, deeply researched, thorough, written with clarity and forcefulness, and amply supported with photographs, maps, a chronology, a bibliography, and detailed notes. And not least among its virtues is the perspective it provides for those whose view of the military has been shaped by the war in Vietnam. No

reader of this volume can fail to be struck by the contrast between the attitude toward the American soldier in Vietnam and Marshall's fierce pride in the GI of World War II, or the "body count" of Vietnam and Marshall's strong feelings about casualties. His deep concern for the welfare of the ordinary soldier, his careful selection and watchful supervision of commanders on whom depended the lives of the troops, and his enormous prestige are timely reminders that in a different time and in a different war the army was the agent of the popular will and its leaders the heroes of a generation of veterans.

LOUIS MORTON  
Dartmouth College

MARGARET TRUMAN. *Harry S. Truman*. New York: William Morrow and Company. 1973. Pp. 602. \$10.95.

SUSAN M. HARTMANN. *Truman and the 80th Congress*. Columbia: University of Missouri Press. 1971. Pp. viii, 241. \$9.00.

Two more books on what Mrs. Daniel calls the "Truman Years." One can only feel morose that historians and relatives alike seem agreed on the term. Even I. F. Stone succumbed to the temptation. Yet after reading these two books one is bound to conjure with the possibility that the significance of the Truman presidency lies essentially in the fact that the fate of the United States and of the world was for a while in the hands of a man of limited sophistication, a man whose mental frontiers seemed bounded by the operations of Jim Pendergast on the one hand and General Douglas MacArthur on the other. Between the two he could and did make a choice. We may rejoice that Truman's choice was what it was—because maintaining purity in domestic American politics was less important than refusing to challenge the Yellow Peril beyond the Yalu River. But to imply that he somehow dominated these years is to give history a hyperbolic distortion. Let us be content to say that we were lucky; it could have been worse, except for the inhabitants of Hiroshima, Nagasaki, and North and South Korea.

Margaret Truman's book is a daughter's *apologia pro vita mei patris*. It begins, understandably, with the spectacular electoral tri-

umph of November 1948 and then starts over again to recount "Dad's" political career from Missouri courthouse politics and highway building to the vice-presidency and after. Despite her unvarnished defense of her father's cold-war policies and her very sparse and lenient treatment of his internal security and civil rights programs, there is much to be learned from Mrs. Daniel's book. Not least are the insights into one of the most important aspects of twentieth-century American social-political relationships. That is the tension between the "Eastern establishment" and the political professionals of the Midwest and Southwest. To read the Trumans' assessment of the Roosevelts is like reading the Johnsons' assessment of the Kennedys—and no doubt the Nixons' views would concur in both cases. Middle America, while not a geographical expression, seems to have a geographical weighting. Mrs. Daniel's comments are precise: "Wooing his enemies with gifts was one of Mr. Roosevelt's favorite tactics. My father never believed it was good politics and frequently said so." In 1940 some Democrats believed that a third term was another Roosevelt innovation that was splitting the party and his daughter reports: "My father, with his respect for American political tradition, felt the same way." The resentment deepened when FDR tried to buy Truman away from running for re-election in 1940 by offering him a life appointment to the Interstate Commerce Commission. References to "Roosevelt yes-men," to FDR as "ever the astute politician," to Harold Ickes as a "Roosevelt prima donna," to "Mr. Roosevelt's inability to pass on responsibility," and to the Roosevelt "Palace Guard" seem to add up to repayment for the gleeful charges that Truman was the "Ambassador in Washington for the defunct principality of Pendergastia."

Beyond these unguarded intimations there are some truly hair-raising points of emphasis. Truman's conviction that he was "very close to war with Russia" in 1948 is further documented. But even more important is his daughter's stress on his interpretation of NSC-68—the National Security Council policy review of 1949. "Although few of his biographers have noticed it," writes Mrs. Daniel, "he specifically rejected the policy of containment. 'Our purpose was much broader,' he said, 'We were

working for a united, free and prosperous world.'"

Professor Hartmann's study of Truman's relations with the Eightieth Congress is a superbly researched book, tightly packed with voting records and yet firmly interpretive. Starting with the unhappy congressional electoral results of 1946 she sets the stage with yet another statement of the social-political tension so often implied by Mrs. Daniel. In this case it is the specific disaffection of the "highly articulate group of middle-class reformers" who had worked for FDR. Seeking in Truman "a new source of unity and inspiration" they found him spectacularly wanting—"the small-town, midwestern, machine politician with the country accent and adventurous grammar could hardly fill the shoes of a sophisticated, charismatic, and seemingly independent statesman." Inept presidential leadership during the Seventy-ninth Congress, the failure of reform legislation, "the widening breach with the Soviet Union and U.S. support for reaction and imperialism abroad" all led to dark despair.

Professor Hartmann traces with exhaustive precision Truman's period of "appeasing" the Eightieth Congress, his success in getting a consensus on foreign policy, and the means by which (with the invaluable aid of Clark Clifford) he forced the Congress to build its famous "record." Professor Hartmann is harsh in her judgment: "His primary objective was not legislative results, but the building of a record against the Eightieth Congress for the 1948 campaign. . . . In bombarding Congress with requests that were certain to be denied, Truman cast himself as the Galahad of reform pluckily fighting the dragon of privilege." At the same time the president scored off the liberals by condemning Wallace's "insidious propaganda" for peace, declaring "I will not accept the political support of Henry Wallace and his Communists." In March 1948 he ignored General Marshall's plea to avoid inflammatory language and publicly charged the Soviet Union with sole responsibility for obstructing world peace.

Despite the sharpness of her scholarly scalpel Professor Hartmann concludes her important book with an essentially moderate assessment: "Given the conservative nature of U.S. politics, which the President himself symbolized,

the general tendency of domestic reform to support the long term needs of corporate capitalism, and the limitations on presidential power, and taking into account the long gestation period of many Populist-Progressive goals, Harry Truman's contributions to domestic reform were substantial."

KENNETH MCNAUGHT  
University of Toronto

MARIAN J. MORTON. *The Terrors of Ideological Politics: Liberal Historians in a Conservative Mood*. Cleveland: Press of Case Western Reserve University. 1972. Pp. xi, 192. \$5.95.

In this doctoral dissertation presented at Case Western Reserve University, Marian Morton carries onto new ground the furious attack New Left scholars have been making against the so-called "consensus" historians of the post-World War II era. Instead of disputing their conclusions, she has concentrated intensively and exclusively on their underlying premises. Her subjects are Arthur M. Schlesinger, jr., Louis Hartz, Daniel Boorstin, Edmund Morgan, and Richard Hofstadter. These men, she argues, failed in their own early intention of giving a more effective and coherent form to American liberalism; they failed because they lost the faith in reason that distinguished their progressive forebears. Her study is both a manifesto for the more rationalistic strain in the New Left and an exploration of the intellectual history of the last three decades.

The most fresh and interesting aspect of the book is its demonstration that the five historians under scrutiny tended to move in the same direction under the impact of McCarthyism. Already deeply influenced by a realistic, nonrational view of human nature derived from modern social science, the liberal historians were trying to find a balance between realism and idealism. McCarthy's assault on intellectuals upset the balance by bringing home the destructive effects of ideologies. The historians then embraced a stultifying realism that has downgraded the intellectual and depreciated the power of idealists to make a better world. Although Morton calls me an apologist for liberal history, I think her criticism is in general outline correct, and I hope the next generation of scholars will do more justice to the place of reason in history

than has the last. But nothing will be gained and much lost if the danger of ideological passions is forgotten and we fall to cutting up one another with logic-chopping meat axes. Morton's analysis—shrewd but ruthless—often squeezes her subjects to fit a conceptual scheme and to force confessions from their lips. Thus Hartz becomes a spokesman for capitalism, and Morgan a proponent of worldly self-interest, while Hofstadter endorses the mental technician as the preferred type of intellectual. These painful distortions culminate in the last chapter in the charge that the liberal historians "have committed the intellectual sin of writing a history which they cannot have believed in." One closes the book with a renewed sense of the arrogance and insensitivity an unbridled rationalism risks.

JOHN HIGHAM

*Johns Hopkins University*

*Public Papers of the Presidents of the United States. Richard Nixon. Containing the Public Messages, Speeches, and Statements of the President.* [Volume 1.] 1969; [volume 2.] 1970; [volume 3.] 1971. (Office of the Federal Register, National Archives and Records Service, General Services Administration.) Washington: Government Printing Office. 1971; 1971; 1972. Pp. lii, 1183; liii, 1305; xlix, 1362. \$14.50; \$15.75; \$15.75.

With the publication of the first three volumes of the *Public Papers of Richard Nixon* for the years 1969, 1970, and 1971, respectively, scholars now have at their disposal a well-organized guide for the Nixon presidency that covers everything from Vietnam and Sino-Russian-American diplomacy to the problems of the economy, social welfare, and the White House's relations with Congress and minority groups.

The first volume (1969) opens with President Nixon's inaugural address in which he called for a lowering of voices at home and the need to begin an "era of negotiation" abroad. But the major problems that beset his administration during that first year in office, namely, Vietnam and inflation, made it difficult for him to achieve either goal. On the matter of Vietnam Nixon moved adroitly to neutralize the volatile domestic issue of the war. In order to avoid either a unilateral withdrawal or a settlement on terms that would have alienated

his constituency and damaged the national interest as he defined it, Nixon hatched the plan of "Vietnamization," which he first articulated during his June 1969 Midway Island meeting with President Thieu of South Vietnam (pp. 443-50). On September 16, 1969, the president publicly announced that troop levels in South Vietnam were going to be reduced (p. 718); and on December 15, 1969, he informed the American people that another 50,000 troops would be brought home by April 1970 (p. 1027).

A key to Nixon's Vietnam policy was revealed in his Guam doctrine: on July 25, 1969, he declared that the United States had no intention of playing a passive role in the Pacific basin or in Asia, but in response to the new forces and needs of Asian nationalism it would accept the principle of "Asia for the Asians. And that is what we want, and that is the role we should play. We should assist, but we should not dictate" (p. 548).

On the domestic front Nixon, in 1969, did not ignore the specter of inflation, but he made it clear, time and again, that the imposition of price and wage controls was no solution to the problem. A better tack, he argued, was to reduce federal spending, retain the surtax, and tighten credit controls—all of which, he felt, would restore the economy to a state of noninflationary balance and growth, even though such short-term policies would produce higher unemployment (pp. 808-14). In the area of social services Nixon retained the Office of Economic Opportunity. But more important was his strong endorsement of a comprehensive family assistance program (FAP) to replace the archaic welfare system with something akin to a guaranteed annual income for the working poor and those who were simply unemployable for reasons of health or age or who lacked legitimate job opportunities (pp. 637-45). This program, which Nixon shrewdly tied to a revenue sharing scheme for the states and municipalities, was a landmark proposal, deserving careful consideration from liberals and conservatives alike.

As the 1970 volume reveals, President Nixon's second year in office was far more conflict-ridden than his first, especially in his relations with Congress. Although Congress generally supported his Vietnam policy there

were rifts between the White House and Congress on matters affecting the appropriation of funds for such social welfare programs as OEO, hospital construction and modernization, and education. Nixon vetoed these congressionally sanctioned bills, charging that they made it difficult for him to bring an inflationary economy under control (see pp. 21-26, 513-15, 663-65).

In another area, the president moved to enlarge his political base in the South, a move dictated by his need to head-off or weaken a possible Wallace campaign in the heart of Dixie preparatory to the 1972 campaign. Therefore, Nixon began to speak out in defense of the neighborhood school system and to oppose busing for the purpose of achieving racial balance (p. 315). He also nominated a Southerner for a Supreme Court seat, Judge G. Harrold Carswell of Florida, who, like an earlier Nixon nominee, Judge Clement F. Haynsworth of South Carolina, failed to obtain Senate confirmation. Piqued by this defeat Nixon castigated the Senate by charging that both Carswell and Haynsworth had been victims of "regional discrimination" (p. 346).

Perhaps the people of Cambodia were the more authentic victims of "regional discrimination" following Nixon's order to American troops to liberate territory located inside Cambodia. Escalating his rhetoric to match this precipitate move, Nixon solemnly declared on April 30, 1970, that "if, when the chips are down, the world's most powerful nation, the United States of America, acts like a pitiful helpless giant, the forces of totalitarianism and anarchy will threaten free nations and free institutions throughout the world" (p. 409).

The campus protest against Nixon's Cambodian incursion was enormous, though it led to the tragedy at Kent State University and, later, the killings at Jackson State College. Responding to the profound opposition to his Cambodian policy from college students across America the president created a special commission to examine the causes and origins of student unrest and rebellion and to recommend ways of better protecting academic freedom (pp. 498-99). That commission, headed by former Governor William Scranton of Pennsylvania, submitted its report, which was somewhat critical of his war leadership, to

him on September 26, 1970. Waiting until December 12, 1970, to answer Governor Scranton, Nixon absolved his administration of any responsibility for the campus turmoil of the previous spring. His full statement can be found on pages 1115-21 of the 1970 volume.

Another interesting feature of the 1970 volume, aside from the lengthy State of the World Message to Congress (pp. 116-90), is the complete coverage of President Nixon's active involvement in the 1970 congressional elections. Nixon, once described by Lyndon Johnson as a "chronic campaigner," was off and running from mid-October until election day in early November, seeking support from "the silent majority" for both his law and order program and votes for his party's congressional candidates other than Senator Charles Goodell of New York. But despite his hard work and tireless campaigning Nixon made little progress in changing the composition of Congress. Fortunately for the Democrats, economic worries, inflation, and the fear of unemployment blunted Nixon's efforts to establish a more solid Republican base in Congress, a fact that the president would not forget during 1971.

The 1971 volume is full of continuities, as many of the issues, problems, and programs as well as the rhetoric are carried over from 1970. For instance, in 1971 the president continued his strenuous rhetorical and legislative campaign in support of environmentalism; he once more stressed the need for a volunteer army and again endorsed a basic reform in the Selective Service Act (pp. 75-78). And ever mindful of the racial fears of millions of white Americans he repeated his defense of the neighborhood school system and opposition to busing to achieve racial balance (pp. 848-49). In addition, he made it clear that the federal government would not "impose federally assisted housing on any community" (p. 731). Nixon also continued to support revenue sharing, suggesting that here was the means of "opening the way to a new American revolution—a peaceful revolution in which power was turned back to the people" (p. 54); and the family assistance program was endorsed on all appropriate occasions.

"Vietnamization" was another theme the president orchestrated in 1971 at every opportunity. Pointing out that by the end of 1971,

400,000 American troops would have been withdrawn from South Vietnam, he suggested that the other 150,000 troops still remaining would come home as soon as military and diplomatic circumstances permitted (p. 1101).

What appears to be expressly new in 1971 was Nixon's public commitment to an expansionary federal budget to help bring about a full employment economy (p. 52) and a program for a major reorganization of the federal executive "to focus and concentrate the responsibility for getting problems solved" (pp. 56, 86). Of far greater importance was Nixon's skillfully executed policy of opening the door, once again, to China, culminating with the announcement that he would go to China in early 1972 (pp. 819, 1143). News of a breakthrough in the Strategic Arms Limitations Talks (p. 648) led, in time, to another dramatic revelation, namely, that the president would visit Moscow in May 1972, presumably for the purpose of helping to put the frosting on the cake being baked at Helsinki (p. 1030).

A remarkable shift in the administration's economic policy, having profound domestic and international consequences, was communicated by President Nixon to the American people on August 15, 1971 (pp. 886-90). Among the points made by Nixon were: the need to impose temporary price and wage controls, the establishment of a ten per cent import tax, the abandonment of the seven per cent excise tax on domestic built automobiles, and a proposal to modify and restructure the international monetary system to promote "stability and equal treatment." That speech, which should be read in conjunction with a talk Nixon gave earlier in Kansas City on July 6, 1971 (pp. 802-13), clearly indicated that the president was taking cognizance of the fact that world politics was in a far more fluid state than at any time since 1945 and that he was trying to adjust American power to that change in such a way as to keep his political and economic losses at a minimum.

A final comment is in order: all three volumes are superbly edited, handsomely printed, and are accompanied by well-organized appendixes that provide additional information, trivial or otherwise.

WILLIAM C. BERMAN  
University of Toronto

## LATIN AMERICA

CYRIL HAMSHERE. *The British in the Caribbean*. Cambridge, Mass.: Harvard University Press. 1972. Pp. xii, 240. \$12.95.

In its British edition this book forms part of a series dealing with the social history of the British overseas. Its title gives one reason to hope that Mr. Hamshere will continue the work so solidly initiated by Carl and Roberta Bridenbaugh's recent *No Peace Beyond the Line*. Instead, one is given another short history of the British West Indies—hardly more than an outline, as the author himself admits—that is arbitrary in the selection of material, fragmentary in treatment, distorted in structure and emphasis, and markedly inferior to the volume with which it must beg immediate comparison, J. H. Parry and P. M. Sherlock's *Short History of the West Indies*. Happily the latter is still available and at a much lower price.

Mr. Hamshere is most at home when he deals with pre-emancipation society. Nine of his thirteen chapters are devoted to this period, but the reader will find nothing in them that has not been related several times before. Nor will he find a single reference to the many problems in West Indian history around which considerable scholarly heat continues to be generated: the degree of profitability of the plantation economy, the ambiguities of slave insurgency and docility, and the role of religion in West Indian society, to name but a few. Although the book is innocent of footnotes, Mr. Hamshere tells us in a bibliographic note that he has leaned heavily on A. P. Newton, V. T. Harlow, and J. A. Williamson for his account of the sixteenth and seventeenth centuries and on the journals of Lady Nugent, Monk Lewis, and Thomas St. Clair for his picture of West Indian society as it approached emancipation. After that, his sources begin to fail him. He would appear, for instance, to be unfamiliar with such standard works as those of W. L. Burn on apprenticeship, R. W. Beachey on the sugar industry in the nineteenth century, W. Sewell on the condition of postemancipation labor, and Gordon K. Lewis on the modern West Indies. Small wonder that the twentieth century is dealt with in nineteen pages, while the

critical disturbances of the late 1930s receive two hasty sentences.

The numerous illustrations, in contrast to the text, are an unqualified delight.

ALAN H. ADAMSON

*Sir George Williams University*

DAURIL ALDEN, editor. *Colonial Roots of Modern Brazil: Papers of the Newberry Library Conference*. Berkeley and Los Angeles: University of California Press. 1973. Pp. xiii, 294. \$12.50.

In the opening essay, Professor Charles R. Boxer applauds the many contributions to the historiography of colonial Brazil that have appeared since 1950, when he last surveyed the field. Thus he sets the stage for the articles that follow, all of which were delivered at the Newberry Library conference in November 1969. Fresh from their dissertation labors, these young talents have presented original research pieces, rather than contributions to a predetermined theme. And the criticisms of the discussants at the Chicago meetings have been incorporated into the present version, carefully edited by Professor Dauril Alden of the University of Washington.

If there is a dominant theme in the various essays, it revolves around the ability of the Portuguese government to implement its objectives overseas. In the first essay, "Centralization vs. Donatarial Privilege: Pernambuco, 1620-1630," Professor Francis A. Dutra (University of California, Santa Barbara) explains in detail how the proprietor of that province was able to maintain his prerogatives, despite the centralizing trend elsewhere in Brazil. This was made difficult after 1602 as certain governors-general, out of self-interest, spent much of their time in the Pernambucan capital rather than in Bahia. But the appointment of the *Donatario's* brother to the governorship of Pernambuco in 1620 and subsequently to the post of governor-general of Brazil assured the victory for donatarial privilege—an outcome that also benefited the Crown. Professor David M. Davidson (Cornell University) in the second article, "How the Brazilian West Was Won: Freelance and State on the Mato Grosso Frontier, 1737-1752," argues convincingly that it was primarily state power that secured Portugal's victory over Spain in the western frontier

and not just the vaunted frontiersmen and miners of Brazil. In the geopolitical exposition that follows, the author illustrates a common characteristic of this volume: the expertise of these young historians with the methodology of the social sciences.

The third essay, "The Generation of the 1790's and the Idea of Luso-Brazilian Empire," by Professor Kenneth R. Maxwell (University of Kansas), reveals how certain Portuguese leaders encouraged the Brazilian elite to support plans for an empire in Brazil even before it became a reality in 1808. The slave revolt of 1792 in French America and the Bahian events of 1798 had a sobering effect upon Brazilian whites, some of whom had previously leaned toward republicanism. Moreover, the sugar boom of the 1790s and the favorable relationship with the Portuguese market promoted a mood of accommodation among Brazilians. Capitalizing upon this sentiment, the leadership in Lisbon offered the 1803 project to establish the empire's capital in Brazil. Portuguese nationalists, of course, frustrated the effort; but European events of 1807 favored the reformers, thus facilitating the transfer of the Portuguese throne to Brazil.

Another essay, "The Indian Labor Structure in the Portuguese Amazon, 1700-1800," by Professor Colin M. MacLachlan (California State University, Long Beach) underscores the government's powerful role in developing a labor system for the tropical forest areas of the Amazon. The state had a vigorous lead even during the mission period (1680-1757), further reinforcing its control under the directorates (1757-98). As it turned out, only wealthy entrepreneurs had sufficient labor. Therefore, it was decided at the end of the century to allow the free market to determine the distribution of most of the labor force. The conclusions of this excellent article invite comparison with the Spaniard's methods of labor control.

The two remaining essays, "Free Labor in a Slave Economy: the *Lavradores de Cana* of Colonial Bahia," by Professor B. Schwartz (University of Minnesota), and "A Preliminary Inquiry into Money, Prices, and Wages in Rio de Janeiro, 1763-1823," by Professor Harold B. Johnson, Jr. (University of Virginia), are both exploratory in nature. Armed with

impressive charts and statistics, they leave no doubt as to the value of notarial documents, plantation account books, price lists, and other materials in the reconstruction of social and economic patterns. Schwartz's presentation challenges the standard masters-and-slaves characterization of colonial Bahia, showing that there was much more variety in the economic system. Of the free elements, he chose to describe the role of the cane growers, their aspirations, the terms of their rentals, their treatment of slaves, and so forth. And Professor Johnson makes his point well: Latin Americanists can no longer ignore price history.

This is a model work that Brazilianists can point to with pride.

MARIO RODRIGUEZ

*University of Southern California*

ROBERT BRENT TOPLIN. *The Abolition of Slavery in Brazil*. (Studies in American Negro Life.) New York: Atheneum. 1972. Pp. xvii, 299. \$10.00.

This study, a welcome scholarly contribution to the expanding body of historical literature on the American slave systems, methodically examines the processes of change through which Brazil moved during the final two decades of slavery. Toplin reviews the familiar external circumstances that indicated the growing universal opposition to the slave trade and slavery: British political and diplomatic pressure; the Civil War in the United States; the discernible movement by Spain to abolish slavery in the Spanish Antilles; and the new intellectual climate and social consciousness that viewed the institution as anachronistic, uneconomical, and highly undesirable. The movement toward gradual abolition within Brazil began with the passage of the Rio Branco law of 1871, but until the late 1880s the movement was largely confined to the upper classes. Toplin shows that the movement was centered in the northeast, traditionally the cradle of Brazilian slavery, and that the Catholic Church played a rather insignificant role in the process of abolition. He emphasizes some of the subtle (and not so subtle) regional distinctions that coalesced to foster or hinder the movement toward emancipation. The abolitionist attack was broad-based, accentuating morality and

justice as well as the deleterious effect of slavery for all Brazilians. To the usual humanitarian appeal they then added the unwarranted power of the slaveholding classes, which "paralyzed the national economy . . . kept Brazil dependent upon single crops and hindered the development of cities and industry [while] the land monopoly of the slave proprietors left the majority of the people landless and poor" (p. 121). By the 1880s, Brazilian slaveholders, on the other hand, could no longer defend slavery as a positive, socially beneficial institution. They stressed nationalism and the rights of property—transparent arguments that became increasingly hollow when the abolitionists began to recruit something representing a popular following and to resort to physical means to free the slaves. Toplin concludes that abolition resulted from the general desire of the government and the slavocrats to avoid nationwide anarchy, not from the ordered, reasoned, deliberative process abolition is so often pictured as being.

*The Abolition of Slavery in Brazil*, while lacking the rich detail and felicitous prose of Robert Conrad's *The Destruction of Brazilian Slavery, 1850-1888* (1972), substantially demolishes the arguments supporting fundamental distinctions within the American slave systems and lends support to the long-standing doubts of writers such as Stanley Stein, Eugene Genovese, Richard Graham, and Emilia Viotti da Costa. The conditions in Brazil do not seem at all atypical for the general process of disintegration of slave systems in the Western Hemisphere, especially those in the Caribbean and Latin America.

FRANKLIN W. KNIGHT

*Johns Hopkins University*

HÉLIO SILVA, with the collaboration of MARIA CECÍLIA RIBAS CARNEIRO. 1942: *Guerra no Continente*. (O Ciclo de Vargas, volume 12. Documentos da história contemporânea, volume 11-K.) Rio de Janeiro: Civilização Brasileira. 1972. Pp. 448.

Brazil's proximity to Africa made her a key factor in World War II, and her leaders in 1941 and 1942 found themselves playing roles that had international significance. With much to be negotiated between Brazil and the United



States, strains developed. Army Chief of Staff Góis Monteiro, sometimes excited, annoyed earnest United States military officers who wanted to place American troops in Brazil's northeast. Osvaldo Aranha, the indefatigable foreign minister, argued brilliantly for anti-Axis pan-American unity but denounced what he called "impertinent daily letters from the United States Embassy about economic and military matters." Even Getúlio Vargas, Brazil's calm president, is described as becoming impatient during Finance Minister Souza Costa's mission to Washington to get long-promised arms. "We want to know whether or not it is worthwhile being a friend of the United States," Vargas cabled Souza Costa.

Subjects are dealt with in separate chapters that overlap chronologically. They include: Axis airlines in Brazil, Argentina's pro-Axis position, U.S. relations with the Vichy government, and the possible stationing of Brazilian

troops on the Azores (opposed by Salazar).

A strong point of this helpful book is the inclusion of much material from the Vargas files and from the archives of the Brazilian Foreign Ministry. Furthermore, archives at Stuttgart, Germany, and at the Brazilian Maritime Tribunal are the source of interesting, shocking details given in thirty-seven pages about the sinking of almost thirty Brazilian vessels. The discussion of efforts to settle the Peru-Ecuador boundary dispute is based largely on papers published in the Department of State's *Foreign Relations of the United States*.

Hélio Silva praises Vargas and Aranha and observes: "We Brazilians, even those who . . . participated in the events, like this old reporter, did not have an idea of the greatness of Brazil's position and the stature of some of our statesmen."

JOHN W. F. DULLES  
*University of Arizona*

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## Communications

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*A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.*

*The following letters have been received in connection with the publication of Ray Allen Billington's article, "Tempest in Clío's Teapot: The American Historical Association Rebellion of 1915," AHR, 78 (1973): 348-69.*

TO THE EDITOR:

Ray Allen Billington's article makes several references in its first paragraph to recent discontent in the American Historical Association.

Billington puts this recent discontent in the past tense. He says it "*has been rejected* by the majority of the members" but "*has encouraged* long overdue reforms generally tending toward more democratic procedures" (italics added).

I had not been aware that the recent discontent was over. On the contrary. I should like

to ask Professor Billington to explain the factual basis for this assertion.

STAUGHTON LYND  
Chicago, Illinois

TO THE EDITOR:

Something might be added to the "Tempest in Clío's Teapot." Moses' version of the Ten Commandments in Deuteronomy 5: 6-21 and Exodus 20: 2-17 seems to differ from that of both Billington and J. Franklin Jameson (p. 369). "Thou shalt not steal" is number seven; "Thou shalt not bear false witness against thy neighbor" is number eight; "Thou shalt not covet thy neighbor's wife" is number nine.

RAYMOND J. JIRAN  
Thomas Nelson Community College

PROFESSOR BILLINGTON REPLIES:

Although I am not privy to the inner circles of the American Historical Association, I have the distinct impression that the "discontent" to which Staughton Lynd refers reached a crest in the late 1960s and has steadily declined since that time. Personal opinion can never be exactly measured, but one bit of evidence can be advanced: the handful of the faithful who attended the 1972 business meeting of the association in contrast with the multitudes who swarmed to attack or defend the "establishment" a few years before. Whether I am right or wrong concerning the degree of satisfaction of the members, I share Dr. Lynd's hope that discontent does remain and that it will continue to motivate the reforms through which administrative processes are democratized.

The question raised by Professor Jiran con-

cerning the numbering of the Ten Commandments was posed by others who read earlier drafts of the article, forcing me into an unaccustomed excursion into the mysteries of Biblical scholarship. This venture took me no farther than the *Encyclopedia Britannica*, the *Encyclopedia Americana*, and the *Catholic Encyclopedia*, but this was enough to satisfy me that the order and numbering of the commandments varies amazingly. "There is," says the writer in the *Britannica*, "no agreement concerning the enumeration of the ten commands." He goes on to say that Jewish tradition makes the prologue, "I am the Lord your God," the first command and the prohibition against other gods and images the second; medieval practice and Luther lumped these into one command; Greek Orthodox and Protestant Reformed traditions treat the prologue and prohibition of other gods as the first and the prohibition of images as the second. The *Catholic Encyclopedia* describes at some length the reasons for the division between Catholic and Protestant numbering.

I found what seemed to be the most useful ordering of the commandments in the *Americana* (1964 ed., vol. 8, p. 553), which lists them, "shorn of their elaborate commentaries," with "Thou shalt not steal" as number eight and "Thou shalt not bear false witness against thy neighbor" as number nine.

Others more learned than I may quarrel with my wording, but they will have to take on the authorities I have cited. I bow out of the battle.

RAY ALLEN BILLINGTON  
*Huntington Library*

#### TO THE EDITOR:

Carl E. Schorske's "Politics and Patricide in Freud's *Interpretation of Dreams*" (*AHR*, 78 [1973]: 328-47) argues that Freud's interpretation of his "Revolutionary Dream" of 1898, which referred to the prince as "the father of his country" from whose "power the other social authorities have developed" in history, foreshadowed Freud's "mature political theory." Its "central principle" was "that all politics is reducible to the primal conflict between father and son." Schorske states, in effect, that psychoanalysis was Freud's "revenge on politics," in that "patricide replaces regicide: psy-

choanalysis overcomes history," and "politics is neutralized by a counterpolitical psychology." Schorske's footnote 40, apparently meant to support this thesis, states: "This theory [i.e., "all politics is reducible to the primal conflict between father and son"] was set forth in Freud, *Totem and Taboo* (1913)." The expression "all politics," however, artificially broadens the scope of Freud's comment on government and law. This was, in any case, a secondary motif in *Totem and Taboo*. Its primary objective was to link the repressed patricidal (oedipal) wish of every boy and the killing of God the Father, "as in the Christian myth," to the totem meal, "perhaps mankind's earliest festival. . . a repetition and a commemoration of [that] memorable and criminal deed [the murder-cannibalism of the tribal sons of their father, possessor of the females] which was the beginning of so many things—of social organization, of moral restrictions and of religion." The mention of religion last should deceive no one. The dramatic message of *Totem and Taboo* was: The crucifixion is a symbol of repressed infantile sexuality.

Psychoanalysis, said Freud, merely confirmed "the habitual pronouncement of the pious: we are all miserable sinners." But the analyst (unlike the priest, who was able only to reinforce the mechanism of repression) could relieve the religion-implemented neurosis by displacing the ambivalent attitude toward the father onto a substitute. But such displacement could not resolve the original ambivalence.

Thus Freud's discovery of psychoanalysis (his self-discovery, too) could not, as Schorske writes, have enabled Freud to "overcome his Rome neurosis," nor could it have reduced his "own *political* [italics added] past and present to an epiphenomenal status in relation to the primal conflict between father and son, giving his fellow liberals an ahistorical theory of man." It was his feeling about religion that Freud reduced to epiphenomenal status.

Freud's Rome neurosis was one of many conflicts that lasted throughout his life. "My emotional life has always insisted that I should have an intimate friend and a hated enemy come together in a single individual," he wrote in *The Interpretation of Dreams*. And he passionately attacked religion—the factor that may well have barred his career in Austrian law

and politics—in many of his medical writings and, of course, in *Totem and Taboo*, *The Future of an Illusion*, *Civilization and Its Discontents*, *Leonardo da Vinci*, and, last but not least—the year before he died—in *Moses and Monotheism*, which extended to the Jews the crime of Father-God murder of the Egyptian prince Moses, who had saddled the Hebrews with his own religion. Thereby he had reduced the entire Judaeo-Christian teaching to “a neurosis of mankind” whose “grandiose powers” were explainable “in the same way as we

should a neurotic obsession in our patients.” Freud’s “ahistorical theory of man” was a definition of monotheism as a societal disorder, which, if abolished by psychoanalysis, would allow the irreligious Freud to enjoy the heritage of Judaism, unmarred by its obsessional forms, and to enjoy Rome (that is, Vienna, capital of the Holy Roman Habsburgs) uncluttered by Catholicism.

STANLEY W. PAGE  
City College,  
City University of New York

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## Recent Deaths

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EDGAR A. J. JOHNSON, professor emeritus of economic history at the School of Advanced International Studies of the Johns Hopkins University, died from multiple myeloma in Washington, D.C., on August 17, 1972, at the age of seventy-two.

Johnson came from a Swedish immigrant family, which had settled on a farm in Illinois. He received his education at the Orion, Illinois, high school, at the University of Illinois, and at Harvard. His career was varied and peripatetic. His primary teaching posts were successively at the University of Oklahoma, Cornell University, New York University, and the Johns Hopkins University, but in addition he was visiting professor at a number of other institutions.

During World War II, he served as chief of the economic branch of the Allied land forces in Norway and as deputy chief of U.S. supply control in Germany. After the war he was a civil administrator in Korea, economic adviser to the ECA mission to Greece, deputy chief of the United States aid mission to Yugoslavia, and more recently was consultant to the Indian National Council of Applied Economic Research.

In addition to his teaching and public service, Johnson found time to produce a long list of publications, the most important of which were: *American Economic Thought in the Seventeenth Century* (1932); *Predecessors of Adam Smith* (1937); (with Hermann Krooss) *The Origins and Development of the American Economy* (1953); *Market Towns and Spatial Development of India* (1965); *Organizing Space in Developing Countries* (1970); and his extremely interesting autobiography with the unlikely title of *American Imperialism in the*

*Image of Peter Gynt—Memoirs of a Professor-Bureaucrat* (1971). Two posthumous works, *Foundations of American Freedom* and *Spatial Aspects of Economic Development*, are scheduled for publication.

Johnson was a staunch advocate of a closer intellectual relationship between historians and economists, which accounts, in part, for his role in founding the Economic History Association, of which he was president from 1960 to 1962. As the first editor of the *Journal of Economic History*, established in 1939, he displayed high standards of scholarship and composition. Our personal friendship came from our collaboration on the *Journal*, for I was associate editor and treasurer of the association.

Johnson received many honors for his great variety of activities. He was made a member of the Order of the British Empire (1945); he received the King Haakon VII Cross of Freedom (1945); and he was given an LL.D. by the Johns Hopkins University (1972).

Johnson is survived by his widow Virginia Gravel Johnson and by his son, Edgar A. J. Johnson, Jr.

SHEPARD B. CLOUGH  
*Columbia University*

DOUGLAS R. LACEY, chairman of the department of history at the United States Naval Academy, died July 27, 1973. He was sixty years old. Dr. Lacey attended Illinois College and Rutgers and Columbia Universities. He received his Ph.D. from Columbia in 1959. Joining the faculty at the academy in 1941, he was named chairman of the history department in September 1970. In 1971-72 Professor Lacey was a senior research fellow at the Folger Shakespeare Library, and he spent 1946-47 in Lon-

don as a Rockefeller fellow in the humanities. He had previously taught at Rutgers, the City College of New York, Sarah Lawrence College, the University of Alberta, and the University of Maryland.

HENRY LITHGOW ROBERTS, who died October 17, 1972, at his home in Rochester, Vermont, served for nineteen years (1948-67) in the history department of Columbia University. In addition to holding a chair in the university (from 1954) he was also director of Columbia's Russian Institute from 1956 to 1962 and of the East Central Europe program (which grew into the Institute on East Central Europe) from 1954 to 1967. He was editor of the *Slavic Review* from 1965 to 1967. After leaving Columbia he held a chair in history at Dartmouth from 1967 until his death. Roberts was one of the leading pioneers of East European and Russian studies in America.

Roberts took his B.A. (1938) and Ph.D. (1942) at Yale, specializing in modern German history. War service in the navy and in the OSS widened and deepened his interest in Europe. He spent most of 1945 in Romania as a member of the American component of the Allied Control Commission. In the same year he married Deborah Calkins. While still at Yale he had won a Rhodes scholarship but had been unable to take it up owing to Britain's involvement in war in 1939. He and his wife decided to use this opportunity after the war to spend two years (1946-48) at Balliol, where he took an Oxford D.Phil., developing through systematic research the understanding of Romania that he had acquired during his official service. The doctoral dissertation was remolded into a book, *Rumania: Political Problems of an Agrarian State*, published in 1951. This, his largest published work, remains still the best survey of a subject that is not only interesting in itself but also can be illuminating to all who seek to understand the interplay of political and social forces, of ideas and action in "developing societies." The author's sympathy for the Romanians is clear on every page, as is his awareness of the richly comic and tragic features of their history. He neither took them at their own valuation nor fell for the persuasive arguments of their many enemies, but rather he saw them as they were and how they had grown into

what they were, critically and compassionately.

During his Columbia years Roberts also did much work for the Council on Foreign Relations. For eighteen years he edited the book review section of *Foreign Affairs*. He was also part-author of *Britain and the United States: Problems in Cooperation* (1953) and sole author of *Russia and America: Dangers and Prospects* (1956), both based on study groups sponsored by the Council. In both books Robert's own hand is clearly visible; and of their quality it may perhaps suffice to say that, after all that has happened in the last twenty years in all three countries, they still bear reading. The questions he put may not have been answered, but they are the right questions. During these years he also wrote numerous articles, mainly on aspects of modern Russian history and politics, toward which, under the influence of G. T. Robinson and Philip Mosely, his interest increasingly turned. Some of the best of these were put together in a volume entitled *Eastern Europe: Politics, Revolution and Diplomacy*, published in 1972.

Roberts was outstanding as a historian, a teacher, and an organizer. It is possibly in this last field (broadly understood) that his achievement was greatest—though he would perhaps have been surprised at the thought. He was indefatigable, both in promoting the study of Eastern Europe and Russia in the United States and in helping students and colleagues, both his contemporaries and his juniors. He showed infinite patience, vigilance, and wisdom in the long hours of committee and office work. To a large extent the scholarly achievements of others, whose burdens he assumed, form part of his own achievement. When he left the turmoil of New York for his beautiful Vermont valley he had earned his rest; but the change meant no end to intellectual effort, as his colleagues at Dartmouth and his, sadly, uncompleted work on the Russian Revolution of 1917 bear witness.

Roberts succeeded, as well as any one can do, in seeing the conflicts of twentieth-century history both as an American and as a citizen of the world. The moralizing denunciations of the early 1950s were as alien to him as the moralizing self-flagellations of the late 1960s. This does not mean that he was unaware of the moral dimension in world politics: it was not his style to run away from it behind a smoke screen of

phrases about value-free judgments. He was second to none in scrupulous exploration and verification of historical evidence, but he did not seek in professional devotion an excuse to opt out of the age in which he lived. He never succumbed to the temptation either to lavish unwanted advice on other nations or to bow uncritically before the culture of Europe. From the first attitude he was saved by his moral sensitivity, from the second by his sense of humor.

It has not been easy to write an obituary of my first and best graduate student, who was also

my first and best American friend. But the loss is more than personal; and many others who knew him little or not at all will remain deeply indebted to his efforts and have good cause to be thankful for his life.

HUGH SETON-WATSON  
*University of London*

Others members of the association who have died recently are Joseph M. Carrière of the University of Virginia in Charlottesville, Virginia, and Peter M. Isajiw of Philadelphia, Pennsylvania.

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## Association Notes

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Mrs. Eleanor F. Straub has been appointed assistant executive secretary of the AHA, succeeding Mr. John J. Rumbarger, currently editor of *Prologue: The Journal of the National Archives*. Mrs. Straub, whose appointment began in September of this year, expects to be awarded the Ph.D. from Emory University by the end of this year.

Mr. Edward C. Papenfuse, Jr. resigned his post as associate editor-bibliographer of the *AHR* to become assistant archivist of the State of Maryland. Mr. James Dougherty, formerly a lecturer at University College, University of Maryland, has joined the staff of the *AHR* as assistant editor-bibliographer. In addition to his other responsibilities, Mr. Dougherty will be the director of the new *Writings on American History* project, which has been made possible by funding from the National Endowment for the Humanities.

Mrs. Janet Hayman, who has become editor of the *AHA Newsletter* on a full-time basis, is leaving the staff of the *AHR*, where she has been an assistant editor. Mr. Randall Koladis has been appointed an assistant editor.



## Other Books Received

Books listed were received by the *AHR* between May 1 and July 15, 1973. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

### GENERAL

ALBRECHT-CARRIÉ, RENÉ. *A Diplomatic History of Europe since the Congress of Vienna*. Rev. ed.; New York: Harper and Row. 1973. Pp. xix, 764. \$6.95.

ARON, R., et al. *Politica di potenza e imperialismo: L'analisi dell'imperialismo alla luce della dottrina della ragion di Stato*. Ed. by SERGIO PISTONE. Società e politica, 1. [Milan:] Angeli Editore. 1973. Pp. 404. L. 3,500.

ASHLEY, MAURICE. *A History of Europe, 1648-1815*. Englewood Cliffs, N.J.: Prentice-Hall. 1973. Pp. ix, 295. Cloth \$8.95, paper \$4.95.

BEN-AMITTAY, JACOB. *The History of Political Thought: From Ancient to Present Times*. New York: Philosophical Library. 1973. Pp. xv, 318. \$20.00.

*Bibliography of the History of Medicine*. No. 6, 1970. DHEW Publication no. (NIH) 72-315. Bethesda, Md.: U.S. Department of Health, Education, and Welfare, Public Health Service, National Institutes of Health, National Library of Medicine. n.d. Pp. vi, 295. \$3.70 postpaid.

BLUMENSON, MARTIN. *Bloody River: The Real Tragedy of the Rapido*. Boston: Houghton Mifflin. 1970. Pp. viii, 150. \$4.95.

BOUWSMA, WILLIAM J. *The Culture of Renaissance Humanism*. AHA Pamphlets 401. Washington: American Historical Association. 1973. Pp. 40. \$1.00.

BREISACH, ERNST. *Renaissance Europe, 1300-1517*. New York: Macmillan. 1973. Pp. xxiii, 456. \$10.95.

BRINGLE, MARY. *Eskimos*. First Book. New York: Franklin Watts. 1973. Pp. 87. \$3.95. Grades 4-7.

CHAGNIOT, JEAN. *Les temps modernes, 1661-1789*. Le fil des temps, 6. Paris: Presses Universitaires de France. 1973. Pp. 311.

CHENG, RONALD, YE-LIN (ed.). *The Sociology of Revolution: Readings on Political Upheaval and Popular Unrest*. Chicago: Henry Regnery. 1973. Pp. xxviii, 334. \$12.50.

CORTADA, JAMES W. *United States-Spanish Relations, Wolfram and World War II*. Barcelona: Manuel Pareja; distrib. by International Scholarly Book Services, Portland, Ore. 1971. Pp. 134. \$6.00.

ELMANDJRA, MAHDI. *The United Nations System: An Analysis*. [Hamden, Conn.]: Archon Books. 1973. Pp. 368. \$7.50.

EUBANK, KEITH (ed.). *The Road to World War II: A Documentary History*. New York: Thomas Y. Crowell. 1973. Pp. xiii, 284. \$2.95.

FINOCCHIARO, MAURICE A. *History of Science as Explanation*. Detroit: Wayne State University Press. 1973. Pp. 286. \$15.95.

FISCHEL, WALTER J. (ed. with an introd. and notes). *Unknown Jews in Unknown Lands: The Travels of Rabbi David d'Beth Hillel (1824-1832)*. New York: Ktav Publishing House. 1973. Pp. 130. \$10.00.

FISHMAN, JOSHUA A. *Language and Nationalism: Two Integrative Essays*. Rowley, Mass.: Newbury House. 1973. Pp. xvi, 184. \$8.95.

GAY, PETER, and WEBB, R. K. *Modern Europe to 1815; Modern Europe since 1815*. 2 vols. New York: Harper and Row. 1973. Pp. xiv, 536, xxiv; xvi, 537-1122, xxxiii. \$6.95 each.

GEORGE, ALEXANDER L., et al. *The Limits of Coercive Diplomacy: Laos, Cuba, Vietnam*. Boston: Little, Brown. 1971. Pp. xviii, 268. \$3.50.

HAVILAND, VIRGINIA. *Children and Literature: Views and Reviews*. Glenview, Ill.: Scott, Foresman. 1973. Pp. 461.

HEIMANN, SUSAN. *Christopher Columbus: A Visual Biography*. New York: Franklin Watts. 1973. Pp. 57. \$4.50. Grades 4-5.

HIRSCHFELD, GERHARD. *The People: Growth and Survival. First Cycle*. Foreword by KENNETH E. BOULDING. Chicago: Aldine Publishing Co., for the Council for the Study of Mankind. 1973. Pp. xxi, 239. \$7.50.

HOLLANDER, A. N. J. DEN (ed.). *Diverging Parallels: A Comparison of American and European Thought and Action*. The European Association for American Studies. Leiden: E. J. Brill. 1971. Pp. 222. 60 gls.

HOLLANDER, PAUL. *Soviet and American Society: A Comparison*. New York: Oxford University Press. 1973. Pp. xx, 476. \$12.50.

- HOLSTI, OLE R., et al. *Unity and Disintegration in International Alliances: Comparative Studies*. Comparative Studies in Behavioral Science. A Wiley-Interscience Publication. New York: John Wiley and Sons. 1973. Pp. xv, 293. \$12.95.
- HUGHES, MICHAEL (ed.). *The Letters of Lewis Mumford and Frederic J. Osborn: A Transatlantic Dialogue, 1938-70*. New York: Praeger. 1972. Pp. x, 493. \$15.00.
- ISSAWI, CHARLES. *Issawi's Laws of Social Motion*. New York: Hawthorn Books. 1973. Pp. 185. \$5.95.
- JOSHUA, WYNFRED. *Nuclear Weapons and the Atlantic Alliance*. Strategy Papers no. 18. New York: National Strategy Information Center. 1973. Pp. vi, 60. \$1.00.
- KATZNELSON, IRA. *Black Men, White Cities: Race, Politics, and Migration in the United States, 1900-30, and Britain, 1948-68*. New York: Oxford University Press, for the Institute of Race Relations, London. 1973. Pp. xii, 219. \$9.95.
- LE ROY LADURIE, EMMANUEL. *Le territoire de l'histoire*. [Paris:] Gallimard. 1973. Pp. 542.
- LUKACS, JOHN. *The Passing of the Modern Age*. New York: Harper and Row. 1970. Pp. ix, 222. \$7.95.
- MCMAMARA, ROBERT S. *One Hundred Countries, Two Billion People: The Dimensions of Development*. London: Pall Mall Press; distrib. by Praeger, New York. 1973. Pp. 140. \$5.95.
- MLECKA, LOUIS F. (comp.). *Famous People: Historical, Biographical Book of Birthdays*. Brooksville, Fla.: the author. 1973. Pp. unnumbered. \$1.95.
- NUGENT, WALTER T. K. *Creative History*. 2d ed.; Philadelphia: J. B. Lippincott. 1973. Pp. 178. \$2.45.
- OHL, INGO. *Die Levante und Indien in der Verkehrspolitik Venedigs der Engländer und der Holländer, 1580-1623*. Beiträge zur Sozial- und Wirtschaftsgeschichte, no. 2. Kiel: Kommissionsverlag Walter G. Muhlau. 1972. Pp. 145. DM 10.
- PENLINGTON, NORMAN. *The Alaska Boundary Dispute: A Critical Reappraisal*. The Frontenac Library, no. 5. Toronto: McGraw-Hill Ryerson. 1972. Pp. ix, 141. \$3.25.
- PENNINGTON, M. BASIL, OCSO (ed.). *Contemplative Community: An Interdisciplinary Symposium*. Cistercian Studies Ser.: no. 21. Washington: Cistercian Publications, Consortium Press. 1972. Pp. 366. \$15.95.
- PHILLIPS, DEREK L. *Abandoning Method: Sociological Studies in Methodology*. San Francisco: Jossey-Bass. 1973. Pp. xvi, 202. \$8.75.
- PRICHARD, JAMES COWLES. *Researches into the Physical History of Man*. Ed. and with an introd. by GEORGE W. STOCKING, JR. Classics in Anthropology. Chicago: University of Chicago Press. cxliv, iv, 568. \$14.50.
- REISNER, WILL (ed.). *Documents of the Fourth International: The Formative Years (1933-40)*. New York: Pathfinder Press. 1973. Pp. 448. Cloth \$10.00, paper \$3.45.
- RENO, EDWARD A., JR. (ed.). *League of Nations Documents, 1919-1946: A Descriptive Guide and Key to the Microfilm Collection*. Vol. 1, *Subject Categories IA through IV*. New Haven, Conn.: Research Publications. 1973. Pp. xxi, 282. \$200.00.
- ROBIN, RÉGINE. *Histoire et linguistique*. Linguistique. Paris: Armand Colin. 1973. Pp. 306. 34 fr.
- SCHNEIDER, LOUIS, and BONJEAN, CHARLES M. (eds.). *The Idea of Culture in the Social Sciences*. New York: Cambridge University Press. 1973. Pp. ix, 149. Cloth \$9.95, paper \$2.95.
- STAGG, J. M. *Forecast for Overlord: June 6, 1944*. New York: W. W. Norton. 1971. Pp. 128. \$5.95.
- SUTTER, RUTH E. *The Next Place You Come to: A Historical Introduction to Communities in North America*. Englewood Cliffs, N.J.: Prentice-Hall. 1973. Pp. vii, 214. Cloth \$7.95, paper \$3.50.
- SWARTZBAUGH, RICHARD GREY. *The Mediator: His Strategy for Power*. Cape Canaveral, Fla.: Howard Allen. 1973. Pp. viii, 133. \$4.95.
- TEMIN, PETER (ed.). *New Economic History: Selected Readings*. Penguin Modern Economics Readings. [Baltimore:] Penguin Books. 1973. Pp. 445. \$4.95.
- THOMAS, BRINLEY. *Migration and Economic Growth: A Study of Great Britain and the Atlantic Economy*. 2d ed.; New York: Cambridge University Press. 1973. Pp. xxxi, 498. \$23.50. See rev. of 1st ed. (1954), *AHR*, 60 (1954-55): 340.
- WACE, NIGEL, and LOVETT, BESSIE. *Yankee Maritime Activities and the Early History of Australia*. With a foreword by LADY HASLUCK. Research School of Pacific Studies, Aids to Research Ser., no. A/2. Canberra: Australian National University Press; distrib. by International Scholarly Book Services, Portland, Ore. 1973. Pp. xiv, 131. \$6.75.
- WELSH, WILLIAM A. *Studying Politics*. Basic Concepts in Political Science. New York: Praeger. 1973. Pp. xii, 260. Cloth \$8.50, paper \$3.95.
- Western Civilization: Recent Interpretations*. Vol. 1, *From Earliest Times to 1715*, ed. by CHARLES D. HAMILTON; vol. 2, *From 1715 to the Present*, ed. by C. STEWART DOTY. New York: Thomas Y. Crowell. 1973. Pp. viii, 599; xi, 625. \$5.95 each.
- WILLERT, SIR ARTHUR. *Washington and Other Memories*. Boston: Houghton Mifflin. 1972. Pp. vi, 248. \$6.95.
- WITTLIN, ALMA S. *Museums: In Search of a Usable Future*. Cambridge, Mass.: MIT Press. 1970. Pp. xvii, 300. \$15.00.
- WRIGHT, GORDON, and MEJIA, ARTHUR, JR. (eds.). *An Age of Controversy: Discussion Problems in Twentieth Century European History*. Alternate ed.; New York: Dodd, Mead. 1973. Pp. ix, 389. \$5.95.

## ANCIENT

- FINLEY, M. I. (ed.). *Third International Conference of Economic History, Munich, 1965*. Vol. 3, *The Ancient Empires and the Economy*. École Pratique des Hautes Études—Sorbonne. VI<sup>e</sup> Section: Sciences économiques et sociales. Congrès et colloques, 10. Paris: Mouton. 1969. Pp. 110. 22 fr.

PERLMAN, S. (ed. and introd.). *Philip and Athens. Views and Controversies about Classical Antiquity.* New York: Barnes and Noble. 1973. Pp. xiii, 222. \$10.00.

SCULLARD, H. H. *Roman Politics, 220-150 B.C.* 2d ed.; New York: Oxford University Press. 1973. Pp. xxxiii, 325. \$17.75. See rev. of 1st ed. (1951), *AHR*, 57 (1951-52): 413.

#### MEDIEVAL

BROWN, R. ALLEN. *The Origins of Modern Europe: The Medieval Heritage of Western Civilization.* New York: Thomas Y. Crowell. 1973. Pp. 252. \$2.95.

CANTOR, NORMAN F. *The Meaning of the Middle Ages: A Sociological and Cultural History.* Boston: Allyn and Bacon. 1973. Pp. viii, 321. \$5.25.

HIMLY, FRANÇOIS J. *Atlas des villes médiévales d'Alsace.* Publications de la Fédération des Sociétés d'Histoire et d'Archéologie d'Alsace, no. 6. [Strasbourg: the Fédération.] 1970. Pp. 133.

MARTIN, G. H. (ed.). *The Ipswich Recognizance Rolls, 1294-1327: A Calendar.* Suffolk Records Society, vol. 16. [Ipswich:] the Society. 1973. Pp. 151. \$6.00.

PORFIROGENETUL, CONSTANTIN. *Carte de învățătură pentru fiul său Romanos* [The Book of Counsel for His Son Romanos]. Tr. by VASILE GRECU. Scriptores Byzantini, 7. Bucharest: Editura Academiei Republicii Socialiste România. 1971. Pp. 122. Lei 6.50.

VERGER, JACQUES. *Les universités au Moyen Âge.* L'historien, 14. Collection SUP. Paris: Presses Universitaires de France. 1973. Pp. 214.

#### BRITISH COMMONWEALTH AND IRELAND

BEAULIEU, ANDRÉ, and HAMELIN, JEAN. *La presse québécoise: Des origines à nos jours.* Vol. 1, 1764-1859. Quebec: Presses de l'Université Laval. 1973. Pp. xi, 268. \$7.00.

BERGONZI, BERNARD. *The Turn of a Century: Essays on Victorian and Modern English Literature.* New York: Barnes and Noble. 1973. Pp. x, 222. \$13.50.

BOARDMAN, ROBERT, and GROOM, A. J. R. (eds.). *The Management of Britain's External Relations.* New York: Barnes and Noble. 1973. Pp. x, 362. \$16.50.

CADBURY, HENRY J. *John Woolman in England: A Documentary Supplement.* Supplement no. 32 to the *Journal of the Friends Historical Society*. [London:] the Society; distrib. by Friends Book Store, Philadelphia. 1971. Pp. 142. \$4.00.

CHAPMAN, S. D., prepared for the Economic History Society by. *The Cotton Industry in the Industrial Revolution.* Studies in Economic History. [London:] Macmillan; distrib. by Fernhill House, New York. 1972. Pp. 79. \$2.00.

EVANS, E. ESTYN. *The Personality of Ireland: Habitat, Heritage and History.* The Wiles Lectures given at the Queen's University Belfast. New York: Cambridge University Press. 1973. Pp. viii, 123. \$9.00.

HALL, DOUGLAS. *A Brief History of the West India Committee.* Caribbean History Pamphlets. [St. Lawrence, Barbados:] Caribbean Universities Press; distrib. by Ginn, London. 1971. Pp. ix, 60. 90p.

HALL, DOUGLAS. *Five of the Leewards, 1834-1870: The Major Problems of the Post-Emancipation Period in Antigua, Barbuda, Montserrat, Nevis and St Kitts.* Caribbean History Monographs. [St. Lawrence, Barbados:] Caribbean University Press; distrib. by Ginn, London. 1971. Pp. x, 210. £1.86.

HOBHOUSE, L. T. *Democracy and Reaction.* Ed. with introd. and notes by P. F. CLARKE. New York: Barnes and Noble. 1973. Pp. xxxii, vii, 280, xxxiii-xi. \$14.50.

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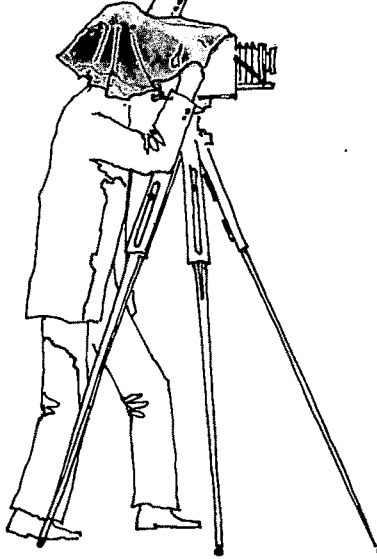


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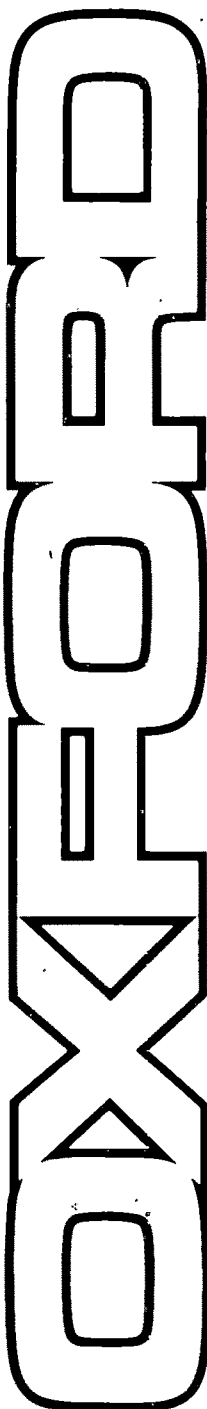
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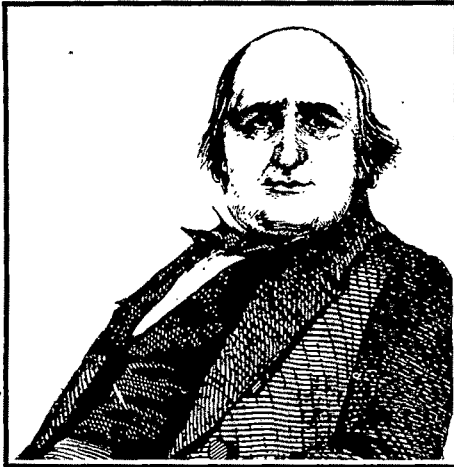
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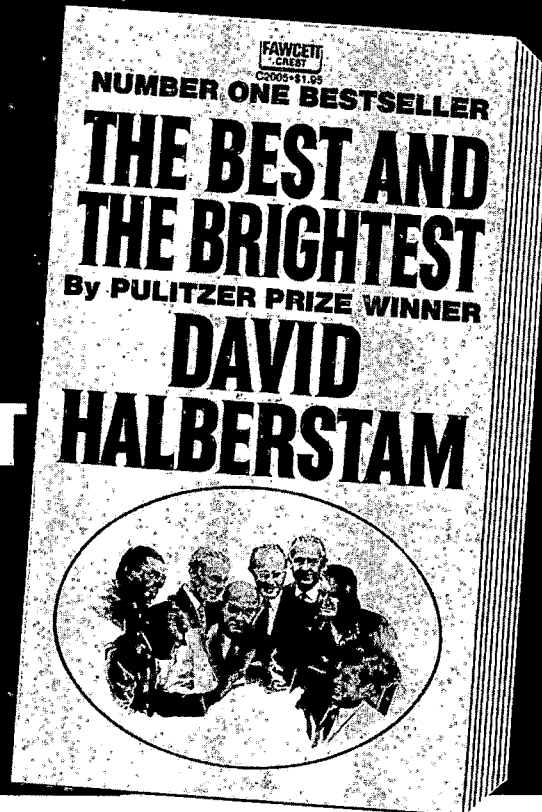
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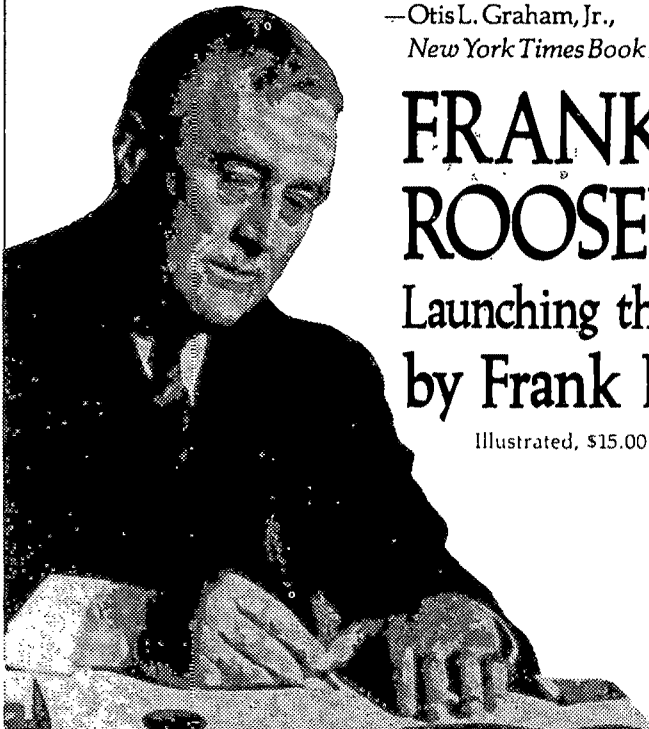
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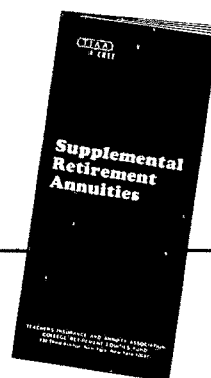
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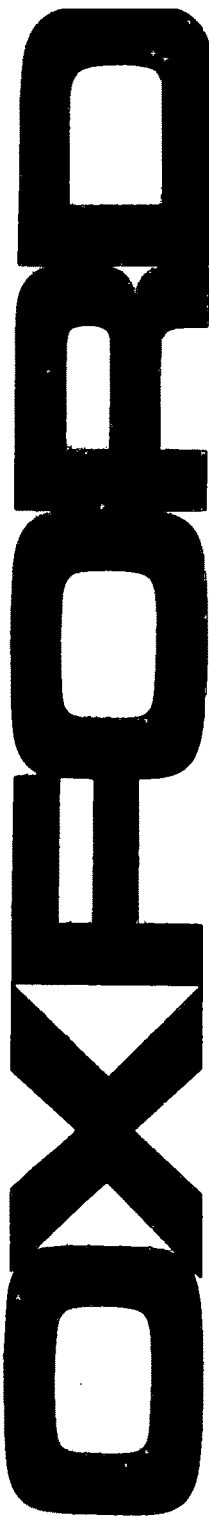
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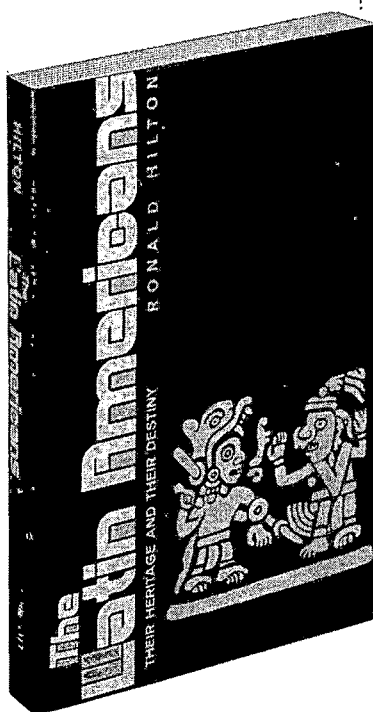
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